Day Two: September 12, 1996

Session Four: Questions and Answers

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

Q (Ron Sloan, NYS Department of Environmental Conservation): One of the things that Bob brought up, which is very interesting from our standpoint as researchers in a regulatory agency, is the possibility of regulating on the basis of sex in fish. This would be very difficult to do, but it raises a very valid point in terms of male and female differences, not only for accumulation and behavior in the estuary, but also in other systems and for other species of fish. For a number of years we have noted tremendous sex differences, with the males always being higher in concentration than the females for striped bass, American shad, white perch, the Pacific salmon in Lake Ontario, and a number of other species. Bob, is there as much work going on with you and other researchers as it relates to sex differences?

Robert Thomann:

I am not doing anything myself, but I have been following the literature rather closely. There is a lot more information now. I think John has done some work on seals. Recently, the Dutch did some work on exposing males and females and looking at bioaccumulation in the laboratory, which showed significant differences. But I have a question back to you. Did I understand your opening comment to mean that you really are thinking about regulating by sex?

Q (Ron Sloan): No, no. There is no real way to be able to do that, but it is very bothersome that we do not have any good explanation for why the males are so much higher in concentration, even controlling for gamete production and the lipid consequences associated with voiding of egg material from the females. The sex differences are still there and they are very dramatic.

Robert Thomann:

Well, I think it is because they are staying in the estuary.

Q (Ron Sloan): Maybe for striped bass, but even if you look at the males in the lower part of the estuary that are freshly migrating in from the ocean, the males will tend to be higher in concentration. That still does not explain the sex differences that we see very dramatically in Pacific salmon and white perch. One of the reasons I raised the point is because we have been looking the past few days now at the uncertainties associated with BAFs and BSAFs, and I think a lot of the uncertainty and the variability is associated with sex differences when we start looking at fish.

Robert Thomann:

Well, I think that is true. The sex differences, at least for this case, do contribute significantly to the variation. If you calculated BAFs here, they would be all over the lot. As John pointed out, part of it is the disequilibrium between the fish and the water. This would be the same for BSAFs. They would be very time variable and very spatially variable.

Q (Ron Sloan): We have looked at that to some extent, and we cannot really account for it because the lipid concentrations are so low. Gamete loss cannot explain the differences.

John Connolly:

We have looked at a lot of data in a more generic sense and it seems to be a mixed bag. We do see a difference for some species, but we do not for other species. You particularily see it in aquatic mammals. Milk production and the loss due to that explains the differences for the aquatic mammals. But I do not think we understand the reason for those differences in fish.

Q (Ron Sloan): My primary reason for bringing this up as a question or an issue is that, if we are interested in explaining a lot of the variability that we are observing, we need to take into account the sex differences.

Q (Nelson Thomas, U.S. EPA, Office of Research and Development): This is a question for John and Bob. In Frank's presentation, he alluded to an uncertainty factor

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of 3 at the 95 percent confidence level. Did you look at your 95 percent confidence level in your calculations and figure out how far off you would be like Frank did?

John Connolly:

The one that I showed, which was for Dover sole, was about a factor of 2. In a couple of other calculations, where we have done uncertainty factors, the factor of 2 to 3 is pretty robust. That is what we are generally seeing in these calculations.

Robert Thomann:

That is true. If you look closely at those percent exceedance frequencies, you will see about the same kind of variability.

Larry Burkhard:

John, I have a slide of the probability distribution for the 2-year old flounders from your New Bedford harbor model. This figure is from a publication in the SETAC Journal. It illustrates the uncertainty that they found using a Monte Carlo simulation for his model. You can see here that a factor of 2 to 3 is right on line. I just wanted to point out the kind of uncertainties that you have with these models.

Robert Thomann:

I interpreted Nelson's question as asking what our uncertainty was in the 95th percentile estimate, or in other words, in the tail of the distribution. We have the ability to project or analyze for the 95th percentile to a factor of about 2 or 3.

Q (Jay Field, NOAA): In situations where we have vertical gradients in terms of sediment concentrations, has anybody looked into the implications of biological activity, which may vary seasonally or by location, and the impact it might have on some of the models? How would any effects that we see affect the way we collect information?

John Connolly:

With some of the work I have done, I have seen more location differences. I have not paid a lot of attention to the seasonal differences that might exist. So, I cannot comment on seasonal differences within a system. But clearly, we see differences from system to system, depending to a large extent on the type of organisms comprising the benthic infauna. In determining whether or not we are worried about the top centimeter or the top 5 centimeters as being biologically available material, that is more of an issue for higher level organisms. This is something that again introduces considerable uncertainty, both in models and into the BAF and BSAF. That was one of the points I was trying to make with the Fox River. The model results suggest that the carp are seeing material recently deposited in the river, as opposed to material that is buried a few centimeters down. If carp were exposed to material a few centimeters down, they should have been at much higher concentrations than they were.

Robert Thomann:

The only calculation I have done in that area was associated with the cadmium and the blue crab in Foundry Cove, where there was a considerable amount of sediment mixing and sediment bioturbation. In that particular case, bringing up deep sediment to the surface and then letting that get into the food chain, actually retarded or slowed down the recovery of the cove. Recovery was delayed because you are just accessing higher concentrations over a longer period of time. It is very interesting. It increases the flux to the water column, so you are actually depurating out the sediment with a higher flux. But, because you are also reaching down into higher concentrations, you also retard the response.

Q (Ken Finkelstein, NOAA): Dr. Thomann, you presented a figure early in your talk that showed the concentrations in the striped bass with a no-action or as is. You also showed on the same figure one that would indicate that there was no change at all, even with some kind of decrease in the load. Actually, I think you decreased it down to zero, or in other words, it was remediation. I was wondering if you could shed some light on that because it puts some question in remediation plans, both there and elsewhere. I have seen this at other sites with models where the models do not show any kind of significant change with drastic cleanup operations.

Robert Thomann:

What you are focusing on is the calculation that was done in 1987 and 1988. There actually is a difference between remediating the upstream sediment and the no-action. The difference is only in the sense that it takes you a little bit longer to get to a certain percent frequency below 2, if you do not do anything. But eventually they come together. The reason for that is we have projected in the calculation, which I showed in the load projection, that the upstream load will continue to decline whether you do something or not in a no-action case. So, that was that load projection going down. That is what we did in 1987, and that is what we are still doing today. That is being revised, but we are still in the process of doing that. That load is declining, but there are also downstream loads, including loads in the metropolitan area, nonpoint source loads, and atmospheric loads. That calculation essentially said that, after a period of time, the relative contribution from the downstream sources became more dominant, and the upstream source became less dominant. So, its impact became less and less as time went on, and remediating it had less and less of an impact.

Q (Ken Finkelstein): Maybe I can also relate a little bit of a different experience that we have had in doing similar types of calculations. Consider the case where the *remediation is remote to where you are looking. For example, you are remediating in the upper Hudson River and looking at benefits in the lower Hudson River. In many cases, you do not see a big bang because that load is not controlling the concentrations and the exposure in the remote area. In the case of the lower Hudson, it may be because there is another load. In other cases, it may be that past historical discharges are controlling the system, rather than the current discharges coming from an upstream area. In these cases, eliminating the upstream source will not show an impact in the downstream area since that source was not contributing to contaminant levels in the fish downstream.*

Robert Thomann:

That says nothing about the advantage of remediating for the upriver area. There is a local benefit from that.

Q (Arnold Kuzmack, U.S. EPA, Office of Science and Technology): I would like to make a couple of comments of a somewhat philosophical nature. First, in terms of the discussions of the uncertainty being within a factor of 2 or 3, I think we have to be careful of how we use the terminology here, particularly when distinguishing between variability and uncertainty. Uncertainties tend to deal more with things that we do not know. For example, I am sure when you were doing your original models in the 1980s, you assumed that the fish were homogeneous in a migration pattern. You probably did not realize you were making that assumption. If you had listed your assumptions, that probably would not have appeared there, and it turned out to be wrong. That is an example of uncertainty. There are other uncertainties in just how well can we measure, what all we are including in the models, and so forth. The second comment I would like to make relates to John's discussion of the data versus modeling. I absolutely agree with your comment that whenever you use data you have some sort of implicit model. I would indicate caution on the part where you said that the models incorporate everything we know about the science. I think almost by definition they do not include everything. They include some sort of schematization and rationalization of our knowledge. And we sort of discard the things we do not understand that are in the data. I think because of that we have to be careful to do an appropriate degree of ground truthing of the models, which you all did in your research. I think one could criticize regulatory agencies in perhaps putting too much reliance on models that do not have that kind of ground truthing, particularly for situations like some permit process applications or decision-making based on model results that are not clearly understood. I think in these sorts of situations, you have to be pretty careful about having the models run away with things. I think that is what probably the data advocates are trying say.

Robert Thomann:

I think the other point, too, is deciding when a model is useful for regulatory or management purposes. Who makes the decision that models are ready for use in some kind of decision-making process? I have always felt that regulators and lawyers do not make that decision. They are not in a position to evaluate it. The people who ultimately make the determination of whether a model is suitable for management purposes or decision-making are members of this community. It is the scientific community that determines when the model can account for all the important factors and produce results that make sense relative to the data. This is the group that throws holy water on it.

Q (Dave Michaud, Wisconsin Electric Power Company): John, in looking at the carp data from the Fox River and Green Bay system that you presented, I am not surprised about what you saw. The carp population in the Fox River is a restricted population that lives in a highly regulated environment with a number of pools controlled by dams. You see greater variability in Green Bay because there is an intense depositional gradient along the east coast, and the carp can and do move freely between Green Bay and Lake Michigan. I would guess that you were probably sampling fish that were 10 to 15 years old, so they have a unique uptake history relative to the alewife. That leads me to my second comment. At one point in your presentation you grouped alewife and carp together in terms of trophic levels. I am not sure I understand the logic behind that. Except for an occasional amphipod or mysid, alewife feed primarily on plankton. The food habits work that we have done on carp in Lake Michigan suggests that it feeds almost exclusively on benthic organisms. We have found very little plankton in examining the gut contents. I would not call these the same trophic levels. How would you also factor in the obvious age differences in terms of exposure? Typically, alewife live only 3 years, while carp can survive for more than 25 years.

John Connolly:

The data that we had was for carp over a rather wide range of ages, so we were not looking specifically at carp that were 14 or 15 years old. When I was talking about the Fox River, I was referring to the lower Fox River below DePere Dam, where the carp are not confined in pools. But you just raised a lot of questions that I think are all valid. The point I was trying to make is that models allow you to explore those questions, as opposed to just using them to generate a number you can run around and extrapolate with.

Q (John Zambrano): This is a question for the panel from a regulator. Yesterday Philip Cook described the work that EPA did for the Great Lakes Initiative (GLI) for the purposes of establishing BAFs for setting water quality standards. What does the panel think of that work, particularly its applicability to the rest of the nation and the use of a disequilibrium constant of 25?

Frank Gobas:

My understanding of the process is that they used a bioaccumulation model for a particular system to derive bioaccumulation factors, food chain multipliers, and BSAFs. Then they applied these values to different systems, assuming that they are applicable for these other systems. What we try to do in our modeling is exactly the opposite. We are providing people with models that allow them to put in site-specific factors so that they can derive these values, the food chain multipliers, BSAFs, and BAFs, for various systems. We believe that not all systems are the same and that you cannot extrapolate from one system to another. So, my feeling is that the models can be used much more effectively when they are applied on a site-specific basis to work out these important bioaccumulation properties.

John Connolly:

I think you have to put the GLI work into context. What the GLI work was attempting to do was to develop bioaccumulation factors that could be applied to the Great Lakes. I do not think they had it in mind to use these factors for national criteria. So, I do not think that there is any intent to say that this approach is automatically applicable nationwide. I think that the GLI documents are very careful to point out a lot of the things that came out in these talks, in terms of the cautions in the use of this data. They are very clear in talking about site-specific application. They provided a generic number because it was their responsibility to do that. But they also provided all of the cautions associated with the application of those generic numbers.

Larry Burkhard:

I might add that in the GLI itself there is a tiering, starting with field data from the Great Lakes and ending with something that is totally model-derived from very generic kind of parameters from Lake Ontario. That is reflective of the knowledge that we had at the time that that was constructed and advanced.

Q (Mike Kravitz, U.S. EPA, Office of Science and Technology): This is sort of along the lines of the last question. What happens in terms of marine waters? What is the value of using things like BSAFs in systems that are so dynamic? Will we be able to do that?

Robert Thomann:

BAFs and the BSAFs have served us well and will continue to serve us well as kind of a first-order screening. There will always be a utility to BAFs and BSAFs combined with the kinds of calculations we have been showing. So, I do not think it is totally a question of discarding entirely the BAF and BSAF concept. That is not the point. The point is to be aware of the kinds of variability that can exist and the causes of that variability in those two numbers. In marine systems, I think ultimately we would get to the point of looking much more specifically at fully time-variable calculations, which, up to a point, are entirely doable today by just about anybody. Again, we must recognize all of the caveats and all of the assumptions that have been made. But I think we may be heading in the direction of working on those kinds of systems, supplemented by the BAF and BSAF concepts.

Q (Gayle Garman, NOAA): I have spent most of my career dealing with issues of whether a site should be remediated and to what degree it should be remediated, particularly sites with PCB contamination. So, I was particularly interested in Dr. Thomann's presentation and his projections for the Hudson River. I found it a little disturbing since it seems to imply that the PCBs in the Hudson River are going to essentially be assimilated. Am I going to end up doing calculations of assimilative capacity for PCBs? I think about that and also about some data we have for Puget Sound, which show that the salmon that return to Puget Sound actually have higher levels of PCBs in their tissue than the salmon that leave Puget Sound. So, I wonder again, if your models are very accurate for the little part of our system that you are modeling, but that they do not show us what happens outside of that system. I would like you to comment on that and what you think your models imply for decisions about remediation.

Robert Thomann:

Again, I think remediation has an impact locally. There is no doubt about that, since that has been shown any number of times. This was a question of remediating an upriver site and its impact on a migratory fish population. So that is the focus. One should not conclude from the results that remediation does not affect a population or the concentration of PCBs in a population. It does. But it depends on its relationship to the target organism. I do not mean to imply the question, why remediate? Why do the PCBs seem to be declining? Fundamentally, what happens in this calculation is the PCBs ultimately are being flushed out into the ocean and the exchange with the ocean is diluting it. There is also a continual burial of PCBs. If you look at the rate of decline of the striped bass over time and make an estimate of average burial rates over that period of time (and this is the crudest calculation), you find out that PCBs are continually being buried. Also, the assumption here is that the upstream load is declining. Why is that declining? It is declining for similar kinds of reasons. But remember, this is a long-term projection. There are two issues you need to be aware of. One is this calculation assumes that the loads to the system from point sources and nonpoint sources will continue to decline. That may not be the case. They may level off at some point. The second is that this calculation assumes when the fish goes out to the ocean, it sees zero PCBs. That is what we did in the old days. We are revisiting that part of the calculation, now that much better data are available for what is going out into the ocean. So we send these larger fish out 8 or 9 months of the year, and they see zero PCBs. If the concentrations out there are now higher than zero, which in fact they are, then those fish are going to come back at a different concentration than we had originally calculated.

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