Session Three: Questions and Answers

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

Q (Bob Barrick, PTI Environmental Services): Dom, I was going to just sit here and listen, but you brought up Fred Praul's work and it is such a central part of your talk that I really need to speak. As you know I work with Fred. His conclusions from his research are 100 percent opposite of your conclusions.

Dominic Di Toro:

Exactly right. You can conclude what you like.

Q (Bob Barrick): I want to make three points here. One is that when you are going ahead and using bulk organic carbon to normalize sediments, it misses the fact that organic carbon is disproportionately found in the different fractions. It is not a weighted average. Unless you quantify the organic carbon in each of those size and density fractions, you are going to come up with the wrong answer. Bulk organic carbon is not necessarily appropriate, even if organic carbon works. That is also the case with what Fred found, which was a disproportionate amount of PAH versus organic carbon in those fractions. Now, that is obscured a bit because Fred did present arithmetic plots in his thesis and you have converted those to log plots, which has dampened down the variability. If you take that out, it is a pretty big scatter in there.

Dominic Di Toro:

Bob, we have, as you know, organic carbon measurements. He made measurements in each of the size classes, so we know what the organic carbon concentration in each size class is. So, you divide by the organic carbon concentration in each size class.

Q (Bob Barrick): Dom, my point was that Fred did it properly. What I am saying is that when you are taking the organic carbon fractions and applying them out in the real world, where everybody has a TOC number, that

number is not according to a particular compartment within the sediment. It is a bulk sediment number, and to use that to measure against some kind of chemical that may be found only in one compartment is not what Fred found. If Fred found that, that may be an erroneous way to proceed.

Dominic Di Toro:

Let me just comment on the comment. If you actually take the bulk organic carbon concentration and take the chemistry per unit carbon, it also works for all the size fractions. That is because it is the same in each fraction. So, if you add up the same numbers, you get the same number.

Q (Bob Barrick): It works better if you put it on a log plot. The third thing I wanted to say was that in discussing all these things, it also ignores the difference in something else that Fred found in his research. This was the difference between labile and refractive particles, even though you may be able to extract organic compounds out of both of those using organic solvents in a laboratory. In fact, in nature where exposure is actually happening, there may be no exchange from the refracted particles and considerable exchange from labile particles. And to use the data to say that it supports equilibrium in the environment, when it is looking at an organic carbon-extracted system, is also making a little simplification. The only reason I really wanted to bring this up is because it is something that is now a part of your talk, and it is really counter to the conclusions of his dissertation work.

Dominic Di Toro:

I think the data analysis stands on its own. And you are right. Fred concluded exactly the opposite, which struck us as a little peculiar since the data seemed to indicate what I presented to you. There is a problem, by the way, with bioavailability. All organic carbon is not the same and all phases of sediments and chemicals are not the same. The issue is whether the difference is so large as to make organic carbon normalization useless, or are we arguing about the extent to which we can collapse responses

and understand what we are seeing? If the variability was so large as to be useless, then I would say fine, use dry weight normalization. In fact, why not use wet weight normalization in sediments? People have used it for biota. Why is it not a good idea in sediments? The point is you have to have some kind of database and theoretical understanding of what you are doing. The data speak for themselves.

Q (Dennis Leonard, Detroit Edison): Dr. Cook, could you comment briefly on the development of the BAF for mercury in the Great Lakes? The BAF model that you discussed required the steady-state assumptions to be present. The mercury concentrations that we have in the Great Lakes vary by about a factor of 100. Tributary littoral concentrations are influenced by high concentrations of mercury in rainfall. Open water concentrations are affected by transfer of mercury back to the atmosphere; therefore, we have concentrations ranging from about 15 ng/m³ to 0.5 ng/m³ at any point in time and space. Is the steady-state assumption really valid, and how do you develop a mercury criterion when you have this hundred-fold variation?

Philip Cook:

I would love to answer your question, but I cannot. I do not know that much about the mercury bioaccumulation. If there is someone else here who feels they have that knowledge, I would welcome their participation.

Bob Barrick:

We have done quite a bit of work on mercury and mercury uptake. Mercury is a totally different animal because how it is associated in cells is quite different from other metals. It is pretty complicated. There is literature available, and I think there is a whole conference on mercury that focuses on its uptake in the environment. Mercury does not fit neatly into these models. Consult the literature because there is information on how that should be done differently.

Philip Cook:

I can agree with that.

Q (Gayle Garman, NOAA): I am pleased to hear someone talk about the fact that the environment is not really in equilibrium. Yet you are still making an assumption of steady state and this is for the Great Lakes. Generally, I work in estuaries and they are very dynamic systems. I am wondering if you could comment on our assumptions of steady state or equilibrium for estuaries, and whether you think the modeling approach that you have taken for the Great Lakes is applicable in an estuarine situation?

Philip Cook:

I certainly would not advocate using a steady-state model where we know it would not produce a valid result.

There is obviously a need for nonsteady-state models. To some extent, these empirical tools that I have discussed here today may be usable, when necessary, in nonsteady-state conditions. I think you have to get a good prediction of an average exposure relationship, whether it be sediment or water, and then take into consideration the properties of the chemical that you are trying to model. If it is a lower K_{ow} chemical, I think the fluctuating exposure problem becomes more severe.

Q (Charles Kovatch, University of South Carolina): Could you further explain your food chain multiplier model parameter?

Philip Cook:

Essentially, the food chain multiplier is an expression for the organism that you are trying to model, which represents the increase in the BAF over the octanol-water partition coefficient. So, it is an expression of the total effect (in the food chain) of biomagnification on the bioaccumulation factor. It is applied when you do not have the bioaccumulation factor specific for that chemical, so you use a bioconcentration factor or an octanol-water partition coefficient as the surrogate for the equilibrium accumulation. The food chain multiplier, when applied to either the BCF or K_{ow}, predicts what the biomagnification effect would be on the bioaccumulation factor.

Q (Doug Hotchkiss, Port of Seattle): The comment was made in this last talk and in others that we have a lot of data out there. When we make these lists of data, like the compilations of data by URS and others, it looks like we have a lot of data. I have been involved recently in trying to use some of that data on a very site-specific regulatory basis. This is just a word of caution and a note to everybody out there. These lists may be very good for screening purposes to identify what we should really worry about. But when you apply them to regulating in a site-specific situation, you need to take a really close look at each individual paper. If you look at the specifics of how that number was generated, you will find that some of those numbers can be very soft for regulatory purposes. For example, in a study using a spiked sediment, you should check if they rinsed out the interstitial spike before they ran the tests. Some of the numbers can also be listed incorrectly from a paper. These lists can be a great help, but you ought to reexamine the values that are going to be critical in the decision-making process for both the regulated and the regulatory community. You should sit down and take a critical look at what those numbers really mean before just automatically moving ahead with them.

Q (Weldon Bosworth, Dames and Moore): I would like to direct questions to Dr. McFarland and Dr. Di Toro. For those of us that work in wetlands, we occasionally see organic carbon levels that are substantially higher than the data you presented on your slides. There are also substantial amounts of dissolved organic matter. When we apply this on a screening level or for a Tier I risk

assessment, we need to determine what the potential is for bioavailability. Referring just to neutral chemicals, we might want to go to carbon normalization. How comfortable would you feel about applying your theoretical bioaccumulation potential estimate to something that would range up to 30 or 40 percent organic carbon? What empirical or theoretical data do you have to support your answer?

Victor McFarland:

At 30 or 40 percent organic carbon, you might have quite a lot of something that is not natural organic carbon. But it could also be natural material like peat. What we ordinarily encounter is what you see in ship channels. I have not tried to measure anything like you have described. What we have done is to see how low you can go, rather than how high you can go. In the types of situations that we generally see, we have material containing up to 3 percent and occasionally up to 5 percent TOC. But, in fact, I have had to search hard to find anything that I could use in doing those kinds of studies that was much above 3 percent. We have some idea of how low you can go, which kind of agrees with Dom, but I could not answer about higher TOC ranges.

Dominic Di Toro:

We know a little bit about that. The carbon fractionation data that I showed you went as high as 10 or 20 percent. The theoretical limit is 40 percent, meaning that 40 percent on a dry weight basis of organic matter, sludge, or other material is carbon. So, that is the upper limit. Quite a bit of work has been done on partitioning of hydrophobic chemicals to digested sewage sludge. We used a fair amount of that data in the mid-1980s to establish the partitioning relationships that we use. For partitioning experiments and suspensions, I know that carbon normalization appears to work up to 20 or 30 percent.

Q (Weldon Bosworth): I do not mean to suggest that you could probably get away with a Tier 1 assessment on a situation like that. The result of carbon normalizing and then adjusting bioavailability for that would show that there would not be much that is bioavailable. It becomes a critical issue when you consider the relative risks of leaving material there that may not be bioavailable compared to tearing up the whole wetland and trying to restore it. There is not much work being done on bioavailability in conditions when the organic carbon is that high.

Q (Paul Jacobson, Langhei Ecology, Inc.): I have to say I was a bit bemused by Dom's comments regarding the role of ecology in this whole process. I feel compelled to make a short comment on that. I think it is true that toxicology was developed as a science and applied to pollution control decades ago, because ecology was really not up to the task. However, I think that hardly validates the assertion that ecology process and content really is not relevant today and

into the future. And I think that for the past 20 years or so, pollution control has really been defined in toxicological terms. The upshot of the Edgewater consensus was that the approach of the last 25 years is not going to get us where we need to go. There is a need for a more ecologically oriented perspective.

Dominic Di Toro:

I did not mean to say that I think ecology is irrelevant. What I meant to say was I did not see how the type of ecological work that I think is being thought about would get us there any quicker. I think that simple observations of disturbed ecosystems, with no idea of what the causality is about and without the sort of toxicological information that we have and need to develop, is a waste of time. That was the caution, not that we should not do ecology, but rather if you look in the risk assessment paradigm and open the tool box to find what methods actually exist to do this problem, you find a remarkable lack of quantitative methods.

Paul Jacobson:

Well, then, perhaps we are in complete agreement. I think that the need is for good ecology.

Dominic Di Toro:

Yes, exactly right. I am railing against what I see as a return to an approach that I would call Victorian naturalism. That approach involves observing, making measurements, and trying to make some elaborate arguments linking observed changes to causation.

Q (Peter Landrum, NOAA, Great Lakes Environmental Research Laboratory): I was interested in your talk, Dom, since we have done some of that particle size separation and carbon normalization. I would say that the variance in our laboratory-dosed sediments is somewhat larger than what you showed on your log log plots. We see at least an order of magnitude. And, in some cases, we see the condition where the big organic particles do not seem to adsorb as much material.

Dominic Di Toro:

I think what that says, Peter, is that your lab simulations are not at equilibrium. The data I showed you are field data sets.

Q (Peter Landrum): Right, but I still think where you have bigger particles, they are not necessarily going to be at equilibrium.

Dominic Di Toro:

Well, I ask you to look at three data sets that strongly suggest that large particles with high organic carbon concentrations look just like small particles, and that kind of equilibrium must have been there for that relationship

to work. That relationship tests two very interesting assertions: that the system is at equilibrium and that carbon normalization gets rid of the effect of other sedimentological properties.

Q (Peter Landrum): If you put that on a linear plot, you will see that there is still quite a lot of variance there.

Dominic Di Toro:

There is no doubt about that, but put the raw data on a linear plot.

Q (Peter Landrum): Well, that is what I do.

Dominic Di Toro:

Well, then it looks like hash. If you do not carbon normalize, then it is all over the place.

Q (Peter Landrum): Carbon normalization actually does, in most cases, reduce the variance. I will not say in all cases. The other thing, though, that we have seen is even though you look at that carbon normalization, what we see for the bioavailability in the selection of particles by the organism is that they are selecting particles that we cannot even distinguish on a carbon-normalized basis. We see this based on the measurement of concentrations in the fecal pellets, where they are actually selecting particles at a much higher concentration than we can measure relative to our bulk chemical measurements. That is another issue that needs to be considered relative to what we get out of carbon normalization relative to bioavailability.

Dominic Di Toro:

It occurs to me, Peter, that maybe the problem is that you have to incubate your lab sediments long enough to get equilibrium across the particle spectrum.

Q (Bob Barrick): I am glad that Peter Landrum spoke up because the major conclusions from Fred's thesis directly support a particle-selective model. It says the particles are important, and if there is feeding on a particle-selective basis, then you will have dramatically different results. His thesis directly supports that rather than supporting an equilibrium perspective.

Dominic Di Toro:

My only comment is Fred's data are what they are.

Q (Susan Kane Driscoll, Virginia Institute of Marine Science): Dr. McFarland, I was interested in the BSAF database that you showed where the median value for the PAHs was lower than for the chlorinated compounds. Do you have any impression about whether it is more important that there is metabolism of the PAHs, or if you are seeing some sort of kinetic limitation to accumulation, or if it is a bioavailability question like for soot particles?

Victor McFarland:

Well, fish certainly tear PAHs up, and I do not think that the bivalves are totally devoid of some metabolic capability there, too. But I do not think that is the whole answer. Anthropogenic PAHs are associated primarily with soot. The chemical is going to be distributed throughout the soot particle. In order to desorb from the interior of a soot particle, I think you would have to imagine something like a chromatography column, where you have many steps of desorption, sorption, desorption, and so on, which would make the process quite slow. I think there is some evidence for this in the literature. It is a complex process. There is certainly much more to it than just one or two things that determine bioavailability and the BSAFs that we have measured for organisms and chemicals.

Q (G. Fred Lee, G. Fred Lee & Associates): I would like to ask Mary, what, if any, timetable EPA has for putting where we are now into a regulatory framework?

Mary Reiley:

A long one. If we look at the past history, it is a long process from the time we have data and come up with the theory, to moving that into something that can be practically implemented into a regulatory program, and then to actually getting it implemented. There is an introductory period to get the research accomplished, about 5 years to make it practical, and then another 5 to 10 years to get it implemented. I think a lot of people believe that if we publish something today, it is implemented tomorrow. That is not the way it works. It takes a lot of time to get the bugs out. Five years would be nice, but I would say to actually see it being implemented in a regulatory program on a routine basis would take longer than that.

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