

APRIL 28-29, 2001 MODELING MEETING - TRANSCRIPT OF DISCUSSIONS

DAY 1

Introduction and Welcome by Courtney

Courtney introduced the concept of adaptive management, and suggested that this might be a useful approach to keep in mind during the discussions of uncertainties, particularly as these are addressed using modeling approaches.

Anne Fairbrother, Parametrix Uncertainty and Risk Analysis

Bartell: Has it been agreed on that there will be a goal of adaptive resource management? It's my understanding that there's a proposition to go ahead with the dredging. A BA was initially rendered and agreed upon - while I would certainly endorse an adaptive mgt. approach to the project, that's not my understanding of that's in fact where we are. It's my understanding that we're focusing on the quality of the technical information that went into the previous decisions that were made thus far. Can we now go back and thru the application of uncertainty principles, reevaluate that information to try to understand what an appropriate decision would be.

Young: I agree with Bartell's comments that we're at a point where we're evaluating the risk portion. In fact, there was an earlier agreement to move forward thru the BA, thru the BO, into the implementation of this project. Adaptive management would be a process that could be proposed in the new BA when we consider monitoring.

Boesch: I've worked in various parts of the country in coastal environments using adaptive approaches. One of the challenges in this case is that not all actions and not all risks are as controllable or versatile. In the case of fisheries mgt. and, you can adjust harvest depending on what the results are. And in the case of dams, you can control releases. But in the case of dredging, it's sort of you do it or you don't. Although I guess it's conceivable that, after the fact, if we find that the channel deepening was having effects that weren't desirable, you could let the channel fill back in. From a practical standpoint, though, it's probably unlikely. In a similar vein, some of the massive types of public-funded reconstructions -- like the Everglades, or creating new deltas in the Mississippi -- although they certainly have some adaptability after the fact, the decision to do them is a big decision that you make in the face of much uncertainty, and the alternative of undoing what you've done is not going to be there. How do you factor those kind of differences into management actions? You might say the same thing pertains in terms of reversibility of the risks. I guess the reversibility of the risks in that case, speaking in terms of the magnitude, have to play into it.

Fairbrother: I would suggest that this channel deepening work that is currently going on, as well as the proposed action, is not being done in a vacuum. It's being done within the context of a lot of other management. actions that are going on in the estuary and upriver. All of those are being integrated as we look at how this next step in this action may affect the salmon. And while you

can't necessarily change the physical aspects of having dredged deeper some parts of the channel, you can adjust the other management actions that are going on within the river in ways that can mitigate that action.

Courtney: If the decision is made to proceed, and mitigation plans are put in place for that, and subsequently we find that our underlying assumptions were wrong and the scale of mitigation is not appropriate to offset the consequences of the action, if you don't have an adaptive framework, that's an irreversible decision. But if there is an adaptive framework, something can be done to put the system right.

Whitney asked for comments on the notion of ecosystem management as it applied to the wetted margins. *Fairbrother* replied that this was an issue of scale - how are we defining "ecosystem,"? Are we defining the ecosystem as only within the river channel, or are we defining the ecosystem as a larger portion of the watershed? Certainly, systems interact, and there are a lot of interactions from what's happening on the land and wetted edges of the river channel with what occurs in the river channel itself. Scale continues to be part of our discussion, particularly in talking about our conceptual models. All this has to be put within the context of the proposed action - what is the connection between that action and the different parts of the system? Is there any reason to believe that that action changes other parts of the system that then feed into the river.

Courtney sought feedback from NMFS, given the acknowledged need for more information. If NMFS elected to go forward with this project would adaptive management be something the agency would be interested in adopting? How does NMFS' research and monitoring interests (raised in the withdrawal letter, and shown in document 2) fit in with the idea of going back to the conceptual models?

Tortorici: We need to make a determination about whether the project goes forward as a whole. Assuming that it does go forward, I think taking an adaptive management approach is the appropriate way to go, from a learning perspective. That fits into the context in the monitoring program that we had originally envisioned with the first BO, having a comprehensive monitoring program that looked at physical-biological aspects and integrate those over the short term before, during, and after construction. The caveat that I'll add to this is something that you just said Don. This is a bit different than Glen Canyon Dam. They had the luxury -- if I can use that word -- where they could be testing water from these scenarios and they could stop it and start it, and that was the beauty of what went on there. This project is a little bit different. It's more of dig or don't dig, and so I guess the piece that we need to be cognizant of is how you apply an adaptive mgt. approach to a project like this, and can we make it real, and what are we going to derive from it? So NMFS is not at all opposed to the concept of adaptive mgt. to use in this project as this project moves forward. It's really a matter of how to adapt it to the situation at hand.

Fairbrother: We can distinguish "active" and "passive" adaptive mgt. What you were just referring to was the active type of "let's try different things," and then see what happens in terms of an outcome -- truly an experimental design vs. one like this where you can't do replication, you can't start and stop and try different things. What you can do is develop your monitoring plan to refine your models and reduce your uncertainties so that the parts of it that you are managing can

be changed as you move forward. And I would suggest that even if the decision is made not to move forward with this proposed action, that is a decision, and that decision also deserves monitoring and evaluation. So you would bring this into effect, regardless of which decision you make.

Tortorici: I would also see this approach applied in terms of ongoing maintenance of dredging activities. So I think from a broader ecosystem perspective, it could really change the face of how we're viewing ecosystem operations.

Eriksen: People are suggesting that the decision to deepen this channel is an irreversible one. I just want to point out that the locations we would have to dredge and deepen are primarily the same locations where we continually do maintenance dredging on a regular basis. So if an impact is identified, the decision is not irreversible. Many of the areas would fill back in to the current channel in just a few years.

Ron Thom, Battelle Marine Sciences Lab
Conceptual Model

Bartell: Your conceptual model is important in relationship to this overall deliberation from several viewpoints. One is certainly outlining the complexity of the system and indicating what has happened historically to the system in terms of creating conditions that have either been favorable or unfavorable to the continued production of salmonids as the resource of concern. With regard to the particular decision we are assembled to address, it raises the issue of is the decision to be made primarily in terms of an incremental change to the existing condition, or is it something that has to be looked at more in the historical, comprehensive context of what's happened to the pre-settlement conditions; the notion of a sliding baseline. It certainly makes a difference in terms of what information is pertinent and how it's used in the overall decision-making process, and it seems to me that's one of the reasons why this conceptual model is very important in trying to resolve these differences of opinion.

Thom: Correct me if I'm wrong, but the baseline conditions relative to this project are the present conditions.

Bartell: Well there seems to be lack of agreement there.

DePinto: From the point of view of a modeler, you want to ask yourself not only what the response of the system might be, but you also have to ask whether you can either model or detect that response within the context of the natural variability of the system. Regardless of what your baseline condition is, that issue has to be dealt with in terms of trying to decide what it is you want to model and what the level of resolution needs to be.

Bartell: I agree with you - almost. Depending on the historical complexity of things that have changed the system from pre-settlement times, it might make it more or less -- I'd say more --

difficult to see this additional signal in terms of response from a baseline condition.

Boesch: Steve (Bartell) asked about whether the historical or present condition is the baseline. Perhaps a more constructive way of looking at this with respect to some future desired future condition, is some restoration goal for the estuary. And then one can ask the question, not what is the change of an action from what the condition is today, but the degree which a change affects the attainment of a restoration goal. Does it put you closer or farther away from reaching that goal. So maybe rather than the control being the existing condition, or some pre-existing condition, some potentially, achievable desirable condition that you then have to weigh changes, actions, in terms of whether that makes attaining that goal more difficult or more achievable.

Thom: That's an interesting point of view. Whether the project affects the habitat-forming processes, and how much or not, may affect your restoration goals. And if you're going to interrupt processes that affect say the formation of marshes, and formation of marshes is the thing you're trying to do in mitigation, you need to know that.

Courtney: It's my understanding that the regulatory agencies have an ongoing debate about what constitutes the appropriate standard -- whether that should be a recovery standard, or no net loss of current conditions standard. That's an ongoing debate on a national scale. I think we can all agree that they may be desirable goals but its a question of whether that's the legal standard. I think we need to be cautious about trying to set new goals when that's really not our role. What we should be trying to do is evaluate potential impacts on current conditions, future goals -- lay those out, but other folks will be deciding what standard they're applying and what the law says.

Boesch: Let me try to argue a little on this. It seems to me, one of the assumptions, and we have the literature which provides some evidence for it, that the estuarine condition is going to be a key factor in the long-term survivability of some of these ESUs, right? Because we had some literature which said that even if you removed the dams the population would continue to go down without also thinking about increasing survivorship in the estuary. You can argue about that, but if that is indeed a basis for a decision about endangerment in terms of the future of the stock, then one has to then ask what are those changes that will have to be made in order make that stock survivable. Then those have to be included in some sort of a vision of what that ecosystem is. And it's in that context that you can view any action as inimical to reaching that vision, or neutral, or assisting.

Curtis: One the questions I have about applying some of this information is how good are the data out there for populations, densities, and abundances of some of these organisms you've identified as crucial, the deposit feeders for example. I have some feeling for how much data are out there on the salmon themselves at different stages. I know it's not all that great, but at least there's some historical info. As you worked on this, did you get a feeling for the next level of the food web -- the heterotropes and the deposit feeders. How much do we know about where we are now, where we were, and where we might be?

Thom outlined some of the available data. In the conceptual model , where the lines are connected to salmon, those links are proven - the insects, the Corophium, and the Daphnia. They are found

abundantly in salmonid stomachs. Things like the carbon ratios in the prey, prove that the macrodetritus is supporting the Corophium. There is no available stable carbon data. *Larson* indicated that the data set on benthic invertebrates was fairly good. *Goldman* encouraged the use of stable isotope work if it was available - it takes a lot of the uncertainty out of the food chain analysis.

Dunne: When Anne (*Fairbrother*) was laying out the process of adaptive mgt., she said first of all you lay out your conceptual model and that allows conceptual models to be shared. My question is do the agencies share the conceptual models, or do they have any fundamental objection to its nature?

Eriksen replied that the Corps found the model useful. *Young* stated that the USFWS had not yet the opportunity to review it.

Dunne: I can't believe there hasn't been a conceptual model until now.

Tortorici replied that there's been a huge amount of money focused on the more upstream portions of the system where the dams are - the big black box in all of this is the estuary.

Dunne: But by 1990, there was enough in-depth research in the estuary to have a special volume of *Progress in Oceanography*. There must be some other attempts to do what you've done that people have been playing with as a basis, either for their objections or their plans. Among all those models are there strong differences of opinion about how this system works? Do you all share more or less the same conceptual model?

Casillas: From a general perspective, I don't think there's much disagreement. The issue is how much data exist to support the underpinnings of those particular processes in this system. We don't have the information about how processes support salmon. The *Progress in Oceanography* volume is based on one year of data. The system is very dynamic, so it's unlikely that one year of data will characterize the system in an effective way.

Dunne: What you're uncertain about are the rates of the processes that are represented by the arrows. And your saying that is it that one side of the debate estimates that a certain arrow may be either not quantitatively important, or would not be quantitatively affected by the dredging process, and the other side of the argument says we don't know that?

Casillas: I think it's more fundamental than that. It's not a question of the arrows

Dunne: It goes back to Anne's initial statement. She said that the process of building a conceptual model is a process of developing hypotheses about linkages. Then her diagram shows you represent those hypotheses about linkages by arrows. So presumably in Thom's conceptual model diagram, all the arrows represent is hypotheses about linkages between habitat structure and function. So why are the arrows not vital?

Casillas: They are.

Dunne: So you say they're vital, and the other side says -- now I'm making this up -- that we've addressed the arrows, and our estimation is that this dredging project will not impact that arrow. Is your position, now that you've stated that the arrow is important, that you don't yet know what the magnitude is and what the impact of the dredging program on the magnitude of that arrow will be?

Tortorici: It could be a magnitude issue - it could be a question of whether that arrow should be included at all in the discussion.

Bartlett: That really stimulated one of my earlier remarks in terms of the 'sliding baseline' because, of course, that is one of the points of criticism in the review of the original BA. If it's our charge to review the existing technical information, it really helps me to know if I'm looking at it in context of an incremental impact to the current condition, or if I'm supposed to review it in the context of the historical development of the whole estuarine system.

Tortorici: In the first workshop, when Doug [Young] and I talked about the baseline, we talked about the condition of the project in the context of all that's happened to get you there and overlay the proposed action on top of that. The reason we look at that from the ESA context, is we recognize that the system is degraded; it's on a downward slope, as evidenced by the existence of threatened and endangered species. So the question becomes how will the project shift that slope at all? Is it going to make the shift more severe? Not affected? Or is there something we can do with regard to this action to allow not only the survival, but the swoop back up, allowing that line to come back up toward recovery. So when we look at a project -- not only this project, but projects in general -- we're taking a broader ecosystem perspective in which to place the project and value it.

Casillas: There's a science question of comparing what we think happened historically with present conditions; the overlay, as Cathy [Tortorici] suggested. On top of that, however, is a management issue of determining the baseline. Management decides on the baseline, not science.

Curtis: In terms of what would help the salmon, from reading the paper that the NMFS science center laboratory published in *Science*, they said if you could increase the survival in the estuary and ocean about nine percent it would stop the decline. It would do more than doing more radical things upstream. So really what I'm trying to get to is - do you know what's limiting in the estuary? What specifically affects survivability? In my view, only if we have some insight into that that tells us what to look for when we look at these physical and chemical models.

Casillas: To answer your question, the answer basically is no (no information on increasing survival in the estuary).

Quinn: This paper (Kareiva et al in *Science*) has been referred to a number of times. Initially, we need to point out that it's not for all salmonids. It's for Snake River spring/summer chinook salmon. So not all components, and not all species, and not all races. The second is it's based on a lot of data, which simply aren't presented here. I would need to see a lot more of the data before I

would be willing to accept flat out that it's correct. Ed [Casillas] has made another valid point that the early ocean mortality and estuarine mortality are rolled in; there's absolutely no basis to distinguish those on the data. And a number of other assumptions are made that may or may not be true. I respect the people that did the work, but I'm not willing to simply say that we know this (important role of estuary) to be true.

Sullivan: You [Dunne] were asking if anyone had been working on a conceptual model. They must have because most of this literature addresses and provides some data on most of the species. What's generally not there is the synthesis of how productivity in the estuary is driven for individual species vs. community. But it does seem like some conceptual linkages begin to pop out. Do you have a sense that there are some of the driving interactions begin to pop out from the information that's been collected, and that further information could tone down or improve those hypotheses? Or are you of the position that we simply don't know anything?

Casillas: Yes, we can make some inferences, we just don't have a complete understanding.

Boesch: I'd like some ground rules - you should have to tell us two things you know, before you say "we don't know"! We have a tremendous amount of information here, and it's actually quite a lot of knowledge. It's not perfect, but we ought to be looking at the key things in this body of knowledge that provide the uncertainties that we need to try to resolve to find the disputes. Broad platitudes about what we don't know and would like to know are not very helpful.

Goldman: One of the things we've know for a long time is that estuaries are nutrient traps. And the behavior of the salt wedge -- it's penetration upstream -- is a very important factor in determining how this nutrient trap functions to produce the high fertility that estuaries are well known for. It seems to me that this still remains somewhat of a gap in our knowledge, combining the physical facts of the dredging -- whether it's sedimentation or allowing the salt wedge to go up further or be more concentrated in the channel -- is going to have a major impact on the analysis of whether the food chain in the estuary is going to be helped or hindered by the project.

Larson: There is a lack of knowledge, but there's also a fair amount of knowledge on how these processes go on. There's a fair base of knowledge, which our EIS and BA were based on. Like Ed (Casillas) says it's not a conclusive array of knowledge, but it's still enough to move ahead with an assessment of what we feel the impacts would be.

Courtney: This next presentation is a preview of where we'll be in the next workshop. I've asked the agency folks to provide just a simple summary of what we'll be exploring in considerable depth at the next meeting.

Tortorici and Young
Overview of Salmonid Estuarine Ecology

Eriksen asked about the use of the term lower estuary. *Young* replied that they (NMFS and USFWS) are working from a summary of estuarine sampling for four or five years in the 1980's.. 1980 was the most focused year of work. They split their sampling up between upper estuary river in-flow areas and the lower estuary. And they looked at lateral habitats based on beach net seining. Since you can only run a beach net off a beach, the researchers considered that shallow-water habitat. And they did purse seining out in the mid-channel areas, so they were looking for areas probably greater than at least 5 meters deep when they were doing that work. *Larson* and *Casillas* discussed the use of different capture techniques for different fish types. Stream-type fish in channel margins don't show up in beach seines. Stream-type are caught with purse seines, which are used in deeper water.

Fairbrother: I believe these last two presentations were very helpful. They are starting the discussion we were looking for as to how can we get boxes of information and connect arrows between structure and function, which in this case are the salmon. There are probably still question marks over the arrows, but we've heard that there's collective wisdom -- not just from the system, but from other systems -- that we can apply to this system as well, which allows us to start building those framework concepts. And as we move forward this afternoon and in the workshop two weeks from now, we'll start clarifying some of those boxes and understanding which are the big question mark arrows and which ones are smaller. But we're not entering into this whole discussion with no information, and as we go through the discussions, we start popping up little bits of new information, or different ways of looking at the information, so as Don [*Boesch*] was saying, if you can say two things that you know before you start putting question marks on things you don't know, what are the size of the question marks is kind of what we're trying to get at.

Tortorici: In this presentation, we talked about ocean-type vs. stream-type and the fact that some of these species are spending longer in the estuary than others, but we don't want to leave you with the impression that just because a particular species is spending a shorter period of time in the estuary vs. something like a chum, that that time they're spending there is unimportant. It could be just as important for a fish that's spending three days in there for whatever critical need it may have vs. another species that's spending a month. I mean the value of that is difficult to determine. So just keep that in mind as we talk about the different ways the species are using the system.

Boesch: Pursuant to that point, Cathy, one of the challenging things is that there are so many ESUs here, and if you actually had to define all of the specific habitat requirements and sources of mortality for each of them within the estuary in transition, and then try to develop a management matrix to optimize for all those... It's a fool's errand, quite frankly. What I gather is the approach in the *Bottom* et al. argument is that given this diversity, and that one of the management objectives is the maximum amount of diverse habitat -- I understand that and that makes some sense. But as you get into the specifics of trying to understand the vulnerabilities of these stocks, am I correct in hearing that if you had to pick one type that was particularly vulnerable that you might want to focus on in this analysis to see if the action were reducing the survivability of these populations, it would be the ocean-type chinook.

Tortorici: It's such a painful discussion for NMFS in terms of which do you focus on vs. which you don't because then of course the implication is that somehow you're letting the rest of them go. We actually have talked about this internally. The issue is if you're not seeing a change in the system based on chinook and chum, it's probably going to be much more difficult to see a change based on those other species. I may be overstating this, but we probably have more information on chinook than we do on most any other species. But I think from the perspective of what we would want to do here, we'd want to look at both chinook and chum, see how the habitat changes in relation to what we're talking about, and then go from there. Hopefully, we can generalize enough to those other species so that we're not making some overleaping decision at the end that's going to place us in a difficult spot.

Dunne: Cathy (*Tortorici*) said that managing for genetic and life history diversity was the goal for maximizing survival and that if the project were to change conditions that might alter that range of diversity, you would have a concern. I'm trying to understand the significance of the fact that then, for example, you said there's a range of residence times for a species in the estuary. What does that range of residence times mean? Does it mean that if you took a box full of salmon -- today's salmon just coming back from the ocean -- put them at the mouth of the Columbia and then they would spend some time in the estuary, there would be a probability distribution of residence times, and that that would be because of their genetic diversity, or would it be accidental encountering of food, or is your observation comparing inter-annual variability so they come back one year and conditions are not so good so they scoot through the estuary quickly. In other words, the habitat fluctuates from year to year, do they read the habitat and decide whether to stay or not, or is it something more fundamental that will not be affected by, for example, changing flow conditions of the project? What is the observation that there's a range of residence times in the estuary mean?

Tortorici: This issue of residence time is partly an environmental issue in terms of what those fish are encountering vs. an issue of their genetic component. The thought behind this is, from a survival standpoint of a species, you want to have enough plasticity in the species such that as they're encountering different conditions in the system, there's going to be a component of that species that will be able to react favorably to that and others won't. And so preserving the amount of diversity - the ability of these fish to react to changing environmental conditions -- is really what we're after. I don't know if that fully answers your question or not. We just recognize through observations of these species that they're spending various amounts of time within the system, and from a survival standpoint, that's a good thing because it allows the species as a whole to adapt to changing conditions to make sure that enough are going to get back upstream to spawn.

Dunne: You said, 'We're managing for genetic diversity....' Then you said, 'If the project changes conditions in the estuary, it would affect some of these...

Tortorici: Yes, because if the project is changing conditions such that habitat is changing, then those portions of the population that might have a residence time in a certain habitat type -- if that habitat is no longer there, if it's limited in some way, then you've created perhaps a disadvantage for that portion of the population. It doesn't mean that, on the other hand, that another proportion

of the population might be at an advantage. We can't say that for sure.

Casillas: You [*Dunne*] were essentially asking if these fish are hard-wired for residency time. In our conceptual model, the answer is no. It's really a composite of information -- hardware, genetic, and phenotypic expression.

Dunne: I'm trying to understand the linkage. For example, if the variability were in response to let's say habitat condition, and then we get modeling done this afternoon and they say, 'Well, we can simulate through a time series of forcing conditions what the range of variability will be. If the variability of habitat condition maps into variability of residence time, then you've got a linkage. And what I'm hearing is some of that linkage, but there's a lot of unexplained variability that's interpreted to be phenotypic. In other words, you have a large component of uncertainty about how an individual fish will respond to a condition in the estuary.

Tortorici: And your question is a good one because it gets to the issue of how you interpret that modeling in terms of the behavior of these fish. That's something that we struggled with in the course of this project is how changing conditions are going to impact these fish, given the 'variability' that they show in terms of behavior with residence time, for example.

Dunne: The way lots of sciences deal with that as-yet irreducible uncertainty is through stochastic modeling. Of course, we don't know what decisions individual fish would make, but we know there's a probability distribution and that that probability distribution will respond to say, mean changes in the larger environmental conditions. Are those probability distributions at least known even if they're not understood? What's the basis of saying that there is variation? Are you saying, 'We know there's a range?' Or are you saying, 'There's a modal value and a range, or...'

Casillas: We can document some of that. When they come in the system and when they leave the system. We have these data, but they are not complete.

Karl Eriksen, U.S. Army Corps of Engineers
Overview of Estuary Physical Processes

Quinn: Presumably, in the ancient days it [the Columbia River estuary] was in a steady state; as much was going out as was coming in, otherwise it would've filled up, right?

Eriksen: No; it was filling up. PSU is just now finishing up a study on depositional rates for the estuary. They show deposition back some 20,000 years on up to the present, with the highest rates occurring 10,000-20,000 years ago. Now it's filling slower because the hydraulics are changing and you're seeing more fine materials being carried through now than would've been in those days. But as sea level came up, the estuary filled, all the way upriver to Portland largely.

Quinn: By dredging, are you accelerating or decelerating deposition? Or does it affect it?

Eriksen: We can't quantify that. In the estuary, we dredge here at the downstream tip of Puget Island every year, we dredge in this area around Skamokaway about every other year, and we

have a large point bar here that the channel cuts into and it continually back fills. We have a developing problem downstream of Skamokaway where sands that are washing across the shallow areas are cascading into the channel, and shoaling the south side of the channel. Most of those are dredged by hopper dredge -- we take the material and distribute it within the channel at different locations. We have a disposal island here at Pillar Rock. We dredge near that island almost every year. Most of that is going in-water because the island has hardly any capacity at all. We dredge a large amount here in the Rice Island/Miller Sands area. About half of it is pumped back up onto the Miller Sands spit, which was constructed as an environmental enhancement in the early eighties. It erodes constantly, re-deposits in the channel right about here, and we dredge it and put it back to maintain the spit. Most of the material that is removed from the system is here on Rice Island; there's about 12-13 million yards of material in storage there. We've been using it since the early eighties, so there's 20 years of material there. We have another major shoal that we dredge downstream of Astoria. All of that material goes back in the water, either down here in deep water, or over here in the north channel.

So we are dealing with whatever is deposited in the channel. Most of it is bedload related, coming off the sides and into the channel. What's happening out here and in the rest of the estuary, we don't have much of an effect on. So, suspended sediment, which carries the bulk of the sand, comes through on high flows, maybe is undisturbed at all. Some of it we trap the net balances. We just don't have enough information to say we're trapping more than we would've otherwise, or we're not.

Casillas: You noted that the sediment transport has dropped from the 1970's to currently from 5.3 yards to less than 1. What is your take on how that affects physical processes in the estuary? For the panel, the consideration is what is the impact on biological processes that need to be considered with that drop?

Eriksen: I'm not sure. It's probably reduced the amount of deposition that's occurring throughout the estuary because there's not as much sand coming in. The estimates on fine sediments were that 30 percent of that was depositing, which is also part of the reduction. So with that kind of trapping of fine sediment probably most of the sand is being trapped as well. So annual deposition has probably gone down accordingly. The drop may be significant, and it may not. Channels are still changing and there's still bedload movement out there, both in the main channel and at least in the shallows adjacent to the main channel are still transporting sediment. Beyond that, I can't really tell you what it means.

Boesch: Is there any evidence that it's maybe resulting in a reduced deposition of marsh colonization of tidal flats?

Eriksen: It could be. Evidence, no, because there was no pre-development rate of in-filling, and there's been no measured rate of those created now. In theory, you'd think that that might've been one of the results, but as far as data and such, there's no basis to make the comparison.

Goldman: Any data on shoaling, resulting from dredge spoils at the margins? Away from the channel?

Eriksen: There's data, yes, but there's no shoaling away from the channel. We've done a number of studies over the years. A couple of the more detailed ones were on disposal sites where we went in intentionally and called "point dump". We created a mound so that we had an identifiable figure on the river bottom, and then detailed surveys on those for six months to a year to see what happened to them. Because there was a perception that material dispersed from a dump site. Well, those mounds stayed in place. There was some initial reduction in height. We couldn't track sand waves away from them and we couldn't see a shift upstream or downstream; they just dropped in surface height. It may have been settling -- we don't know for sure -- but they dropped for a couple of months, and then they just sat there. A little bit of movement, some sand waves, developed off them, but largely they were just like the rest of the bed after that. So, there were no immediate effects a distance from the disposal sites themselves.

Dunne: What are the biological functions of sedimentation and sediment transport?

Well, first of all you've got sand that settles onto the bed someplace so there's a burial and an accretion. You've got mobility of bedload, so you've got sand waves moving. Fine-grained sediment -- some of it settles out in quiet water. So there's a shoaling.. at least however small it is, there's a very good accretion rate. Some of that fine-grained sediment presumably has a nutrient content, right? So there's a bio-geochemical flux to that. Even before this settles out, there are patterns of turbidity, and some of that may be mineral and some organic. So there is the physics. What's the biological significance of every one of those? Can we make a list and then write on those little arrows of Anne's [*Fairbrother*] a biological response of each of those? Then we can ask the modelers what will be the effect of this project on each of those physical responses, and therefore, on the biological responses.

Goldman: For instance, in the [San Francisco] Bay delta, the lack of transparency from sediment transports is a major factor in productivity because the nutrient levels are sufficiently high that support all the photosynthesis that the phytoplankton are able to do in their limited euphotic zone. So suspended sediment has a major impact in the Bay delta on transparency and hence productivity.

Dunne: Can anyone put the biological responses on the physical responses I've just mentioned?

Baptista: I'm not a biologist, but I've asked that question of biologists a number of times. Although the answers vary, the notion is that the system seems to be turbulent enough that biologists don't consider changes in turbidity a major issue, at least in the context of Cathlamet Bay and salmon survival. When we've discussed measuring physical parameters, that will help fisheries research in Cathlamet Bay, turbidity has come up frequently as one of the parameters that can be measured, but it doesn't come up constantly at the top of the priority list to be measured. It comes up as 'it would be nice to know,' but not fundamental.

Dunne: So you're saying that turbidity is not thought to be a limiting factor in this system? I was also thinking of suspended particles being a food source. Is there any evidence that that's the case?

Baptista: There is significant evidence that there is biological activity in the suspended sediments in the ETM, not necessarily that it affects Cathlamet Bay-type of issues.

Dunne: That sounds like one of the things we'd like to ask the modelers if they can generate. Examine what this project would do to that concentration of whatever it is -- food, biological activity -- that's suspended sediments, so to speak.

Baptista: There are a couple of things for context that may be relevant for you to know. First, a bank-to-bank survey of this system has not been done since the late fifties. There's been a lot of surveying done in the main channel and near the main channel, but bank-to-bank surveys have been lacking. That is actually one of the difficulties in doing modeling. Just recently, we found out that the bathymetry that we were using in our models dated back from 1953 to 1958, which is the most recent date. And there are some changes between 53 and 58. So that's one general piece of background.

The other thing you should be aware of is that this system is really very turbid. When my field staff goes to the field and dives to maintain the stations, they don't operate by sight, but by touch. It's very turbid in general in the areas where we have instruments, which are really quite representative. These points don't address your question, but those are background...

Dunne: So even though it's a very turbid system, the biologists still don't think that turbidity is not a limiting factor.

Baptista: The changes in turbidity that will come with a small alteration of the system will be noise relative to the actual value of the turbidity (and its variance) that is already within the system.

Dunne: So just going back to my list of physics, are there any other biological responses that the biologists can identify?

DePinto: One of the things that was brought up in Ron (*Thom*)'s talk was that there seems to be sort of a shift towards more biotic solids coming from upstream. A shift toward that as a pelagic pathway for food. And so one of your items was the fine-grained biotic solids transport of the system. There seems to be a change that has occurred pre- and post-regulation, as we saw with the data. That's one thing that probably ought to be looked at. It's not just turbidity per se, but it's the kind of solids -- their size and characteristics -- that lead to that turbidity that we really need to think about if you're going to think about the effects of an action on turbidity.

Dunne: So is the question then for the modelers, 'Will the deepening of the navigation channel increase the efficiency of firing the fine-grained sediment downstream and lowering the lateral diffusion? Is that a question that biologists would want to know?.'

Baptista: You are starting maybe too wide in your parameter list. If you think about what physical modelers can give you, you really need to start at water levels. Then velocities, then salinity, temperature, and sediments. In a system like the Columbia, I would say that modeling sediments is

neither an easy proposition, or one that is going to be accomplished any time soon. I've been asking the question what is the level of interest in turbidity as a parameter? Of course, it depends on what question you're trying to answer. If you're focusing on impacts on juvenile salmon for instance, in Cathlamet Bay, the interest level has been low.

Courtney: We need to hear prioritization from biologists in terms of the parameters in the model.

Tortorici: What Steven [*Courtney*] is referring to is a conversation that we had where we prioritized the elements that would be included from the physical side in the model. We came up with five priorities...

Casillas: And as Antonio [*Baptista*] said, turbidity is not the first thing on our list. It's there, we talk about it, but we're really not sure how to grapple with it as a concept and a feature that salmon are relating to directly. In an indirect manner, the issue may be how does turbidity represent some sets of functions that represent ecosystem function that benefits the estuary in general. In that regard, maybe ETM, and infusion of materials into the ETM, as a metric that you would monitor as one of the measures of ecosystem health that one would go after. But the connection to salmon, which is the question we are posing, turbidity per se is not high on our list, but it's on the list at some point, but we're not sure exactly how to connect it in a direct way.

Bartell: If I recall from the review of the original assessment, there were some assertions made about how increased turbidity might further reduce reactive distance for visual predators.

Casillas: Correct. On the extreme side, there are some behavioral issues, but if we were to look at how the project would affect turbidity, those measurements come from catastrophic events in which they determined effects when Mt. St. Helen's blew. The amount of turbidity that was in the system was well over what would normally be seen. Under those situations, they did in fact see measured reduced reactive distance and an ability to capture food.

Bartell: The other issue was the location of ETM.

Casillas: Right. To me, that's a general sort of question about estuarine function. Again, the linkage to salmon would be somewhat distant. One way to look at would be to say, 'If the ecosystem is operating properly, then it's to the benefit of salmon.' How we qualify and quantify that with that measurement is a bit shaky at this stage. But I would ask Karl (*Eriksen*)-- one of the original questions I had -- on the input of sediment and the sediment change, how do you think that affects the ETM per se? Is it driven by what's coming into the system? Do you have any sense of that? You know, the reduction we have of 5.3 to less than 1 over the past 30 years, shall we say. Do you characterize that as having some impact on the ETM, and do you know that it has or hasn't?

Eriksen: No, I don't know that it's had any impact. ETM is essentially the turbulent front of the salt wedge, and it might lower the concentrations a little bit, only of sediment...

Casillas: But would that be something that we want to go after as one measure from general

ecosystem health?

Boesch: The sediments in the ETM are basically recycled sediments, they're re-suspended sediments, they're not necessarily sediments that are being actively put out at that time. Secondly, the biological importance of the ETM, at least based on studies elsewhere, has very little to do with the fact that it's a TM. It's that the physics, which creates the ETM, also is important in aggregating larvae and food, and things of this sort, independent of what the turbidity is in that particular system. So it's a coincident thing that the physics that results in a turbidity maximum has biological significance. So, the question is, 'What do we know about the effect of changing channel morphology on the location of the ETM and the characteristics of it?' As a physical phenomenon, and not just the fact that it's high turbidity.

Tortorici: When we raised those issues about the ETM, that's what we were driving at. Is a physical change in the system going to affect the ETM, and if so, in what manner, and what can we say about that? And whatever that change is, large or small, then how do we value that change as insignificant, more significant, whatever. We had taken a look at the modeling input-output table as it was being developed. If you look at the left side of the column, it talks about hydraulic parameters of concern, and I'll just read them off: Salinity, ETM, surface water elevation, depth, velocity, shear stress, suspended sediments, and temperature. In having our in-house discussions, we thought that the top five to take a look at from a modeling standpoint would be salinity, surface water elevation, depth, velocity, and temperature. And then following that, in a more nested sense, suspended sediments.

Goldman: I wouldn't agree on the suspended sediments from what we've been finding in the Bay Delta. It's a major factor in the fertility of the system. I think it ought to move up in priority, right toward the top.

Casillas: Did I hear you correctly? You were saying to elevate suspended sediments?

Goldman: Yes. In terms of importance. One thing, you've got automatic filter feeders in there and they take in sediment, along with any organic detrital material that they can use for food. If you've got more suspended sediment, they get less nutrition as they pass food automatically through their guts. Plus the euphotic zone is so greatly reduced by turbidity. In fact the Bay Delta system, according to the most recent studies, is really limited by turbidity.

Boesch: I think there's no question that primary production in this estuary is limited by turbidity, too. I guess the point in hand is the degree to which we think changing the morphology of the lower estuary by deepening it by three feet is going to change the suspended sediment distribution.

Goldman: Well, I was thinking more of how much more stuff is stirred up and the fine components of it staying in suspension. It's been stated, of course, that most of what's been dredged is sand that's going to settle back out, which won't have much of an impact on turbidity. But it would be nice to know how much of the material is fine enough to stay in suspension in lower turbidity in the estuary.

Dunne: Regarding this table, the third column, under modeling approach, it says connection to biota. Do you expect that to emerge from a modeling approach, or are you in a position to specify. What is it you expect will be the connection of shear stress to biota, for example?

Casillas: This is developed by Karl and Rick. We've had input to it, and we tried to identify physical features we think are important that may constrain availability of salmon habitat. We were trying to help by identifying the physical features that one might want to look at as input parameters and that would help guide output parameters. The connection to the biology, we agree, is going to be the difficult one to make.

Courtney: Let's save this till tomorrow (discussion of table). To some extent, I wanted to maintain your focus on trying to identify the factors we need to incorporate. I'm not trying to dissuade you in any sense from driving to that conclusion. I think you've heard that folks have been thinking about it and doing their best to prioritize parameters. Since we've got presentations and we'll get to some of that tomorrow, maybe we should focus back on salinity modeling opportunity.

Rob McAdory,
Overview of Corps Modeling

Goldman: Since the salinity flows go up so dramatically with low flow, what's the minimum possible flow in the Columbia?

Karl: The minimum recorded was 36,000 at The Dalles, and that day had 5,000 in the Willamette, so about 41,000. Historic flow at 40,000; with regulation, even this year, they're predicting somewhere in the 70,000-80,000.

Goldman: That's lower than the 120,000 in the model.

DePinto: In your model, basically the outputs are salinity, tide range, and velocities. You didn't really talk about velocities. I was just wondering whether there were changes in velocities.

McAdory: I didn't compare those. I could've, but I didn't.

DePinto: Well the reason I ask is that while it's beyond the capabilities and the time available to actually model the sediment transport system, we might be able to get a handle on the extent to which this might impact re-suspension by looking at bottom velocities or actually calculating bottom shear stress and to see whether that changes significantly from base to plan. Not actually modeling the sediment transport, but just seeing if there are changes that are large enough to affect re-suspension.

McAdory: This graphical user interface allows you to take the difference between solutions. In fact I did that this morning. So if I could find those others, I could take the difference between the

two, or I could take the ones that I have and square the velocities and compare those and make some idea, depending on what your prejudice is, about sediment transport and how it's related to velocity. I do have some of these old results in the can, and they could be used in that way if Karl wants to see that.

Dunne: Karl [*Eriksen*], you began your presentation by talking about the fact that the morphology of the estuary had changed over 100 years toward closing up the north channel and focusing flow into the south channel. Now, one of the things we were given to read said that the major penetration of salt water on the flood tide comes up the north channel and spills across into the south channel. Presumably, given long enough, and I don't know what is long enough, if that north channel continues to shut off then it would spill more salty water on the flood tide into the south channel. But did you decide not to address this question of the general shifting of morphology in the estuary because now the sedimentation rate's been shut off by the dams, or what?

Eriksen: No, we were trying to isolate the impact from the change in the channel depth.

Dunne: Yeah, but you're doing that given today's morphology. And I'm asking the question, 'Suppose the morphology of the bottom of the estuary generally changed in the direction that you were describing at the beginning of your talk and that that impacted the flood tide salinity distributions. Could you imagine, on some time scale, and I don't know what time scale that is, that eventually you'll have a whole different bottom morphology to deal with. So during the lifetime of your channel, you'd have more saline water penetrating further up the south channel, just because you're shutting off the north channel.'

McAdory: What do you mean by shutting off the north channel, Karl? Did you mean it's shoaling in up here and shutting off, or are you talking about water flowing over this area up here?

Dunne: I thought you said earlier that that process is continuing?

Eriksen: Yes.

Dunne: Then is that likely to shunt more saline water on the flood tide toward the south and have more penetration of salt water up the south channel, and if so, what's the interaction on that natural evolution of the estuary with your deepened south channel?

McAdory: One thing I talked about when I was showing those movies is that you can see that right around in here, there was a spot where the salinity went down and I think what it's due to is that one of these little hills was cut down here and allowed more salinity to come up this way that otherwise would've gone this way. In fact, you saw this get a little fresher up in here, and my interpretation was in the absence of this shoal, more of it would come up this way and less that way, and make this a little bit fresher and that a little bit saltier. And that sort of gets to what you're talking about. In other words, you can change the share of salinity that goes between these two if you change their relationship. If you completely put an island here so there was no exchange this way, that might mean that more salt water got up in this way because it can't all get

up in there, or it might mean less because there is a transport of salinity and water over these shoals into this area. The way it is now, it comes up and spills over in there. If you'd shut it off, it might just cause some kind of a residence in here with a lot more water coming and going. That's why you have models so you can play with that and see. But I do believe that the interaction between the salt in the north and south channel depends on these depths to some extent down here, so it's a good question.

Baptista: So the discussions you're having are being based on the bathymetry that's on the screen. You have to keep in mind that what Rob [McAdory] did was a preliminary study with a preliminary resolution in the estuary. So it may be worth looking at the actual bathymetry, and you'll see that that shallow zone is actually cut through a number of small channels, and there are some differences also in Cathlamet Bay. That's useful for the process of discussion. I can bring that bathymetry up tomorrow.

McAdory: The idea was that you make the same mistakes in the base and the plan, they sort of factor out, and you try to look at the differences that are represented by what you're interested in, that is, the change due to the bathymetry in the channel. Any model is going to miss some details. It's just a matter of making a decision about how close you want to get your answer to make a decision to do or not do something.

Ed Casillas, NMFS Science Center
Overview of the Corps' Estuarine Model

Dunne: I know that reductionism gets a bad name sometimes... but, to me, your approach seems to throw up your hands and say, 'I can't isolate anything.'

Casillas: I'm not referring to single projects...

Dunne: A project, like dredging the channel three feet deeper, could be regarded as a single, independent factor.

Casillas: In the context of how we would try to operate and to conduct the evaluation, that was the discussion of the context. Should we look at it by itself in isolation? We would say, 'No, we would want to look at what has happened in the past, how does it look like now, and what do we project in the future?'

Dunne: How could you ever raise an objection to it?

Casillas: I don't follow your question.

Dunne: How can you raise an objection to a project without doing all those things if it's not possible to isolate a single, independent factor? It seems that taking this approach completely neuters any kind of scientific approach. We can't study everything all at once.

Casillas: No, but what I'm saying is that there are certain levels of integration that we know we

can work with now. Remember, this is in the context of salinity modeling as an element to evaluate this particular project. In a generic sense, we need to be much broader in our evaluations. Past evaluations have gone on ad infinitum, looking at very independent factors, trying to make projections, in a reductionist approach. The issue is when we go to the ecosystem perspective, we know that single factors aren't driving those systems so this is only pointing out the broadness of how we have to look at the evaluation not the limitation that presents a limitation to us.

Dunne: In the past, people have looked at tiny incremental changes, and things have gone to hell in a hand basket. It doesn't mean automatically that, therefore, we have to try to take the opposite approach -- which is completely intractable -- which is to study everything.

Casillas: No, I'm not suggesting you study everything. I'm just saying it should be broader than one thing.

Dunne: Okay, how broad? What does 'taking the ecosystem approach' mean? Particularly, since you've already just referred to it in a full historical context.

Casillas: From our perspective, that's what we're trying to gain -- input, information, and evaluation. One looks at constructs of the habitat that we think are important and are defined by physical attributes that define how these animals use that. And ask questions how is that going to change when we do things, or how does that change in relation to natural variation? What happens when the climate changes? What happens when you have a shift in the species composition? What happens in those sorts of contexts? Those are things that we can do and evaluate from that perspective.

Dunne: Can you give me an example of where an environmental management dispute has ever been analyzed with that full panoply of analyses, and thereby, proven to be tractable?

Casillas: At this stage, probably not.

Dunne: And so, do we know that it's tractable to do that? It seems to me you're talking about re-creating the whole of historically-based ecology.

Casillas: What we will be looking at is a process that I think will get us further than we've been.

Dunne: And how much further?

Casillas: In terms of characterizing habitat change, we think much further, in an integrated sense. The question will be, and we understand the difficulty is really trying to arrive at the crux of the biological consequences of those changes. That is where we all agree we will have difficulty, but we will feel much better. For instance, if the outcome in an integrative evaluation of a physical set of matrices is evaluated, and we find that there is no change by however you evaluate it, we'll feel much better from that perspective. It won't answer the question that there won't be any impact, but we'll feel much better to let the project proceed with some monitoring going on. But, on the other hand, if we in fact see some differences with an integrative set of physical attributes that we

see does in fact change in response to this evaluation, or this impact, then the question becomes how do we evaluate that in a biological context. And there is no clear way yet other than to develop a weight of evidence from the information we know as to how to interpret that information. That will be arrived at probably by regional consensus of those experts who have appreciation of the problem and the situation to gain a better understanding for the agencies, then to proceed and make a decision of how they will go.

Dunne: You say we're not going to get to the level of uncertainty reduction that we all feel... science just doesn't do that. Can you say these are the things we need to know before we feel better.

Casillas: That's the question I've been wrestling with for this project -- how will we know if we should or shouldn't feel better? What we did in the report that we recently finished is to develop a family of curves to describe how the system responds in relation to physical features that we think are important over a variety of different conditions and ask the question, 'What has changed?' If we then impose this project on that evaluation, and ask, 'Can we see any further change, or not?' We know the system has changed through the modifications that have occurred over the past 100 years, and then we ask, 'Did it change much more when we imposed this change on it?' We can't really resolve any change with the accuracy that we have. I think at least two things: One, do we know that we can see change with the evidence that we have, which we have documented already under constraints that we've imposed, and two, that we will have evidence that no change will occur when we impose the proposed change in the system. Now the problem will be what happens if we do see further change in addition to the historical change? And that will be a dilemma for us.

Dunne: Have you agreed what those curves will be? Do you say, 'When we get those curves, and then we look at the proposed project through those curves. Of course, there may not be a single independent factor, but there'll be a finite set of factors, and to me, there's not a big difference between those two things.

Casillas: Yes. Mostly because there is change over time, the system has been modified, and there's natural variation imposed on that -- all of those have added to the set of response curves that we see. I think the difficulty in trying to connect that, and that's what we said is the issue at this point, is that the lack of information of how salmon actually utilize the information, how we validate their use of the system so we can actually put bona fide parameter, numbers, boundaries about what we see. Until we have that, we can't really get to your answer.

Bartell: But it seems that filling in this matrix will be a concrete step in the direction of identifying what some of those curves might look like and how they might relate to a solutions base .

Casillas: Right. We do recognize the dilemma at the end of the road. And we're not clear from our perspective how we would talk to the region and tell them what we think when we look at these what we will actually say.

Courtney: Maybe I can interject and clarify where I think we're headed in the workshop. I think

the important words Ed has here are ‘important’ and ‘single.’ I think where you’re headed is a finite list of a small number of factors you want to see evaluated, and a recognition that those interact. And they’re not entirely independent. And if we can capture those two ideas, and I don’t believe I’m mischaracterizing it, I think what you’re saying is it’s more than just salinity; there are a few other things we want to consider, and we want to see how all those things interact.

Dunne: That’s somewhat more limited than taking into account the history, all that sort of stuff. That gets intractable real fast.

Casillas: Well, that serves as a basis that one would look at. Again, it’s the qualifiers we use to describe what’s going on, whether it’s a small change or a small period of time. How long should we encompass our evaluation? Where are the limits? Where do we stop looking? Those are things that we have to address in this evaluation. We know that we have some information from the late 1800s. We’ve done some of that evaluation, and there’s no reason why we shouldn’t use that to further our understanding. It’s not that we’re trying to create something new out of the blue here. We knew we were developing this. In fact, we did develop it and you saw some of the outputs. So there are some limits. Yes, we recognize that. But we’re basing it on information we’ve already developed. That can help set the boundaries of time you want to consider. How the policymakers will use that information, that will be up to them.

Eriksen: You were talking about response curves. I wonder if you can explain what those are.

Casillas: When we talked about depth and velocity criteria that we thought were important to salmon in relation to flow, in asking the question how did the system operate in some historical context, how did it operate now, and look at those curves and ask are they the same or are they different? If we say that what’s important to salmon is a certain velocity, and we reduce that ability relative to flow, obviously we say there’s a reduction in available habitat. As I said, the whole evaluation is a function of genotype, genetic, structure, the inputs in the system, the phenotypic expression of those animals -- how they interact with the environment -- and the availability of the habitat they use. So, if we remove one portion and see that the system is depressed, the question will be -- as Tom [*Dunne*] has alluded to -- how do we value that? That will be the difficult part. We agree, but at least we can define that and that will be more than what we have so far. And we recognize that that’s probably the limit of where we can go at this stage.

Bartell: Given that their model covered the entire system, do you mean that you’d like to see output from other locations in addition to the 29 places they looked at?

Casillas: The characterization that we have speaks to a focus in the periphery as opposed to the channel. The channel was really what they were trying to characterize, which they adequately stated that that’s where the biggest change is going to occur. But from our perspective, that’s not where the salmon are so we really want to look more precisely at what’s going on in the periphery.

Boesch: But I think the model made the case that the kinds of changes that are likely are much

less dramatic. Why do you need to refine the periphery?

Casillas: We're asking for a justification that it adequately resolves what's going on in the periphery. As you saw, the grid focused on the channel - that was their purpose. The grids were less robust in the periphery so we just need some characterization that the ability to characterize what's going on in the periphery was in fact adequate with what they did. That wasn't presented previously.

Dunne: But you wouldn't need a complicated model to reason out defensively that you'd expect the kinds of results they got in the periphery. Physical reasoning should tell you that they change in the periphery would be very small.

Casillas: That's right.

Dunne: So why do need to go refining that part of the model?

Casillas: We have stakeholders we need to respond to, and they're going to say did you do the best you could do in this evaluation?

Dunne: So stakeholder satisfaction is important? Instead of lengthening the studies, would it not be possible to have someone just explain to the stakeholders the physical reasonableness of that result?

Casillas: They've tried, and they've done that, but stakeholders can be very contentious. I think we want agreement by all parties -- by the agencies -- that in fact the best was done that could be done.

Boesch: Who defines the best science -- the stakeholders or the scientists?

Casillas: Science, presumably.

Dunne: So at some point you have to have a certain amount of trust in the science. There is good reason to believe that at the periphery, far away from the deepening, one should expect very little change. That's what their model says. You don't think that could be said credibly to stakeholders?

Bartell: What I think we're seeing is that there's a certain contingent of stakeholders that would only be satisfied if you construct a linked hydrodynamic, salinity, sediment transport model that would re-create the entire history of the evolution of the estuary to be used to characterize the impact of this proposed project.

Courtney: I'd like to interject. Part of what we do is get you to the point of facing up to the smell test. What I think I'm hearing from the panel is that it doesn't matter, we could do more modeling and would address your concerns, but we all know already what the results would be and that the effect would be trivial. Am I correct?

Dunne: But what about the stakeholders?

Courtney: It doesn't matter; that's not our goal. It's NMFS's concern. Our role is are we already at the point in the science to have confidence in what the results will be? And if we are, I think we should say so. And if you don't think it's enough to pass your smell test, we're here to back you up.

Casillas: And I'll give you the background for the statement why I think we can do better.

[Presentation continues]

Curtis: I have a question and a concern. If you're concerned about the modeling, it seems like you want to be sure you're modeling the right thing. I'm not at all convinced that salinity is a dominant factor in determining the success of these young salmonids in the estuary. Are you?

Casillas: No.

Curtis: I'm not either.. So it concerns me that we're spending a lot of time on the salinity model and we haven't got our eye on the ball. I'm worried about those shallow habitats, too, but rather than doing a dance, that's what we should be talking about. What do you need to define those shallow habitats?

Boesch: Well, if you're talking about altering the geomorphology of an estuary, the first-order question is how it would affect the salinity because that gives you indicators of a lot of other things.

Curtis: I understand. But is what they have on salinity enough? And accept that it's enough and say so. It's time to start talking about the things that we think are more directly involved with survival, or success, of these fish in the estuary.

[Presentation continues]

Tortorici: I just wanted to add one thing before we finish. This gets back to the discussion at the beginning of Ed's [*Casillas*] presentation regarding the stakeholder issue. From our agency's perspective, and I think you can appreciate this, that because these species are threatened and endangered, we have to take a really close look at projects, especially of this magnitude, in order to make a proper policy and regulatory decision. I think sometimes because we talk about these species every day and we say, "Oh, they're threatened and endangered," it sort of flies over our head that the implication is they could go extinct. And so we need to be very careful in terms of the science we used, the perspectives we take, and the interpretation that we make of that information. Quite honestly, our agency is in a fishbowl. Everything we do is scrutinized -- I just can't even begin to tell you -- to the nth degree. And so while on its surface it may be obvious that this change is insignificant and so why can't we just explain it to folks and surely they'll understand it, in this environment in the Pacific Northwest, it just does not work that way. And so

we need to be absolutely sure, to the best extent we can, that we've got the best science to make this decision. I say this to you all because we're not trying to nit pick here, and we're not trying to create extra work or problems or irritations for people. We're really trying to convince not only ourselves, but the other federal agencies and the public we serve that we've done the best possible job we can to do the analysis.

Dunne: Cathy, that doesn't necessarily mean that running a complex, numerical model on the basis of what I'm sure would be adequate data eventually because we just don't ever do it, that's not necessarily the application for the best science. And it's not the best way to get people who don't understand numerical models to believe you. There's another way of doing science that's all about understanding and conveying that understanding. I can tell you, it's a lot more tractable than arguing about mesh size and how to parameterize...

Tortorici: What we're trying to get to is a point where we're comfortable from a scientific perspective that we've got the pieces that we need in place to make the proper decision. I agree with you -- talking about mesh size and all that stuff to the general public is not relevant to them. It's trying to explain that in a biological context about why we're making the decision we are, and translating that into something folks can understand. That's where the rubber really hits the road. But we're trying to get the underpinnings straight in order to make the decision to have the proper explanation from both a scientific and a legal perspective.

Courtney: This is a scientific process, the process we're engaged in here. You are presenting your cases, information to six eminent scientists who are going to give you their opinions. You can change the models on some of the things you're talking about, and it won't change the results. I think that's what I've heard. If that's the case, then that's a scientific opinion, which you must respond to.

Boesch: Just listening to Ed's presentation and the critique of the salinity model, I agree that the focus on salinity is a response variable which, in terms of affecting the salmon alone, is a very uni-dimensional analysis when, in reality, these organisms are responding to a lot of other variables. However, I think the value of this kind of modeling, and I guess we should reserve judgment fully until we hear about Antonio's model tomorrow, in an estuary is that the distribution of salinity tells you a lot more than just what that salinity is at that point and what effect it might have on the organism. Because it is based fundamentally on the conservation of mass, and so it tells you a lot about the changes in the water masses that are taking place. So assuming that Rob's [McAdory] models are sound, I would think of the list you have up there, for the estuarine portion -- that is the brackish portion, the wide part of the estuary -- I would say based on first principles, in terms of understanding how the physics of these systems work, is that number one the salinity itself would not be substantially altered by the plan in the peripheral areas, and also that the velocities experienced by the tidal cycle would not be greatly altered because otherwise if they were, you would be seeing some pattern changes in salinity, and therefore, temperature as well.

The one point you did make that I think would be worthwhile to look at, and I'd be interested to hear Rob's comments, is that the area above the head of the salt wedge, that narrow part of the freshwater tidal estuary, that alteration of the channel might be much more significant in terms of

the hydrodynamics of that part of the system. And it could well affect velocities, even in the shallow areas. It could affect water level even. On many of the parameters you listed -- water level, salinity, temperature, and flow -- I would think that you could draw some strong inferences based upon that model about the relatively small degree of change that would be caused in the peripheral areas by channel deepening. So I think you can say things -- more than just about salinity from the model.

Courtney: I think there are lots of things Ed raised some of which I think would be useful to go through in perhaps more detail. We're slated to get panel opinion, and I think those would be a good place to start. We've begun to talk about the adequacy of the model vs. the completeness of the model. There's the issue of the peripheral zones vs. the channel. I think we had a very rich series of comments, and I think it would be useful to run through them and maybe seek some comment on those. Would it be useful to look at the list of NMFS's concerns?

Baptista: If I may add, Tom, I agree with your questions, 'Can we explain this reasonably to stakeholders?' with one exception. There is something peculiar about Cathlamet Bay, and based on that, I want you to consider whether there's reasonable doubt on whether there needs to be some more investment of time in this particular process.

Dunne: If I understand what Rob said, what's different about that bay is that it has a deep hole channel in it that's connected to the channel to be deepened.

Baptista: A couple of things are different. That's one of them. But the other is that it's a very sensitive zone relative to the limit of propagation of the ocean influence. So there are two things happening in that bay that are different. There are enough things we have done, quite independent of channel deepening, that are worth thinking about.

Goldman: There was one comment about evaluating previous dredging. Do you have that data?

Casillas: That was one of my issues that we would like to see. I don't have that.

Beosch: Is that available in the EIS?

Eriksen: It's not addressed in the current EIS.

McAdory: There's getting to be some data sets about the salinity, like the ones Antonio's getting, I'm not familiar with those in detail, but to the extent they exist, these long-time series of salinity, particularly around the surface and edge, where things are important, but anywhere for that matter, it would be an interesting exercise to see if you could find the signal of the dredging activity in that. After doing some multivariate analysis and taking out the tide and the flows and things like that, can you see that signal in there because they do deepen this thing to these amounts every year or so, and there could be a lot of problems with that. Can you find a signal of the dredging anywhere in these salinity measurements that have been taken for years. Can you see a little change, up or down, that have been associated with that action?

Courtney: I'd like to attempt to reach some resolutions, or at least get your opinions. I'm not trying to cut off discussion... I've put up what I think is the first of the series of overheads where Ed laid out NMFS' concerns. I'd like to run through those with you and hear your responses. The first one, I think speaks to the need to have broader conditions, more information, a broader range of models. What I heard from Ed is that NMFS doesn't have any fundamental concern about the model structure itself. Is that correct? It's just a question of how it's applied. NMFS feels they'd like to see a broader range of conditions because presumably that would tell them sensitivity to other conditions. We've begun to hear a suggestion from the panelists about what they think about that. If we were to ask the modelers to go away and run annual variations, have a broader range of conditions, what would be the utility of such an exercise?

Boesch: I think it would be sort of pointless at high flow. The question is whether you should experiment at some slightly higher, or the extreme low of, flows. That would constrain the exercise.

Goldman: That's my comment that aquatic organisms typically respond to worse conditions and very low conditions in the [San Francisco] Bay Delta system. And I would think that running the model at the minimum flows that are expected historically, or may occur again despite the dams ameliorating those flows to some degree, if you have a drought, you're going to run out of water. Your flows are going to get lower and the organisms are going to be a lot more stressed by this than they are by running averages. I think extremes are really important for the organism. You can lose a whole year class sometimes by some environmental extreme.

Courtney: So what I hear is there is a comment on the fourth of these bullets, which is what are the most biologically relevant changes and you're saying - look at the extremes and that will tell you your worst-case scenario.

Goldman: Correct.

Boesch: The low-flow extreme. I wouldn't say the high-flow extreme because arguably you couldn't make a good case they would be altered ... the project from high flow... periodically.

Bartell: I'm not sure I agree with your initial premise about no problems with the model. At least not according to the document we were given that reviewed the model. I don't mean to put you on the spot Rob, but I'm sure you've seen these comments before, from David Jay et al. Things that need to be addressed include difficulties with the numerical mixing algorithm, potential problems with different bathymetry data, limited horizontal and vertical resolution -- again this goes back to scaling. Not as a point of criticism, just to say that these issues are out there, and I don't know whether they've been addressed or resolved. So the assumption that the model has been generally accepted in terms of its overall structure and operations is not exactly correct.

Tortorici: The issue is that what Ed was trying to explain in the construct of how the model was used, fine.

McAdory: Not everybody likes my work, but I feel good about it, and the more I look at it the

better I feel about it.

Baptista: One thing I want to mention is that in my opinion, the work that Charlie [*Berger*] and Rob [*McAdory*] did, given the constraints they had in terms of time and resources and so forth, was quite good. Whether that would be the work that they would like for any model to use in terms of a basis for a regional study, that's a different question.

Bartell: Don't misunderstand me, I think these guys are among the best in the world. Typically, the people that build and operate the models are the ones that are least satisfied with their performance. They're also the ones who have the best understanding of why they work the way they do and of their strengths and limitations. I was just curious in view of NMFS' concerns about model performance what kind of an effort may be under taken in terms of developing some kind of a consensus between the different modeling approaches.

Boesch: So the question remains is the modeling good enough for the objectives. Cathy pointed out, 'Don't think only about salinity objectives.' You have to ask that question, 'Is it good enough?' rather than, 'Is it as good as it can be?' Because that may be something everyone would like to achieve, but it may not be necessary to answer these questions.

Courtney: Let me press that point a little further. I think, taking your first point up here Ed, that you've got a qualified yes from the panelists that it probably would be worth looking at some broader questions, but not every condition. In fact, a very specific condition -- that is, low flow. Charles is nodding; I'm looking for you guys to record your opinions here.

Dunne: Actually, I'm surprised the Corps didn't smooth their own path and do the low-flow...

McAdory: Well, we did. The group thought it was inconceivable that flows would be at 120. They were thinking 134. When I went down to 120, I thought I was in fairy land, but I thought I'm going to cut it by 10 percent and see. So I don't know if that's true or not, but if you take all the dams out, who knows how low it will go. But as long as those dams are there, and they're required to have the flows at a certain level, they felt we would be wasting our time going there.

Boesch: Under the current flow regulation regime, what's the frequency that flow would be less than 120?

Baptista: I'll show some graphs tomorrow that may help.

Casillas: 70 at the Dalles for a month, I think.

Berger: But that's modulated by the time it gets to the estuary.

McAdory: Modeling a lower flow would be easy. The lower the flow, the easier it is to model because the higher the flow, the more likely you are to have instabilities from those high velocities.

Baptista: For a very different type of change, much more substantial than what we are talking about here, I will show tomorrow some results that indicate that changes for the higher range of river discharges may actually be more significant for the river than for the lower range of river discharges. Again, I'm not a biologist. I'm a physicist, interpreting data and putting it in a context that fisheries researchers seem to understand.

Bartell: Rob, you keep calling this a screening calculation. What would you do to the model to take it beyond that characterization?

McAdory: I'd want to verify it against a large data set. I don't know if there's one thing you could do; the more data you compare it to, the more time you take to run it, and the more detailed it is. Things like that. There does come a point when you feel like to answer whatever questions you have, you're wasting your clients' money. But for the purposes for which it was designed, I feel it adequately answers the questions. I'm not sure I'd do anything to it. We're just trying to understand what the order of magnitude of salinity changes are coming up due to this deepening, and then let a biologist tell whether that's important or not.

Bartell: How deep would the channel have to be before you think could lighten up the system in terms of salinity?

Berger: By the way, I don't know what the actual depths were run here, but it was more than the three-foot deepening.

McAdory: Yeah, it was eight feet because we assumed there was going to be maintenance. I don't know, if you made it deep enough, you might make it very hard for that salinity to get out at all.

Berger: At some point, you're getting pretty far away from verification, and you're putting a lot of confidence in whatever turbulence model you've got behind this thing. Well, of course, it gets turned off when you stratify it. If you get away from verification, I think you can get in trouble.

McAdory: In this case, if you really look at the places where dredging is proposed and how much different that is from where they are, especially after they do maintenance dredging now, you're talking about an extremely small change to the cross-section of the system to all aspects of the system. I don't think anybody expects large salinity changes in the boundaries for these minor changes, given that everything else is the same. However, we're perfectly willing to be surprised and we do find some surprising results occasionally. For instance, your question instead of making it deeper had been make it a lot wider, then Charlie [*Berger*] would've given you some another answer from something we learned recently. So, you start off with some idea of where you're going, but you don't always get where you think you're going. So, I think the results are reasonable, and if we're still talking about whether we should invest more in the salinity order of magnitudes, I'm not sure I'd do it a whole lot differently.

Courtney: Still looking for resolution, I'd like to pose a question to the fish biologist on the panel. Is there anything you would disagree with with NMFS's concern essentially that the periphery of the estuary is really where we should be most concerned?

Quinn: I think it's basically correct. First, I want to step back and say that I appreciate Cathy's comments about their [NMFS'] responsibility because it's a very heavy one they feel, and if they don't carry it, nobody else will. I also think Ed makes a good point that is that it is unreasonable to salinity, how important is that, and we'll write that off. Temperature, how important is that, and we'll write that off. Depth and velocity. Because, in fact, the animals use a habitat space that's defined by a number of those things. To reduce them to simply to one at a time doesn't really do justice to the behavior that the fish have. I think it's a valid point to bear in mind, particularly if there are also interactions with other stressors, pathological or something like that.

It seems to me that the important considerations of the whole estuary are probably on the edges, for the life history stages that use the edges. Those populations that are relatively large when they come down seem to, generally speaking, trundle through fairly quickly. Those that come down smaller spend more time at the edges and -- look around, what have we done? We've basically screwed up the edges, whether it's diking and docks. All kinds of things have been hacking away primarily at the edges. Until recently, people weren't even talking about chum salmon because they weren't counted at Bonneville Dam because there were heaps and heaps of them. And probably a lot of their losses have been estuarine losses, and also the kind of low-gradient rivers that flow into those areas. I think those are the right places to look, and it's reasonable to look at interactive kinds of things. It's real hard for me to see salinity per se being an issue, but in the context of other things, maybe it could be. So my feeling is not to minimize it, but to try and get it into a context where it's brought in with other things that will have a connection to growth, survival, migration, which are the main three response variables we're likely to pick up on.

Boesch: I guess the key questions you might argue about are the third and fourth bullets. That is, whether the model can tell you some things about the potential for changes in the periphery of all those other variables, or whether those inferences are not justified. Or a justification has not yet been provided. In other words, given the kinds of dynamic changes Rob showed us, what kinds of changes would occur in terms of the altered hydrodynamics of that system to the peripheral environments that could make a difference? How would you come up with a plausible explanation. I'm saying that if you buy the adequacy of the model for predicting salinity changes, I think you have to buy the adequacy of the model for predicting salinity changes in the periphery as well. Because there's no other way to get the salinity there. It's all a connected system. So, in terms of the adequacy of the model, in terms of predicting the peripheral situation vis-à-vis salinity, my confidence hasn't been shaken by the debate.

Now whether you can infer from that that salinity isn't changed, therefore, water level, current velocities are not also changed, we can debate that, but I think we can start by drawing some inferences that if the salinity isn't changed, therefore the general patterns of circulation and movement of water aren't changed, so why would water level and current velocities be changed?

Eriksen: A lot of it has to do with the scale of the deepening versus the overall cross section of the river.

Goldman: Right. You take it down three feet and its -- what -- 60 feet wide?

Eriksen: Well, the channel's 600 feet wide, and there will only be a few places where we dig the whole width, usually one side or the other is deeper already. In the estuary, we're four miles wide. Even if you look at flood flows in the estuary, the tides drive water surfaces in the estuary much more than they do upstream. We did do a one-dimensional model for water surface elevations, and we didn't really find any changes until we got upstream of Longview and Klamath. As we got near Portland, we had, I think it was, 12 hundredths to 18 hundredths of a foot reduction in water surface. Again, that is reflective of the scale of the dredging as opposed to the overall scale of the process.

Boesch: Having said that... I'm trying to reason aloud... I think the process among the agencies could be better informed by sitting down and specifically identifying those parameters that NMFS is concerned might be changed as a result of the channel deepening. Then examine the evidence, have a discussion, have a debate, and then come to some specific resolution rather than a general view that we don't understand the periphery well enough. Well, what is it about the periphery in terms of those key environmental characteristics that we have less than adequate confidence about? And how can I convince you that that won't be affected, or alternatively, what further analysis can we do to narrow that uncertainty?

Courtney: I'd like to point your attention to the third bullet, which is what sort of variation should we look at? High flow, low flow... Should we also try to see that in terms of annual variation? Are there other things we need to consider? Temperature of the water coming down? How would we try to get a handle on looking at several factors at once. There are well-trying techniques of sensitivity analysis which do exactly that. And partition variance in terms of inter-annual, or daily, or seasonal, etc., tidal variation - would that be a useful exercise? And where one of the parameters would be channel deepening or not? That would then give you a context in which to understand how much of the variance would be associated with the channel versus what I've heard from Rob's and Karl's presentations, which is that it's a highly variable system inherently.

Dunne: I think the pattern is more responsive to flow and to topography than it is to anything else - your exercise will not tell us anything we don't already know. What we're still not getting our heads around, and I don't know whether it's possible, is that the biologists would say, 'Look, if you could predict patterns of temperature and salinity through that estuary at the following range of flows (and, by the way, the low flow can easily be checked by going to the USGS web site)... Suppose for every month of the year, you've got predictions of temperature, salinity, etc. If you did that, you would know where all the flies are living, etc., and then you could say, 'Here's a kind of time series of flow in the Columbia, including its extremes, and here's how this project changes the volume of habitat, for example. Again, it requires the biologists to say, 'This is what these critters need.' It seems to me you'd get more answers out of direct modeling like that than you would by running a sensitivity analysis. I think we probably know what the answers are most likely to be sensitive to, and then when we know that, I don't think we're going to do anything about it. I would say that you want to be concerned about the effect of the channel averaged over some significant amount of time. That would take into account whether you lose a year-class, have a bad energy crisis, or low flow, and so on. We need to simulate a range of discharges that take into account these rare events. You want to do it for the purpose of answering the questions,

where do the bugs live, and where can the fish survive, and so on. Not just isolate the sensitivity to a variant.

Bartell: We've seen some really good results on a short time scale. If we do this project, is there some mechanism to show how it might impact flows and sediment dynamics that will, in the long run, maybe 50-100 years, contribute to changing the geomorphology of the system to an extent that patterns of salinity do change dramatically, or it does have an impact on the periphery of the system. I don't know how to answer those questions because there are so many other things going on in the system besides the project.

Dunne: Again, in the lowered sediment transport regime, with all those dams up there, it's hard to imagine you're going to get.... I raised that issue earlier -- maybe one of the things to model is what the impact on salinity would be if that northern channel shuts off, gradually over time. But it was hard for me to push that very strongly because sediment transport has been significantly reduced than any changes in the bathymetry of the estuary presumably now going on quite slowly relative to how they used to go on.

Eriksen: Along those lines, I think Don [*Boesch*] pointed out, if you don't see a change in salinity, you can infer there's little change in velocity and flow distributions, flow patterns. And if those changes are small, the related changes in sedimentation, water temperature are also going to be small, undetectable. You can infer a lot from the salinity model and that's why we concentrated on it. We did look at water surfaces upstream with a different model, and there again, we're looking at a two-tenths of a foot change in water surface. It's about one-quarter mile wide, 500 square feet, or something, out of 100,000. So there's your range and sedimentation is not that accurate. To go and invest in a sediment transport model that you think is going to give you answers to that change. You can model the system if you're concerned about the system and you're looking to see how it operates from a sediment standpoint, but to go back and model that system and model that change from a one percent change of hydraulics, it's just not going to happen.

Courtney: I'd like to characterize some of the things I think I've heard the panel say. You seem to be saying that you agree that it's more than just salinity. We want to know how does it translate into salmon habitat attributes. And when we know that, we can give you more information about what's the appropriate level of modeling that might be useful. I'd like to give each member of the panel the opportunity to record their opinions and thoughts on the modeling.

Goldman: No level of statistical sophistication in modeling can compensate for having missed the most important variable. I think it's terribly important as we get into the fisheries part to try to figure out what's important to the salmonids-- particularly to the chinooks in the long-residence time in the estuary. I feel sorry for the NMFS people in the sense that they're faced with a declining fishery, and if the fishery continues to decline with this dredging approved, the public might conclude that the dredging was responsible, when in fact it's the dams and all the other perturbations to the population. So they're in a real difficult position.

The other problem is that I found in the 40 years I've worked at Tahoe is that the smartest thing

we've done is kept it simple for the public. 'Keep Tahoe Blue.' Everyone understands that and it works. In a sense, I think as we market whatever the decision of this overall group is about dredging or non-dredging, they have to make it palatable and understandable to the public.

The other point that I made earlier is that aquatic systems are subject to extremes. The low oxygen is what kills off the fish. It may occur only once every four years in a hyper-eutrophic situation. But most of the situations that really make a difference to the aquatic organisms are the whole food chain. So we have to think about extremes. We have to think about the lowest possible flows that might be created by the drought and the power shortage in terms of coming to a resolution of this.

Day 2

Antonio Baptista **Alternative Approach for Modeling**

Goldman: This was a very impressive presentation. But I'm struck by the fact that you're modeling is really driven by sampling hardware, what you can measure remotely, continuously record...

Baptista: The modeling is not drive by that. The quality of the modeling is controlled by that. We look at the data to have a sense of how well we are doing. But when the data go off, we continue to model.

Goldman: Okay. And it would be so useful if you were able to add just one parameter of real biological importance, like chlorophyll. I don't know if you know about the ROS units, Remote Operating Sampling units. Are your units intact and you go and download them.

Baptista: Actually telemetry, so it's real time.

Goldman: So you could actually include another sensor.

Baptista: We can include any sensor that we can stick in the water. The problem with biological sensors is fouling issues. A lot of our maintenance in the field comes from fouling.

Goldman: I'm well aware of that. The ROS units you can actually park in zones where fouling is less of a problem because they run up and down the wire. But it seems to me that this would just increase the power and usefulness if you could include a biological parameter like chlorophyll for instance, or like turbidity, which is still another physical parameter but which has a lot of biological implications like salinity.

Baptista: We actually have had some sensors in the system. We are putting some now in Cathlamet Bay, which is an area where the fouling should be a smaller problem.

Goldman: The other thing is that if you have duplicate endware, you can just change these, but it

does mean that somebody's got to go out once a week...

Baptista. I didn't mention this, but I have a field station in Astoria, and I have permanent stuff there. So they are in the water 3 out of 5 days a week or maybe more. Now every time you have to go and send a diver to change an instrument, it's expensive. So we have to try to balance what's feasible.

Boesch: Antonio, the visualizations were striking. I got the impression looking at them, maybe a misimpression, that the ocean dynamics that affect the plume shape somehow have an implication of translating back to dynamics in the estuary. If that's the case, what kind of limitations would that provide to the kinds of models that Rob talked about?

Baptista: The indications are not negligible, but it depends on the specifics of the boundary conditions. I think I would've done the same as Rob. He imposed a salinity of 33 part per thousand, which is quite reasonable. And he imposed one tidal level in the ocean boundary. Those are things that you can play with and try to minimize the lack of the bigger system. I believe that the bigger system needs to be there for CORIE; I don't actually believe that for a three-month study, or whatever the length of the study was, that you can afford to put those things in there. There would be a difference in the results -- change of salt, etc.

DePinto: When you say there's a difference in the results, I agree there would be a difference in the absolute concentration, but would there be a difference in the change pre- and post-channel deepening?

Baptista: That's a key point. My sense is that there are other things in the modeling approach that would probably affect the change before that comes into play. For instance, wetting and drying would be one of those and perhaps the six levels that we're using will affect that before you come to the point where the ocean conditions affect the change.

Boesch: You emphasized that it isn't necessarily the low flow that we should be concerned about because the models that you ran from the 1979 and 1890 differences in terms of response of the different flow regimes showed that the habitat opportunities under low flow conditions in the estuary really haven't changed that much. Considering even with rather types of dramatic types of morphological changes compared to the one we're thinking about now. So that if you buy that, additional morphological changes are unlikely to affect the habitat opportunities under low-flow conditions. It was only in the case of Cathlamet Bay and upstream with higher flow conditions that habitat opportunity was affected over the century.

The other thing I noticed with respect to the discussion we had yesterday about what is the low-flow condition that should be tested. Rob's [McAdory] point is 120 is as low as anyone could imagine. But your 2001 data are about that. So, on a kind of a time scale, that is weeks to months, that is going to be meaningful to the estuary. The impression I'm given, based upon those two observations -- both the first one that historically low-flow habitat opportunities have not been altered very much, and secondly that the low flows sort of averaged over the period of weeks to months during this year, which is a very low-flow period, were not lower than the 120

cfs. It seems to me pointless to do exercises at flows lower than 120.

Baptista: I don't disagree with that, but it depends on what you're trying to do.

Curtis: I think one difference though is that right now the reservoirs are down about 30 or 40 feet from where they would be on a normal year because they've been spilling a lot for hydro. I'm not a hydrologist, but I think we're going to have a lot lower flows this year than 120 when we get to the end of the summer.

McAdory: What you really want to look at is August-September.

Baptista: The other thing is what is the critical period in which you are trying to understand salmon survival. If the species you're looking at is primarily is a summer species, then that type of flow is what you should be thinking about.

Quinn: With respect to the depth criteria that they had, what's the general effect of variation in river discharge? At higher discharges, there is a smaller time period with the appropriate criteria. Because if I read the graph correctly, at higher discharges you have a smaller number of hours with the appropriate velocity criteria in the places where there was either no effect or downward. Is that correct for the depth as well?

Baptista: Yes. However, the reason I always try to deflect from that is that I don't believe that the bathymetry representation in Cathlamet Bay that was used in these studies was appropriate.

Quinn: I don't mean specifically Cathlamet Bay, but in general.

Baptista: The trend is similar. The difference that is striking is that the habitat opportunity doesn't go down from pre-development to now in those graphs - actually it goes the other way.

Quinn: I'm not actually worrying about pre-development; I'm just thinking about year-to-year with more or less what we have now.

Baptista: You still separate zones. The same way you separate with velocity.

Quinn: If you have a higher river discharge from one year to the next with the current channel and islands and so forth, does the time window of that depth criteria shrink, stay the same, or grow? Velocity, you've already said, goes down.

Baptista: I'll have to look back at the graphs, which I can do.

Quinn: I can intuitively visualize that. I'm just asking what the answer is. Otherwise, I'm left to conclude that the more water going down the river the worse things are. It's a darn good thing we built all the dams to save the fish from the river [laughter]

Baptista: The reason I'm hesitating with my answer is that I don't believe in the depth-criteria

results, given the uncertainties in the bathymetry. So in a complex system like this, I try to shy away from a model result I don't believe in.

Quinn: Okay. You see what I'm getting at though. It's a huge magnitude of river discharge. I'm just trying to gauge the sense of the effects if they're positive, negative, or neutral against which we can gauge the effects to be associated with something projected.

Boesch: The other way to think of that though, Tom, is in response to yesterday's discussion with Rob and Karl, in that, based on the model at least in the simple context of the change in the cross-sectional area in that part of the estuary on the basis of channel deepening, that the water level is not affected by channel deepening so that it's hard to imagine how the area time of depth inundation will be changed by the result of that.

Quinn: Okay. But if you're trying to model the effects on the fish, you have a certain background that's imposed by year-to-year variation in discharge as well as tide and so forth. Where does all of this fit in.

Goldman: Well, one thing I think that's quite obvious is that high flows have positive and negative effects. For one thing, you flush the food out of the estuary fairly rapidly. Phytoplankton doesn't have time to produce and divide and feed zooplankton and young salmon. At the same time, low flows can have negative effects from possibly low oxygen concentrations, concentration of predators with the prey. So moderation in all things -- maybe average flows are probably the best.

DePinto: One thing I think we need to remember in this whole discussion is that this analysis of habitat opportunity that was done here was done with a two-dimensional model. So the only criteria they could look at was velocity or depth. The issue of opportunity vs. flow conditions for salinity might be very different, and you can only do that by applying a three-dimensional model.

McAdory: Also the depth-average velocities in a stratified system are problematic because you could have large velocities going opposite directions in the water column, yet they average to zero, so it has its limitations.

Baptista: It's true that the depth-average model... The only useful of this exercise was to start thinking about how to interpret this process. So this is the behavior for habitat opportunity based on the depth criteria. And you still separate the zones, but you have a much flatter behavior -- in some cases, it's a reverse behavior. This is the riverine zone. Those are Cathlamet Bay and Gray's Bay. Those are the two ocean bays -- Young's Bay and Baker Bay. So there's a fundamental different change in behavior, how it relates to river discharge.

Bartell: Would it not be possible to go back and analyze the results of Rob's [*McAdory*] model in much the same way that you're doing this, using the same regional definition to look at plan and without-plan comparisons, and then certainly the argument falls back as to the credibility of those model predictions? Alternatively, you could do the same with your model.

Baptista: Yes, but you require a range of discharges, of controlling forcings, to do it. You can't do it just based on low-river discharges.

Bartell: Well, granted. But at least they can look at the two data points they have -- 134 and 120...

Baptista: That's too close in one of the ends of this spectrum. Remember this goes from the 100's to the 600's/500's.

McAdory: When you increase the flows, you increase the stratification. But, you move the whole thing downstream to where it's not relevant, it's less relevant to a place where the low... They're two balancing things there... If you get 600cfs, I'd bet you could drink the water at Astoria...

Baptista: By looking at these types of things, you can compare a reference situation against whatever changed. That's true for pre-development vs. modern, and it's true to some extent for base vs. plan. Whether you can see a significant difference or not that's something to be looked at. But yes, absolutely.

Berger: If you want to look at base vs. plan, then you could look at something like this, but you need to know what criteria you're looking at. I say this as a caution: numerical models are not reality.

Baptista: Yes, it's important to keep in mind that these things are models. They are a basis for you to think about the system. That's it. Don't push it any further because we can't give it to you.

Bartell: Yes, but coming back... The channel deepening would fundamentally alter the governing equations of determining how your system performs. And so whether or not you believe that 30cm is actually 30cm per second, you can still look at the habitat opportunity with or without the plan. And then argue what's the necessary level of resolution and calibration and evaluation I need to decide whether or not I believe that and use that information in the decision-making process.

Baptista: And that's in effect what you guys [*McAdory/Berger*] have done with salinity. Right? You have a base and you have a change.

BREAK

Casillas: Do I think we need more spatial resolution than was done? The answer is yes. And more emphasis on the periphery, as we said. And, as Antonio showed, if the simple divisions that he made that were based on the CREDDP study point to that the estuary responds differently to various forcings, and so at least that seems to be a starting point to make those divisions. We had talked in our discussions that maybe if there's another set of even further divisions that we want to make -- we haven't looked at that yet -- but that's something we may want to consider. If the time frame allows this, I don't know.

McAdory: With regard to the periphery, in my opinion, there's a small effect on salinity. Whether

or not that has any effect on the organisms, someone else will have to determine. I think the spatial resolution was fine. I had nothing to do with the biological interpretations of the model.

Eriksen: I support Rob as far as being satisfied with the study we did. I think one thing Antonio's analysis showed is that the only area of the estuary that really showed any significant change to any physical changes that have happened since 1880, was the Cathlamet Bay area. The lower bays and the central estuary didn't show velocity, or depth-wise, and real changes over that 120-year period. So I don't think there would be much point in putting a lot of effort into those areas and refine them any more than we have to measure the effect of a three-foot deepening on the existing conditions. If there are some questions about Cathlamet Bay, that maybe is an area where we could pick up a little more detail on the model. But I can't see going beyond that.

Boesch: Would interpreting the velocity data that were generated by Rob's [*McAdory*] model be adequate to address the question of whether velocities would be changed in an area that you showed was sensitive to long-term modification, in terms of habitat opportunities.

Curtis: I guess the question I have is the same one as yesterday. Any modeling that is done I hope it relates to what the fish need. Maybe not a quantitative, but a qualitative, understanding of how that model would help people understand whether we are going to change quality of habitat -- not favorable habitat, but maybe critical habitat -- for the young salmon. You talked about depth and velocity. You talked about salinity. It's not clear to me that salinity directly relates, but is maybe a surrogate that we know how to model and it may reflect other physical parameters. But I worry about things like food availability and cover, etc. So if a lot of modeling were to be done, my first question would be, 'How are we going to use it in evaluating -- you know, is it going to change the amount of habitat that's available?'

DePinto: I think it's an excellent point. My suggestion is that we ought to identify habitat that's critical with regard to those biological considerations. And then look at how the things we can model might change within that area, pre- and post-implementation of the plan. And then the biologists could potentially make their inferences about if the salinity is changing by so much, then that might mean that other things within that area might change. So it's a way to use the model in a comparative fashion while factoring in the critical habitat and biological considerations. I'd be interested in hearing what the panel thinks about that.

Boesch: Well, I think it follows on. Antonio's presentation talked about you're limited in terms of the kinds of physical parameters models can predict, and I think he did a good job of trying to indicate how you could use that to at least get some dimensions of the habitat value with respect to the habitat opportunity criteria that were discussed -- velocity, depth, and salinity. So I think to answer your [*DePinto*] question, that's basically what you could do. That more complex issues of biological performance based on the physics is beyond what the models can do. So the question is whether you do that on a more detailed level to satisfy Ed's [*Casillas*] concern that the margins and the shallow areas in Cathlamet Bay might not be well represented in the model. But, I don't think it's a simple matter to go in and put more detail in Rob's [*McAdory*] model. So, I don't know how you're going to get there in a reasonable time frame.

DePinto: But if you did identify an area in Cathlamet Bay that you felt was an important habitat, the question I guess is are we confident enough in the output of the WES model in that region to be able to say whether or not the things we can model will change pre- and post-implementation of the plan? To me that's the only way at this point, in a reasonable length of time, that we're going to be able to factor in what we understand about the biology of the system.

Boesch: I think Antonio tried to address your question about whether the model's adequate at that level of detail. He said he had some concerns about it because of the wetting and drying. The tidal channels that come into this part of the bay affect the hydrodynamics. So in the context of base vs. plan, could you tell us qualitatively or verbally how channel deepening might affect that exchange.. Thinking about it in terms of habitat opportunities for the fish, we've already seen that in the deeper channels, Rob's [*McAdory*] predictions of the delta due to the plan are relatively small -- that's sort of the extreme because that's down to the saltiest, most dense, part of the system. So that the change in salinity of water that actually enters the shallow part of the system has to be less.

McAdory: That main channel that goes to the right there -- and I pointed out that it was maybe the only place where you saw any influence in there and it was less than what was in the main channel. And that other channel is not that deep, and so I would estimate that it would have less effect than the deeper one.

Baptista: It depends on what habitat you're looking at. There's more than one access point.

McAdory: Any model's going to stop at some level of detail. So you have to make a judgment where to stop.

Courtney: Let me see if I can draw NMFS back in. I've heard some opinion saying yes, we'd like more resolution, more detail in the periphery. All the discussion I've heard frankly is in one area of the estuary. Let's suggest if WES did more modeling in this area, will that be useful, adequate, a means to go forward?

Tortorici: The only concern I have with that [the WES model], is that I'm not sure it's broad enough in terms of what it was really intended to do to get us to where we need to be. Do we want to go back and look at additional side-channel habitat? Sure. Which model we're going to use I think is still in play.

Boesch: What do you mean by not broad enough?

Tortorici: Well, when you look at Antonio's model, it just seems like it's a little more robust in terms of the type of inputs and what it can handle. From a regional perspective, that's what we've been talking about all along in terms of taking more of an ecosystem perspective.

Casillas: As we look at other measures of habitat opportunity beyond salinity, and that was the issue that I posed that salinity can be a surrogate for other things. So we looked at velocity,

temperature, and depth of available habitat. And I would add in terms of area Steve's [Courtney] question would be only Cathlamet in our evaluation. Cathlamet is an important transition zone-- that's why we focused on it. But the other part that I've tried to emphasize is that that's not the only place of concern. The effect is through the whole area, from Portland down through the mouth. So we really want some evaluation that incorporates that, or some discussion that says that that impact really reflects all of that and a justification that the discussion we have in this area is representative in the entire region and is representative for these reasons. Antonio's model shows that Cathlamet Bay does operated differently than other parts, but it also shows that the mainstem portion, our region 6, also operates quite differently. And so we would want the extension to go beyond just Cathlamet...

McAdory: Operates quite differently from what?

Casillas: In relation to flow, from the historic to the present.

Boesch: So you think the WES model gives you an adequate representation of the changes in depth in the estuary? Is that one of the habitat criteria?

Casillas: I don't know because I haven't seen that information.

Eriksen: We can do water surface, depth, and velocity.

Boesch: Okay. Temperature?

McAdory: We didn't do temperature.

Boesch: You can't do temperature.

McAdory: We can, but we didn't.

Eriksen: We'd want to discuss whether you have enough information to model temperature.

Boesch: But how about velocity? You could look at changes in velocity from plan to action, you could do that?

Eriksen/McAdory: Yes.

Boesch: So it seems to me that until you see those products... You have to see the products in order to make the conclusion about whether they're adequate or not.

Casillas: And the difficulty is integrating all that, looking at it first independently and then together as you compile the information.

Boesch: It seems to me there are some interpretations and uses of the existing model that could at least be brought forward to deal with NMFS's concerns about the adequacy of the information.

You might judge them inadequate in the end, but until you do that... I think based on Antonio's presentation, those are the physical factors you can model with some degree of reliability that have some ecological importance.

Casillas: In accepting, or looking, at that information, Antonio had a model comparison that's something we would like to look at as well, or at least consider, rather than try to verify the existing model to get some reliability, and we had proposed some model comparison outputs to get some confidence that the outputs were seeing from that model are applicable.

Courtney: The issue of adequacy of physical models. I would like some further discussion of the fact that yesterday it seemed to me we had agreement that the WES model was adequate to the task. You hadn't seen the outputs you might like, but as a model in and of itself, and I think that's where Don's [*Boesch*] going, there's no evidence to suggest that it wasn't adequate to the task. I think Antonio said the same thing this morning. I think we should separate model identity and structure from 'did you get in the past the required output that you wanted?' To the extent the models can give you things you want, that's a separate issue.

Boesch: I'd just like to respond to Ed's comment about comparing the models. I agree, it's a very important thing to do because, if nothing else, you're going to learn something about how the system works by looking at the differences and how they might be resolved. The other thing I think is important in that regard, though, is keep in mind the issue at hand. So I think you might want to examine the difference in the performance of the models from the viewpoint of would they make a difference in looking at base- vs. action-type of scenarios. So there may some significant differences in the models, in the outputs, that you say, 'Well, we need to understand and resolve these.' But, they may not make a difference in terms of the base- vs. action-type of applications. Whenever you do those comparisons, then, you have to do them from that context.

Casillas: I agree with that. But one of the points I tried to make yesterday is that the context is an issue for us as well in terms of temporal context of how we make our comparisons. If we look at how things operated over time with respect to issues relative to salmon, their survival and completion of their life cycle, even though the base-to-plan is the period we have at hand, we want to somehow frame it in the context of how the system has operated over the time frame for which we think we have some reliable information, and keep that in perspective, as well. How we use that information in decision-making, that's something that remains to be decided. We've opted for this long temporal scale and then put it in the context where we are now and in the future. The issue is if we don't see changes in the future that we think are important in this base-to-plan change right now, but we see that the system operated quite differently 100 years before, the question becomes are the models able to resolve differences physically that are important to salmon that see biologically. That will be an issue we will have to deal with. I don't have an answer for that, that's just the issue that we have. Because we can't resolve the differences from base to plan existing to the future, doesn't mean that the salmon aren't going to be responding to something that we can't define.

Boesch: What time horizon do you have in mind for the future?

Casillas: If we look at the plan, there was a discussion that this is not just a temporal change, it's a change for the next 100 years or forever, so we have to look long range.

Boesch: Forever is hard to model.

Casillas: Yes. That's probably a decision that we have to subjectively impose.

DePinto: Relative to your suggestion of comparing the models, and I always think it's a good thing to do, we know the conditions under which the WES model was run, and I wonder if Antonio's 3-dimensional model could be pretty quickly run under those same conditions so that we could in fact make a comparison and see whether there are significant differences in the output, the results. I understand that Antonio ran his model under real-time, forcing conditions because it's a forecasting model, but you [Antonio] could, I think, take the study flow conditions that were used, use the forcing conditions that were used for the WES model and run your model with it to make a comparison.

Baptista: The answer is technically yes. I want to make a point. The reason I have deepened the channel in my model is because I strongly believe that it could be a lot better to do it when my model is regionally certified, which means a number of agencies cooperating. So I've been trying to avoid doing that, but technically it's possible.

DePinto: Well, I wasn't even suggesting looking at deepening, just for the base conditions. Compare the models using the base conditions.

Baptista: Even before you start running more models. Well, the answer is yes. What I was also saying is that maybe you want to use some of the available data to compare against the model conditions.

Eriksen: A comparison of base conditions doesn't really tell us that much because what we're trying to measure is the difference between with and without a project so we need to look at with and without project for both models because they're going to have their own variations, verifications, built-in errors...

Courtney: I think that's not the issue. The issue is do the two models essentially give similar results. If so, we can use one model to model the project.

Eriksen: All I'm asking is do they give similar results to the deepening?

Boesch: Well, that would be the idea. But in the absence of that capability, and Antonio gave you reasons why, I think you could run them with the same base assumptions and look at the differences and then ask, not with a qualitative model but intuitively and analytically, whether those differences are likely to make a real difference with respect to the channel deepening.

McAdory: That wouldn't make any sense unless you did both base and plan.

Baptista: Yes. The WES model has been designed to look at change. And you need a comparison of change. It's technically possible.

Courtney: It may be technically possible, but I'm not sensing any enthusiasm for doing it.

Baptista: If they agencies agree it's a good thing, I'll be enthusiastic about doing it.

Courtney: Does the panel think this is a good idea?

Bartell: It seems to me there are two fundamentally different scaling issues. One has to do with getting the hydrodynamics to a degree of acceptability which we don't really quite know what that degree is, and the other scaling issue has to do with the salmonids. I can appreciate that you'd want to have as accurate a description on the physical side, and then you can roll up those results either the way Antonio has done it on a regional basis and wanting to know if you're getting those regional descriptions of say habitat opportunity lost, or however you want to characterize it in relationship to the deepening. But then if you don't have the salmonid information at a similar spatial resolution, I'm not sure exactly what you gain. You at least have to take that into consideration as you're making your demands on the resolution of the physical modeling. So I don't really see the need for getting additional spatial resolution.

Goldman: I agree with Steve [*Bartell*]. It seems we're lacking this key information on what's limiting favorable habitat for salmonids in the estuary. If we knew what the limiting factor was, then maybe we could utilize this information to improve the survival.

Curtis: I feel pretty much the same way. I think modeling is useful if you can relate it to some outcome on habitat. I defer, however, to the physical modelers. If, whether or not, a measure of velocity or salinity or temperature is going to help with scoping on the impact of the project and the modeling at this phase and allow it to go forward, I think it's fine. Again, I'll just stick to my one narrow point. As long as it relates to something that you really believe tells you something about the quality of habitat for the fish, I think it's useful. But if you really can't convince someone or yourself that it really relates to the physical habitat for the fish, I don't know why you'd bother.

Dunne: Let's assume that Antonio's model is useful, that all this talk about physical modeling really does have some value. As I said yesterday, I'm surprised the Corps hadn't done more modeling. And not just the wider range of flows we talked about yesterday, but also hadn't mined the data that I'm sure are in Rob's [*McAdory*] files about the depth, velocity fields, etc., and presented them in a more ecologist-friendly way. They've needed to back up some of the fluid-mechanical judgments that they've internalized. Having said that, what I don't know is how much extra effort it would be for them to go back and either mine information they already have, and do a low flow as Charles suggests. I don't have a good quantitative idea of the logistical problems of putting some finer resolution into some of these shallow-water habitats.

Now, if we take this hot spot everyone's talking about [Cathlamet Bay], yesterday Rob [*McAdory*] told us this is a place that behaves differently from the rest of the area. We could put

more resolution into it, but we can probably already tell you roughly the order of magnitude of the impacts from what he showed us yesterday. So let's imagine it's logistically feasible... If you're going to do all of this, there ought to be some kind of an agreement about what you do at the end. I would like to see an agreement of how the results, I think the general nature of which we can predict, would be used... The rational analysis of how this stuff is going to be used needs to be said up front rather than go through the whole exercise and say, 'I don't know; could be, couldn't be...'

McAdory: The way I understand it, that's the way the first three workshops were set up. They were narrowly focused around changes in salinity and the resulting biological effects. It's unfortunate that they didn't also include changes in depth, or changes in velocity. There was a lot of arguing in those first workshops, but in the end, people agreed to pursue, or we didn't do anything...

Dunne: The other thing I think needs to be decided is to make a decision about whether a finite amount of modeling in the next few months would be worthwhile. Up front there needs to be a resolution made about whether this is just for the dredging, or whether the debate continues to open up to well we'll see the results in the context of everything that's happened. If that's still allowable, then I don't think there's any point in doing much further modeling.

Boesch: My colleagues' concern about the lack of definition of a salmon's requirements, I understand it. But on this modeling issue, if you could resolve whether there were any appreciable physical changes, and if there were none, on the basis of the base-to-action plan, then it's hard to make the case that salmon habitat is really affected in any meaningful way. So if a model comparison, using the same assumptions of the two models that would incorporate a deepening action proposal, were done could resolve that, and if the parties agreed to accept the outcome, if they basically agreed in broad measure... I don't have any idea whether it's feasible cost-wise or time-wise, but if indeed the WES model depictions of the kinds of hydrodynamic changes that drive salinity are robust, I would be surprised if Antonio's model shows something dramatically different. And if it does, then we should learn that. So I offer a qualified yes, if it can be done. If it can't be done, I fall back on Tom Dunne's position. That being that there are some other things that can be done with the WES model that provide some more information...

Courtney: And I think the commitment is there to do that. So that's the default condition -- to go back and get more out of the WES model. I want to ask NMFS to comment.

Young: Could I just say something about spatial scale? Using velocity as a measure of opportunity, the biggest change... opportunity there greatly increased in the riverine portion. I'd like to see that extended up into the Vancouver area to at least two more sites. We need to answer the question how important is that riverine reach for NMFS's issues.

Casillas: The discussion on the biology that Rob [McAdory] referred to is on sedentary organisms, not fish. What we have been asking for is a full discussion on salmon, and that wasn't apparent so that's what was missing and that's what we're asking for. So that's what this whole point is about. Is the discussion relevant to the species of concern?

In relation to Tom's question, the decision isn't just science only and that's why we can't bring to a boundary to say we will or won't decide beforehand. There are other elements that go into the decision matrix. If the region sees no difference and still decides no, it'll be basing that decision on other issues that add to that mix that we can't define right now, but that's something that Cathy and Doug will have to deal with. That's why they reserve the right to say no despite whatever the science...

Boesch: Let me get this straight. If you're absolutely convinced that there are no habitat changes as a result of this action, you could still say no.

Casillas: That's what I propose is the issue. Because most of it is that we're going to have still a gap of understanding.

Courtney: So what you're saying is that you reserve the right to be risk-averse.

Tortorici: Yes.

Young: Probably the application of that is the Endangered Species Act... jeopardy opinion so you get an incidental take statement in term of issues that would modify the project in such a way that we took care of some uncertainty. There's no such thing in my mind in saying 'no.' There are opportunities for more negotiation.

Dunne: I can appreciate that. But when you start asking scientists these uncomfortable questions, will you be prepared to say that the science says no?

Courtney: I think there's some miscommunication going on. I want to stop it before we go much further with it. The question was, if we do these two models and a comparison shows no difference between the two, will you feel comfortable with that and move forward with the WES model, and I think the answer to that is...

Casillas: Yes.

Courtney: I'd like to summarize where I think we are. I'd like to characterize some important progress I thought we had in the morning, which is the question of the adequacy of models and the need for more modeling. I think we heard something important which was the panel is impressed with all the models. The piece which was missing -- at least until now -- was how does that fit in with the biology? I heard from the panel the following concern that the resolution on the models may be very fine and precise, but if we map onto that very imprecise biological function, suitability index, whatever you want to call it. If we don't really have good information to map onto the biology, then we may be dealing with an issue of 'false precision' to ask for more and more precise and extensive models. Really one of the questions we need to address and reach some resolution on is how much more do we gain by increasing the power of our models if we don't have the biological information to map onto it. So I'm not pre-judging the decision, I'm merely stating that we need to find out how precise we can be on the biological parameters in

order to answer the question about modeling, about how precise should we make the modeling.

That was one of the discussions I heard. One of the points that we discussed over lunch was the idea of finding out more about models and about the relative strengths of the models. I asked the folks from NMFS whether it would be useful to have a comparison, and what would a comparison look like. I think there was some interest in finding out at least what were the capabilities of the model if run under the same conditions with the same issues, same parameters, would they show essentially the same results? And I asked for and received a commitment that if such a comparison were made, and if it shows essentially the same results, that it would only be necessary to move forward with one model. And there was no decision made about which model to use. Another discussion we had over lunch, I asked if there was willingness to move forward with a comparison and with more modeling. I think there's still some more discussions to be had about that. I've been asked not to proceed too much further with that discussion today, and focus on the biology, with the understanding that within the next two weeks you all will have discussions about what you want to do and reach some resolution and you'll talk to Antonio and to the WES folks and find out what's feasible, what it would cost, what the availability is, what the willingness is, and that the policy group would make some decisions in the next two weeks. We've reached as far as we can go with the one caveat that I think you've heard some feedback from the panel that they're pretty happy with the models generally in the sense of their capability of understanding the physical parameters of what's going on. So to characterize where I think we are now, there's a commitment to try to figure this all out, get back in two weeks, by the time we all see each other again, we'll know. Folks will be coming to report to you what's been decided and we'll know how the modeling is going to proceed. But I think that decision will be informed by where we are now, which is to try and map the biology into the physical parameters and to understand how precise really we can possibly be.

DePinto: One of the things that maybe is important for this exercise is to... Essentially what you're asking is to form a conceptual model of the relationship between the physics and the biology of the system. I don't want people to forget that we did talk about that a little bit and maybe we should look back at Ron's [Thom] presentation.

Courtney: Yes, we're going to need you, Ron. Before I put up the matrix, I thought it would be useful to talk about what the Corps did before, and the intent here is not to discuss adequacy. It's really just to lay out a launching point for where we go forward. Kim Larson, a fisheries biologist with the Corps will spend a few minutes doing that.

Larson

Goldman: Would your conclusions have been the same if you'd used half the flow ___ as we might anticipate under power problems, drought?

Larson: What we did was take the physical stuff that was generated by the model and decide what that meant from a biological standpoint, using the procedure I said. We decided at the beginning that we didn't want to look at a flow that low. I guess if we'd run that flow, and it had some kind of a dramatic change in the shallow water salinity, then we'd have come up with a different

conclusion. We never tried to do a sensitivity analysis as to what the group would've felt would've been an impact.

Eriksen: The salinity tolerance range of most of the species we looked at was quite large compared to the predicted range they were living in, wasn't it?

Larson: Yes. For Corophium, we did some salinity tests on them several years ago at OSU and found out they prefer somewhere around 14 parts per thousand, so they have a large range. I think everyone who works in the estuary realizes that most species have a large range. It depends on flow and tide as to what they experience on a given day. If they can't handle it, they're dead. We didn't do freshwater organisms because we figured they would be distributed and move back upstream.

Casillas: Your tolerances are based on mortality, yes?

Larson: Tolerances are based on what was published in the literature. It wasn't sub-lethal; it was lethal. It was mostly based on the range they gave us. In some cases, if they had the range that was like a preferred salinity, that was included in the database but for the lethal part of it, it was based on exceeding the maximum level they could tolerate.

Casillas: So what would happen if you used sub-lethal ranges? For instance, if you used a growth-range tolerance or a reproductive tolerance?

Larson: ...Sub-lethal is certainly a major concern, and that wasn't really where we could go with this because we didn't have the information to go there. Perhaps half a part per thousand would have a long-term sub-lethal effect on some species, but we didn't have any way to identify that.

Casillas: So it's possible you might interpret it differently if you had another set of tolerances?

Larson: Well, yeah. If we had some science that showed us that that level of change had a sub-lethal or lethal effect, there'd be no question that the group would change their mind on it. This was based on the existing information that was available. Like all scientists, we're open to new information to help us interpret things.

Courtney: Next, I'd like to do what we can to develop the idea of the relationship between different factors that could be modeled and their particular effects, where those effects would be felt, and how they might be plugged into the model. Before we do this I want to remind everybody that if we can't do this, it's going to impact where we go with the models. It's essential to us to move as far as we can along making the connection between the biology and the physics. If we can't make that connection, then the conclusion we may end up with is there's no point in developing further the models. So it's essential to take this as far as we possibly can in order to allow us in the time remaining, before the agencies make their respective decisions, to develop such information as is useful. Before beginning, do the two of you [*Tortorici and Casillas*] want to make any overarching comments?

Tortorici: I think what we're doing today is start at filling out this matrix. We have a fish habitat workshop that's coming up in two weeks. What I'm suggesting here is what we're going to put in this table from the standpoint of biology is going to have to be refined as we go into that next workshop because that was really one of the purposes of that workshop is to add definition to this. Start laying out some sideboards here about how far we're going to get during this discussion on biology because there's other people we wanted to bring to the table to help add to this matrix.

Courtney: That's one good overarching comment. Are you ready to launch in? You don't want to make any comments about relative importance before we start? I know that NMFS already had some discussions about their relative levels of concern about the different parameters and the ability to distinguish amongst them.

Tortorici: And we listed those out yesterday, the ones in the left-hand column that we thought we'd be most interested in... What we're looking at is Antonio's table: water level, water velocity, salinity, temperature (although we recognize that temperature is more complicated to get to), dissolved oxygen, and what did we decide about sediments?

Casillas: We want to talk about them, but we don't know how far to go with them

Tortorici: Yeah. Oh, and one other, water depth.

Courtney: Temperature is an issue that was not addressed in the original BA and is one of the things that you [NMFS] want to see more discussion of.

Goldman: We could talk about temperature and oxygen essentially simultaneously because of the inverse relationship. At low flows, you can have a warming of the estuary, lower oxygen.

Courtney: So oxygen affects eutrophication.

Sullivan: Well, temperature will drive the metabolism of the fish, so they will definitely respond to temperature. It's one of the most important variables in their environment. They'll have a range of tolerances - it's a fairly wide range - but it will cover the entire range of temperature that's experienced in that estuary over the course of a year. They'll grow at different rates. If one of the eventual concerns is to understand the growth of the sub-yearling Chinook, then temperature should be a factor in their environment that would have to be considered. It may not change with the project, but you certainly would have to understand it if you wanted to understand growth potential in the estuary for that particular age class. For fish that aren't spending much time in the estuary and passing through, you'd probably just want to make sure they don't hit lethal temperatures, which it appears they don't ever do.

Courtney: You [Sullivan] talked about two different things, lethal temperatures and growth functions. Do we have adequate information on those parameters for juvenile Chinook?

Sullivan: Yes on both counts.

Courtney: And that's readily accessible and we could put it into a function that would say, for example, 6 weeks at this temperature as opposed to another temperature would have a growth increment of....

Sullivan: Yes.

Courtney: In estuarine conditions or in freshwater conditions?

Sullivan: Most of this work that I'm familiar with has been done in freshwater. Tom [*Quinn*], to your knowledge, is there an effective salinity on any of those temperature-growth curves?

Quinn: Certainly the vast majority has been done in freshwater. But with all the aquaculture, there must be something in salinity.

Goldman: Well, I think they'd have a little less oxygen, but it would be a small fraction as salinity increases.

Quinn: There's a period when they're adjusting that they grow slower, and if they thrive the growth rates take off. If they don't, they fall. My suspicion is it would probably be minor compared to temperature, but I don't know for a fact.

Casillas: You can probably evaluate from salinity tolerance curves in mortality and energetic costs - incorporate that into a temperature-growth curve, you can get some semblance.

Quinn: At a given temperature, let's say coho, will they grow faster in saltwater or freshwater, or will it make much of a difference? The same ration, the same temperature. I think that's what Kate's [*Sullivan*] is getting at.

Casillas: I don't know. Walter Coffee(?), the endocrinologist in Seattle might know. Tom's [*Quinn*] right. The aquaculturists usually look for these efficiencies to evaluate in their production, so there's some extrapolation to be made.

Courtney: I'm going to betray my ignorance here and ask about how the range of temperatures we might experience in the estuary in different seasons, different flow conditions, would map onto the range of concerns you just talked about Kate [*Sullivan*] in terms of growth rates and mortality. Are we anywhere close to lethal temperatures, or can we forget that? Are we close to areas where this is likely to influence growth rate or again are we so far out of that that it's irrelevant?

Sullivan: Well, there's no question that a range of temperatures are going to be experienced in the estuary over the course of time that the fish are there. And the temperatures range from fairly low to fairly high that their growth rate can potentially be from its lowest to its highest points. With a wide range there should be differences in response to temperature. One of the interesting questions with regard to the project that was suggested in some of the readings that were given to

us is that there is clearly going to be a temperature boundary from going in and out every day. The ocean's going to bring in cool water in the summer, the river's bringing in warm water. So the action of temperature around the front may influence productivity in the habitats. So there is actually temperature that goes along probably with that turbidity maximum.

Goldman: This is a really good example of multiple stresses on the fish. For one thing, as they move from freshwater into saltwater, they have to make an osmo-regulatory change, which stresses them. At the same time, they're moving from a warm to a cold, or a cold to warm, that's also a major stress, and actually lethal if it's extreme enough. In addition, if you had a very low-flow condition, and you had eutrophication from sewage upstream, for example, you put an additional oxygen stress on the fish. It's really a complex and wonderful example of multiple environmental stresses on the downstream migrant. They're pretty flexible because they survive most of those. But one thing we're concerned about is that they don't shift to an extreme where they don't survive.

Sullivan: There is an interesting question about lethality. The temperatures in the Columbia are not rising high enough to be at the point of lethality in a few minutes. The river is too deep for it to reach such temperatures; it's going to modulate itself. The river is so large that its temperature profile doesn't vary much more than plus or minus a half degree a day. So when the river gets up to 22, which is a typical summer peak, it's staying for a long time at that temperature. It's not fluctuating over the day so that the fish spend some time at cooler temperatures. Actually that means that the effect of the temperature on their metabolism could be fairly high because it's very prolonged. And it's high enough at 22 that you're on the top edges of their productivity. Their growth rates should be declining pretty sharply at 22 and above.

Goldman: But the flow rates, for instance this summer where we're expecting low-flow rates, went to half, you'd get a warmer estuary than maybe has ever before been experienced.

Tortorici: The other thing to remember here is that the Columbia has been listed as water-quality limited for temperature. So if there's going to be increases in temperature, even on an incremental basis, that could create an additional difficulty for the fish.

Sullivan: I wouldn't think the project would change the temperature, but there is an interesting question about temperature that goes along with the change, it's probably correlated with the change locally the temperature as the change of the ocean water coming in and out changes.

Thom: What I wrote down in the matrix for connection to biota is that temperature affects respiration rate primarily. So as Charles [*Goldman*] was saying, if the respiration rate goes up in lower oxygen conditions, it really increases stress. The other thing is that juvenile salmon are found feeding on flats and in channels that in the spring and summer tend to heat up quite a bit. You'll find the salmon at the leading edge. So it seems to me they must have a pretty high tolerance of temperature changes. As far as the effect of ocean and river on temperature in the system, at least at Willapa, the flats have a major effect on water temperature because in the summer especially, they heat up like mad. When the water flows over, it really affects temperature.

Sullivan: Actually, if they're able to feed like you described it, it's a draw for them to tolerate the warmer water. The combination of food and warm water will actually get them the best growth. But warmer water without the food will hurt their growth. I would surmise that if you could put temperature in with that you'd see that there's a temperature profile changing around with the salinities.

DePinto: If you're getting more ocean water in, the temperature should go down.

Sullivan: There's only a brief time during the year where you wouldn't see that, where the temperature in the ocean and the river are the same, probably a few weeks ago.

Baptista: Twice a year.

McAdory: Is the fact that the water upstream is coming out of the bottom of the dams mean that the fresh water in the system is cooler than it would've been before the dams?

Quinn: It's warmer.

Sullivan: It will heat up once it hits the river system...

McAdory: But, before the dams, it was heating up all along the way. Whereas now, it's sitting in an abayment and it's coming out the bottom where it's colder. If anything, there's a problem with low oxygen when the water is too cold.

Quinn: Empirical evidence says it's getting warmer.

Sullivan: I've looked at the temperature profile down the Columbia, which you can get from USGS, and there are a couple spots next to dams where you do see it's coming out colder...

Courtney: This is interesting, but not relevant to where we need to go. I wanted to hear what Antonio [Baptista] had to say about the correlation between salinity and temperature.

Baptista: Kate [Sullivan] was talking about the area above the estuary. For most of the stations outside of the very shallow areas, if you plot salinity and temperature at any given time, you will see essentially a straight line. The slope will change a bit over time, from tide to tide, but that relationship is very strong except when there is inversion of the ocean magnitude of ocean and river temperature during the fall and spring. Modeling salinity is not the same as modeling temperature because in addition to modeling the change between river and the ocean in the case of temperatures, you also need to model the change with the atmosphere. That makes it a tougher problem.

Sullivan: I'm just saying it's an important factor for fish. I don't know whether the project will have a temperature influence that's significant to fish.

Courtney: Let me try to get us back to the matrix. One of the things we want to try and do is talk about how precise an estimate we might want on changes in temperature to answer the question is there a project impact?

Sullivan: If I were trying to interpret whether there was going to be an impact to the fish, I would look for crossing temperature thresholds. You'd probably be hard-pressed to pick out growth-effect changes at 17 or less. Maybe even 18 or less. At 19, you might begin to ask the question of what are the effects.

Casillas: There's disagreement among us as to the importance of how to include it. We haven't really come to any resolution yet about what that boundary would be. So right now, I reserve any judgment...

Courtney: Precise boundary, yeah, but the idea of gross ranges...

Casillas: I think we'll get there, but for now, I'd like to reserve comment.

Quinn: I don't have the tables in front of me, but the relationships between temperature and growth are pretty well understood. Over a fair range, they're pretty tolerant. At the high range, it tends to fall off faster and it climbs at the low end. They're living in estuaries and the different races are coming down at different times of the year...

Courtney: I think I've driven you as far as you're going to go. Let me turn it around to the modelers. What we're trying to do is map the two, one onto the other. We've heard there are some thresholds that we may want to understand where they lie, and there may be disagreements as yet. Ed's telling me that NMFS will reach agreement on what those temperatures might be. How precise can we make our models? If we wanted to go to half a degree, can we get to that?

Baptista: I think it's the wrong question. If you tell us what you need, it's then a question of how much effort you need to put into the models to get to that model. If you ask for 0.1°C, I think you're dreaming. But if you ask for a reasonable number, a few degrees C, you may be able to get there. Depending on what a 'few' means, you may get there very quickly or after a lot of work. So you need to know what you need. Right now, the models compute temperature, but it's very rudimentary, so I don't trust them. Effort has not been put into validating them. Refining these models would take quite some time.

Courtney: Let me give you two scenarios. I give you six weeks and you could do within five degrees - is that correct?

Baptista: This is for shallow environments only?

Courtney: Yes. And the second question is I'm going to give you six months.

Baptista: Yes. It's not rocket science; it's a question of time.

Quinn: You're talking five degrees - that seems meaningless.

Sullivan: Yeah. I wouldn't bother. The temperature range is from 10-22.

Baptista: No. That's not true. That's why I asked where. Because if you're talking about the main channel, that's easy; we're doing better than that right now. What I'm worried about are the lenses of high temperature that you sometimes monitor when you are doing a vessel survey.

Curtis: I think we're getting into an academic quagmire here because of the spatial heterogeneity. The fish go, they really behave in these temperature gradients. There are fish that use temperature gradients to go up and feed in warm waters and move to shallow waters to digest the food for bioenergetic advantage. They do all kinds of things with temperature. I'm not saying you can't have problems with temperature, but with a system that's as spatially heterogeneous as the lower Columbia - and I saw the period of time you're talking about where you're hitting 22, it's about a month in the data you [Baptista] showed. As you said, the point question is really crucial. These 14 stocks, and the different species will have different ranges of thermal tolerance, but these animals, unless they're stranded or really constrained, they'll move within that environment to work the areas of temperature that they find advantageous. And the additional complexity... what's the temperature of the ocean water?

Sullivan: 10 degrees.

Curtis: So you've got a 10-degree wedge up against a 20-degree wedge, it's extremely complex, and I almost want to rely on the behavior of the salmonids, not that it couldn't harm them. But to get in there and model that just seems extremely complex. I'm not sure what you'd gain without knowing anything about the behavior.

Baptista: So, again, if you forget about the very shallow zones where temperature balance is extremely important. The rest of it is a problem of mixing two numbers. And that's no different than salinity -they correlate very well on a tidal cycle basis. So if that's what you're talking about, it's a relatively easy problem. If, however, you're talking about getting details for lenses of heated water, then it's a very complex problem.

Bartell: But what would the project have to do with those lenses of water?

Sullivan: Well, it's the same as the salinity. You saw the models that showed that change.

Bartell: You're saying that these lenses are the result of effects in shallow water that don't have anything to do with changes in salinity.

Baptista: Yeah. The project has very little to do with those lenses.

Sullivan: The second part of the question, though. I'm not sure. I think you'd have a very similar result that you had with salinity in that you probably could model and actually physically measure some local changes in temperature, but you wouldn't find them significant to the fish. I'm not

sure, if I were spending a limited amount of money, that that's where I'd put it. But it is important to understand in the productivity of the estuary.

Bartell: Well, I'm not debating that temperature's not important for fish. Obviously it is. The question is how long are you integrating those select changes in temperature having to do with how long the particular species are occupying the estuary to have any manifested changes in growth.

Sullivan: You would definitely have that for those sub-yearling Chinook. Plus, the information that was given suggests that they're feeding at a fairly low rate, which means that the temperature could have a fairly significant influence on what's going on.

Bartell: One thing that could be done would be to use the linear relationships between salinity and temperature that Antonio's been talking about, map the projected changes of salinity to see what sort of ranges of temperature change you'd expect to see corresponding to those salinity changes, look at those ranges over the natural range of temperature variability in the estuary, and answer your question.

Eriksen: It sounds like there's a concern with the water temperature exceeding 18 or 19 degrees in terms of affecting the salmon. And the potential impact of the project is to allow colder water to come farther up with salinity intrusion, lowering temperatures. What we would have to model is the absolute temperature, not the delta, not the change in temperature. We'd have to know what the exact temperature is. To me, this sounds like we're getting into an extremely complex modeling issue, which is not very relevant because potential changes are so small. Am I off base?

Baptista: If you're referring to the high temperatures in summer being a problem, which I think they probably are, the project would be, if anything, increasing the penetration of the ocean during that period (being beneficial)...

Young: However, wouldn't those same intrusion areas potentially increase the amount of shallow water saline lenses that could be heated up?

Baptista: Not saline lenses. The heating is a process between the water and the atmosphere. It's going to happen whether that channel is there or not. I think Karl [*Eriksen*] is fundamentally right on this.

Quinn: And after all, they're going down the river that's 20 degrees. It's not like they first encounter it in the estuary. That's what they've been swimming down for hundreds of miles. I'm still having a hard time seeing what the issue is. Of course temperature is important, but how is it connected here?

Sullivan: Well, it's connected. It's just like that salinity problem. It's going to change in a small amount in local places. It probably does not have a significant effect; I think you can draw the same conclusion as you did for salinity. The purpose of filling out this matrix wasn't that there's an issue. The question is, is it important? Yes, the temperature of the estuary is important to the

fish living there.

Thom: I'm just thinking about ocean conditions and the temperature in the estuary. You know, we have these El Nino events and they really drive temperature and productivity in the system. They're going to complicate the analysis. Just thinking about the ecosystem, these large ocean, climate-driven events can affect productivity greatly. They'd need to be factored in to the analysis.

Courtney: So far, we've heard skepticism from some of the folks here about the need to do this. We've heard, not enthusiasm to doing the modeling, but capability to do the modeling at some level of precision. There seems to be an approximate match of the model's capabilities with the sorts of ranges and temperatures that we'd be interested in if we were to do this.

Eriksen: I don't agree with that. Anonio's scale is plus or minus 5; Kate's scale is plus or minus one. If the river's 20 degrees, and we can't go above 19, we're already in the hole. I don't see where modeling plus or minus 5...

Baptista: Okay, again, it's where you're looking at this temperature. If it's in the mainstem, we are really not talking about plus or minus 5; we're talking about a lot better than that. If you're talking about these small peripheral spots, then all bets are off.

Eriksen: And that's what we're concerned about; that's where they live.

Baptista: On the other hand you made a good point about the impact of the project. Is there any thought process to say that Karl's reasoning is wrong?

Casillas: I think the reasoning's fine. But the issue we have is that even though these animals are in a variety of habitats, they are making decisions based on the physiology in ways we don't fully comprehend. So there's an element of the habitat feature we think drives that. The fact that it's improving in the context Karl describes, doesn't mean you shouldn't look at it. It means you should in fact include it in the evaluation. Whether we want to model it or not is another question. We want to consider it because it may be a tradeoff in terms of evaluating the impact overall of habitat when you look at the full spectrum of features that drive the availability of habitat. If we are losing some because of some action on one element, but it's compensated for in some other fashion, I think it'd be important to know. So even though you think intuitively it's better so why worry about it, we think we'd want to look at it so that we have the full evaluation to the extent that we can of the tradeoffs that are occurring in the system.

Courtney: So it sounds like you're saying that if it's not a big effort and it can be done, you'd like the information anyhow.

Casillas: Yes.

Courtney: And if it's a major effort, you might prioritize on whether it was worth doing.

Tortorici: We have a technical synthesis paper in our office that I'd be happy to give to folks. One

of the other projects I'm working on is a regional review of the water-quality temperature criteria for Oregon, Idaho, and Washington. As part of that effort, there was a tremendous amount of work done to review all the available literature on temperature and its effects on salmonids. This is a peer-reviewed document and it's got the most recent information. There's also a table in there that lists all the tolerance ranges of temperature based on known literature for all the species we're talking about here. So the point is, if we decide to model temperature, there's information in a concise form that we could give to the modelers. So there's no data gathering that would be necessary because it's already been done through this other effort.

Casillas: One way at least to get us further, is to get more information that would help in defining at least an aspect of something that the salmon use and that's a habitat - whatever that feature is - that combination. If we have that information, then the discussion is well what do we think that means? We understand that's a very difficult interpretation, but at least having moved us ahead in terms of what we know and habitat defined for salmon I think would be better than subjectively making that call without even looking.

Courtney: Let me characterize where I think we stand on temperature. What I've heard is, it could be done. Different levels of precision, different levels of resolution, different levels of geographic resolution. Could be done in the range we're interested in. I think we have a pretty good idea of what the ranges are and we have good information on where to proceed, and we have published literature that we can go to that will allow us to look at the presumed biological effects. We're in good shape on temperature compared to other things.

Baptista: I'd like to clarify that the model does have proper mixing processes. What's rudimentary is the heat-exchange mechanism.

[Break]

Courtney: When we consider velocity, is there a direct impact on the fish?

Casillas: In our discussions in trying to decide what range would be preferable, it wasn't defined on the basis of direct and indirect impacts, it was defined more on where we found them and the type of velocity they preferred. Given that there's a full spectrum of velocities, we found that juveniles occupy certain ranges. The other feature we use is that a body length per second, or 30 cm/s, seems to be an optimum velocity. This was based on a sustainable swimming speed; that if you exceed a body length per second, that bioenergetically costs the animal.

Courtney: So you're talking about the duration period when those conditions would be maintained in the system as opposed to not being maintained and whereabouts those conditions would be.

Casillas: Correct.

Courtney: What would be the level of concern that changing the system in some sense would increase or decrease the flow in a range that would be impacted by channel deepening?

Casillas: There was no description of that. That was the issue.

Courtney: So it's really geographic extent of conditions, habitat opportunity... Deepening the channel would presumably increase flow in that centralized area and decrease flow elsewhere, no?

Casillas: In a subjective sense. Again, we didn't have any information to evaluate that.

Courtney: Could we get to the point of modeling with that degree of precision (one body length per second) over those geographic areas?

Larson: There's another factor associated with that. Clearly velocity is a factor, but it's got to have duration associated with it.

Casillas: The question to us is how do we encapsulate that in a form so that we can then try to assign some interpretation to. That was the dilemma we have faced in our discussions with Antonio. It's been an ongoing, interpretive process.

Sullivan: This is an extensive physiological problem. Fish actually are limited significantly by their ability to have velocities under 2-4 body lengths per second. Over that it's called burst speed and they can only maintain it by either swimming that fast or holding position in a current above 4 body lengths per second for only a few seconds.

Larson: I'm aware of all that. This is a different situation.

Sullivan: This is laboratory work on juvenile salmon. There's a lot of documentation. You can demonstrate that it will determine the presence or absence of fish in a current.

Larson: Exactly.

Casillas: There are two elements to answer your question. One is that we're not explicitly saying there is a point. We're actually putting boundaries, so it's actually 0-30 cm. So we're not saying it's one speed only, we're saying it's a range of speeds that we think are preferable. The other point is these are our best guesses right now, based on a lot of laboratory data and where we find them -- presence and distribution. The issue we still have on the biological side is how do we validate that that's a real number?

Tortorici: How variable do you think the velocities are in the tide flat for example?

Larson: I don't know; we never actually measured that.

Eriksen: You're going to find a wide range of velocities across a cross section of the channel. But the model's not going to pick that up. You're going to find slower velocities along the edges that the model's not going to pick up.

Tortorici: I was just curious about the extent of variability.

Courtney: The issue is you'd like some documentation of whether there's a change in opportunity and whether that's a significant change in opportunity. Could be in either direction. You're not trying to put value on this other than to say no more than 30 cm/s, right? I want to ask the modelers could we find and map, with whatever level of precision, changes in velocity at that sort of scale? Because if we can't do it, the discussion's moot.

McAdory: Yeah, we probably could do that. You can calculate the velocity and how you think they're going to change when you change the physical attribute of the system. Depending on the level of detail you're looking for it could get quite expensive.

Berger: It's a little bit of a challenge, though, in the shallows.

Courtney: I think we can leave the discussion at this point. We've recorded your concerns. You've given us a range, and we've heard that it can be done. There's going to be further discussions about how much it would cost, how long it would take, and whether it's appropriate to do those things.

Casillas: There is an element also of where those features exist within the estuary. All these habitats aren't created equal. So there's some issue of how to connect the information together so that the detail has to be of some resolution to tell us is it low velocities adjacent to marshlands, is it low velocity next to the channel? Those are different features that salmon perhaps value differently. We do that with respect to zones.

Young: [Largely inaudible, but wants to make sure riverine system is not ignored]

Casillas: When I'm speaking on these issues, I'm speaking of the whole project area, which is from...

Young: Are we saying the models can get us all the way to Vancouver?

Berger: They could, but I don't know if they have enough detail.

McAdory: Our model goes up to Bonneville, but I wouldn't feel confident using it past mile 70. The model was designed to look at the estuary only. I think the district has models they use in house that give all the information that they thought they needed for the riverine parts of the model. So that's why our charge was the estuary only.

Thom: Velocity is a factor affecting habitat properties, sedimentation and erosion. Also it structures a community. Can shift from filter feeding communities to decomposers.

Courtney: Panel: you've been expressing a lot a frustration over the difficulty of trying to map the biology onto the physics. Are these discussions helping at all?

Goldman: There's been a lot of dredging on the Columbia over the years. Have there been any studies on the dredging that we might look at? Impact of sedimentation on the benthic organisms, turbidity changes, that sort of thing.

Larson: [Basically, yes. Some of it is in the EIS]

Curtis: I think velocity is the most straightforward thing so far.

Dunne: Well, velocity and depth were the two things that Bottom et al. agreed on. Seems to me the obvious thing to concentrate on, along with salinity.

Quinn: Everything that's been said is correct. The question is what are the effects going to be as the fish move into shallow water where the effects are more difficult to model.

Courtney: So for the record, are we getting to the point where you're feeling some level of comfort now about the mapping of the physical models onto the biology?

Quinn: It seems intuitive to me. The modelers are saying that the salinity, the temperature and the velocity are all rolled up. You can't have one feature without the other. So if those are the things that are being modeled then we just look at those and see the change in one part of the river or another.

Bartell: I think this is very useful. If we think of this in the context of developing the kinds of stress response relationships that would go into a risk-based assessment then this is quite valuable. Where we're seeing some confusion, or perhaps lack of agreement, if you can't demonstrate that the stressors (i.e., changes in temperature, salinity, etc.) as a result of the plan lie outside the current and historical variability that these organisms experience then what's the point? You're not going to see any incremental risk. So if we can convince ourselves that there is going to be some additional impact along one or more of those habitat opportunity dimensions, then it makes this even more valuable. But I haven't convinced myself that from the information we have at our disposal thus far that the fundamental stressors as a result of the proposed project lie outside the current and historical range of variability.

DePinto: Would it be helpful to prioritize stressors, as NMFS has suggested?

Bartell: I'm just speaking for myself here, but if I were tasked this problem I would certainly want to address the habitat components or the potential stresses that had a direct effect on the physiology of the organisms, and then secondarily look at the energetics associated with growth as it might influence growth increment on these fishes as they move out to sea.

Courtney: I think that we have some proposed prioritizations. The ones I've heard are depth, velocity, salinity, and temperature being essentially where all the action is.

Dunne: Well, this is the best we can get. If we start getting into food and energetics, we're not likely to get that information.

Courtney: I think that's an important point and let's build on that. Let me see if I can encapsulate what I've heard and charge that as an opinion, which can be taken away. Yes, there is some point to getting more information on those 3 or 4 main variables, and reaching such resolution as is readily available from the models which we've heard can be put together. And to go further, would be an exercise in false precision. Is that correct?

Curtis: I would agree that those 4 factors can be connected to the modeling. I believe other things are important to fish, but I don't think they can be modeled. This is the modeling workshop, and we're going with the best science.

Goldman: I have maybe a minority level of discomfort. At some point we need to make a better link between the physical-chemical and the biological if we're going to make the best judgment. Perhaps the estuarine person coming to the next meeting, Si Simenstad, will shed more light on some of the things that are troubling me about tying all this together.

Courtney: There's no precise function that would allow the regulatory folks to make a firm decision on say the number of days of lost opportunity and have that as a real concrete number. But I do hear the general sense from the majority of panelists at least that finding out more information about those factors would have utility and exactly how the decision would be made by regulatory folks that's still a concern.

Goldman: It seems to me that holistically we're dealing with a fishery that's gone down to about 10 percent of what it used to yield and most of that can be laid at the existence of the dams and the loss of upstream spawning and the fact that the hatcheries haven't been able to take up the slack. We're now dealing with how the estuarine component is going to be impacted by this additional dredging. We've got other instream components to think about. We've the possibility of lower water levels in the modeling than we've dealt with in the past. So I'd kind of like to keep the argument open and get the best we can out of the modeling and attempt somehow to link it with the biological elements that I hope will be exposed at our next meeting.

Courtney: I think we've got an understanding that some more modeling would be of some utility.

Goldman: I give a yes on that.

Bartell: We seem to be hamstrung by the inability to do additional modeling in the context of aquatic systems food web modeling. Given the attention on the physical side, it's rather unfortunate that even with our apparent rudimentary understanding of the structure and function of the system that we can't develop some example food web models and look at what happens to production dynamics as you change depth, temperature, salinity... I think if we looked at the literature, we could find models -- apparently not for this system -- but perhaps that could be reasonably be adapted to fit this system that would help translate some of the results from the physical modeling side into corresponding ecological impacts. To me that would provide the link between changes of whatever magnitude as a result of the proposed plan to projected changes to the lower trophic level production dynamic as well as direct impacts on salmonids.

Eriksen: I have a lot of concern about what you just concluded that salinity, depth, temperature, and velocity are the right parameters because we've changed the velocity parameters. If we change the velocity criteria to a set velocity of 30 cm/s, more or less, but not to exceed 30 cm/s, that's a level of precision for a model that we don't have. Especially when you consider criteria for the shallow areas and side channels. There are no data what those velocities are. If we now have an absolute criteria, let's just say that we model it perfectly at the threshold of 30cm/s and we deepen it and now we're at 31. You have an absolute criteria that you now don't meet. We haven't validated the models at the various velocities. We could model differences in velocity better than we can the absolute velocity. We can get precision, but we can't get accuracy.

We've also put temperature into the same type of standard, that is we're not putting C at 18 or 19 degrees, starting at 20 degrees coming down the river in a situation where increased salinity intrusion would drive the temperatures down, not up, but we have to model that, which means you go through the same level of effort as if you were trying to find a negative impact. So your accuracy has got to be plus or minus a half degree. I think while this can be done, we need to address the question of how long we're going to have to do this, and what kind of conditions are we going to have to get to verify because we're not going to have the same velocity patterns out there at 600,000 cubic feet per second as you will at 80,000 cubic feet per second. We've been talking with the reconsultation time schedule that we would have the modeling done by the first of August. Are we now making decisions to blow that schedule out of the water, or setting impossible deadlines and accuracy requirements for a modeling effort that will ultimately have questionable results? Where are we going with this?

DePinto: It wasn't my understanding that these were absolute criteria. I thought we were looking at the change on the base conditions vs. the plan.

Courtney: [Based on lunch conversations] We don't yet know, because we haven't consult the modelers and the modelers were going to report back to us on what they could do in a particular time frame, and that would then be a basis for decision among the policy level folks about whether that was a useful exercise to carry out in the time available. So there is no decision yet that we will move forward with a massive modeling exercise.

McAdory: I would hope that we'd be looking at differences, not absolutes.

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Courtney: Karl's concern is that we're rushing to judgment. It's my understanding that the project managers and the policy folks, after they've heard back from you, Karl and the other modelers, about what is the capability, the time frame available, the resources that are needed that in the next few days there will be discussion, following the feedback from the modelers, on what is feasible.

Hicks: The point that Karl is making is fundamental...trying to assess ... impact to proposed actions and the difference is very important.

Sullivan: If they do the velocity distribution, they will calculate an area of the estuary that will be usable habitat. If they calculate it under a plan, they'll have another area. So the difference in area could be used. If you're concerned that on every given point in the grid that you have to get it right, then you would be concerned about the model error. But if you just say, we're going to look at the total area, and look at differences in area, then I think you're still looking at an area. There are some issues to work out in terms of velocity.

DePinto: You can do it. You just have to specify the criteria. That's the advantage of using the model to look at changes or differences is that you can specify exactly the conditions under which you want to look at those changes. An average velocity over a time cycle is not going to be what you want to look at... The biologists need to tell us what to look at.

Berger: Is the magnitude of velocity the kind of thing you're looking at?

Sullivan: That would have to be carefully thought out.

Dunne: I thought we believed that specifying habitat and depth as habitat indicators arose from work that Bottom et al. used. What was the velocity they used?

Eriksen: 30 cm/s.

Dunne: They characterized velocities in the estuary so their flows must have been subject to the question that Rob [McAdory] is bringing up. So how did they apply this criterion in the estuary?

Baptista: In Bottom et al, what we were looking at was a way of thinking about the problem. In this case, it's a decision on whether to go through a project, in this case channel deepening, depending on what the number is. I think that what the Corps is saying is what is the criterion for which we are asking... I think it's a fair concern.

Sullivan: Antonio, I believe Charlie's asking about where Bottom got the numbers. I believe they constructed those from the literature. It was not done in an oscillating environment. It was done just watching fish swim in currents. So this is sort of a new application for that problem.

Courtney: We want the modelers to come back to us with a statement of what is feasible within the constraints we've outlined. And that will then be developed as a proposal by the project managers. That proposal could take any form -- we can't get anymore done in the time left, or we'll expend x dollars to get this thing done in a particular time frame... That proposal will be worked through by the partners and will be presented the next time we see you. What I would like to do with the remainder of our time is to ask the folks who are going to be tasked with this issue, what do they need to do in order to come back to us with something meaningful in a matter of days, and then to ask the panelists for what help they can give us in the time available.

McAdory: I can get with Karl and determine various things we think can be done in the time left.

Courtney: What more information do you need about modeling parameters?

McAdory: I think I have enough.

Eriksen: I want to make sure I understand what they are. They are a velocity criteria of 30cm/s or less, a temperature criteria of 19 degrees or less, so that we are modeling the actual values...

Berger: I won't be giving you a proposal on temperature. I'll leave that to Antonio. The model does model temperature. But I think there are so many problems with that, that that is not a short-fuse issue unless you've already been working with it like Antonio has. Besides that, in my personal opinion, you're wasting your time worrying about temperature.

Baptista: So are you asking for a joint consensus proposal?

Courtney: We want you to talk to each other, yes.

Torotorici: But it doesn't mean that there needs to be a consensus if we come up with two or three different options or approaches.

Courtney: But we need that feedback swiftly.

Tortorici: Right. It's important that you folks talk to each other to see the variations in opinion on what or what cannot be done.

McAdory: On the issue of depth, I never heard a number. Is it adequate on a first stab just to look at what changes might come from deepening? Or is it tied to a number?

Casillas: Antonio had a range from 0.1 - 2.0 meters, which characterized our definition of shallow water habitat which is based on where in other systems we find juveniles, the sub-yearling chum type.

Baptista: Is there a deadline for the delivery of products?

Courtney: There is no deadline, but we're interested in what's feasible in 3-4 months. What else do the modelers need to hear?

DePinto: In making these proposals, I think it's really important that we don't tell you just what parameters we're going to look at and how we're going to force the model. I think we need to be more specific about exactly how we're going to aggregate in space and time those model outputs in order to get an answer the biologists can look at. Because before you go ahead and do something like that, you want to make sure there's a comfort level on the part of the fisheries biologists. If we just give you a map of how things are going to change over time, that's not easy to evaluate something like that in terms of a biological response. Part of the proposal has to be not only what model runs will be done, under what forcing conditions, but how will the output be aggregated. Karl is worried, with some justification, that we don't want to necessarily just say we're going to give you a map of velocity over the whole system at an instantaneous point in

time. That's not going to be as useful as some other aggregate.

Berger: I don't think you can do that a priori. When we met for the other workshops, it took a long time. We argued for two days what approaches would be taken. In this case, whatever product you come back with may not be the most useful. So I think these maps would actually be of benefit because then the biologists could look at them and they could get a gut feeling of whether the change is dramatic or not.

Courtney: I'd like to ask Antonio about something that was raised earlier but not resolved. Asking Antonio to consider replicating some of your earlier results from the WES model. Run your model, Antonio, to see how different your results would be given the same inputs. The intent there was not anything other than to establish whether the models are essentially comparable or not.

Baptista: So as a general comment, what I assume you're looking for is a cooperative modeling effort where the essentially leveraged strengths of existing models and give all the agencies added confidence that they have an answer that makes sense.

Courtney. Right.

Bartell: I can't help thinking down the road, when we're done with the sediments workshop, that we will have wished we had asked for models of shear stress.

Courtney: I think we heard earlier from NMFS that they're not too fussed about that.

Bartell: But it's really your only hook to sediment resuspension...

Courtney: And we had some comments on sediments and they weren't too fussed about that but Charles told them to do it anyhow.

Bartell: Well if that's off the table...

Casillas: I don't know that it's off the table, it's just less of a concern. We know, in talking with the various modelers, that it would be very difficult to do. We're not taking it off the table, however. It's just less of a priority at this stage.

Courtney: If you want to include it for the modelers to think about now is your chance to say.

Casillas: I want to be in the middle ground here.

Curtis: I'd like to make a general comment on the model outputs. I think it would not be beneficial to put the absolute numbers into the model. If I were a fish, the various units don't matter. What would matter, in terms of velocity, is whether I had to swim harder to maintain position. The same thing with temperature. It's the magnitude relative to where the fish is. I don't think biologically I can see a big difference if you get a percent change and then an absolute

change around the desired numbers you used to define your critical habitat. And so, not that I don't believe in those numbers, but if it makes it easier for the modelers, I'm saying why not.

Sullivan: The change associated with temperature is a non-linear quadratic function.

Curtis: I don't know the model, but I do know the fish, and I know the fish cares how many degrees C change it experiences and you can express that as a percentage.

Casillas: Can I clarify something from NMFS' perspective? We didn't use absolutes to define the world for fish habitat. We used it to define a set of criteria in which we were most interested in looking at change. But the parameters were set to put the boundaries where we thought the relevant changes should be asked of the model.

Goldman: I'm hoping you'll run the salinity model to reflect the possibility of half flows from the 134 that you might experience this summer. Also we had a little discussion about the existence of previous dredging studies that have been done, and there might be some data available to look at the next meeting, which may make things clearer for the biologists.

Quinn: Would it be difficult to build in the very strong inverse relationship between flow and temperature? In the years where flow is particularly high, temperature is low and vice versa. If you're going to model a real low-flow year, you also should model that as a high temperature year, is what I'm getting at.

Berger: Antonio is the one to ask.

Courtney: We've given ourselves a big task and set some expectations. The panel and I are going to hold you to them. There's a sense of collective urgency and the need to come to closure. I also want to acknowledge the significant effort that everyone's gone through. I know this is all very painful. On the other hand, I think you're getting to the part that you need to be in your process.

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