1995 RESTORATION
WORKSHOP TRANSCRIPTS

JANUARY 17-20, 1995

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EXXON VALDEZ OIL SPILL SETTLEMENT TRUSTEE COUNCIL

RESTORATION OFFICE
Simpson Building
645 G Street
Anchorage, Alaska

1995 RESTORATION WORKSHOP

Anchorage, Alaska
January 17-20, 1995

PARTICIPANTS

Introductions

Ms. Deborah Williams	Special Assistant to Mr. George Frampton, Assistant Secretary, U.S. Department of the Interior
Mr. Craig Tillery	Trustee Council Representative for Mr. Bruce Botelho, Alaska Attorney General, Alaska Department of Law
Ms. Molly McCammon	Executive Director, EVOS Trustee Council
Dr. Robert Spies	Chief Scientist, EVOS Trustee Council

Presenters

Dr. Alan Springer	University of Alaska, Fairbanks
Dr. Jeffrey Short	NMFS, Auke Bay Laboratory
Dr. Stanley "Jeep" Rice	NMFS, Auke Bay Laboratory
Dr. Ray Highsmith	University of Alaska, Fairbanks
Dr. Leslie Holland-Bartels	National Biological Survey
Dr. David Irons	U.S. Fish & Wildlife Service
Dr. David Duffy	University of Alaska
Dr. Al Tyler	University of Alaska, Institute of Marine Science

Dr. R. Ted Cooney	*University of Alaska, Institute of Marine Science*
Mr. John Wilcock	*Alaska Department of Fish & Game, Cordova*
Mr. Mark Willette	*Alaska Department of Fish & Game, Cordova*
Mr. Dana Schmidt	*Alaska Department of Fish & Game, Soldotna*
Ms. Kathy Frost	*Alaska Department of Fish & Game, Fairbanks*
Ms. Rita Miraglia	*Alaska Department of Fish & Game*
Ms. Judy Bittner	*Alaska Department of Natural Resources*

Peer Review Panel

Dr. Stan Senner

Dr. Pete Petersen

Dr. George Rose

Dr. Phil Mundy

Dr. Chris Haney

VOLUME I

January 17, 1995

P R O C E E D I N G S

(Tuesday, January 17, 1995)

MS. MOLLY McCAMMON: I'd like to welcome you to the 1995 Restoration Work Shop. (Aside comments) There is a change in the agenda. Dr. Jeep Rice was left off the presenters today and he will be giving a presentation on oil distribution and long-term damage. His will be at 11 o'clock, following the 10:45 break. I wanted to introduce myself, I'm Molly McCammon, Executive Director of the Oil Spill Trustee Council, and here to give introductory remarks and a welcome is Deborah Williams, Special Assistant to the Secretary of the Interior for Alaska and is the federal Trustee representative for the Department of Interior.

DEBORAH WILLIAMS: Good morning. It's a pleasure being here today. As some of you, but not all of you know, my undergraduate degree was in biology and there few things I love more than science and a few things that I think are more important. I want to welcome you all to the beginning of Phase 4 of the Exxon Valdez Oil Spill Trustee Council activities. This is a very important time and you during the next few days will be playing a very important role in what the Trustee Council will be doing for the next many years. Let me briefly just describe what I think are the four phases and what I think are some important things to focus on and then I'll just give you a few of my thoughts. I think the four phases that we have gone through -- the three phases we have gone through and the fourth phase that we now embark upon are, of course, the first phase was the response to the spill itself. I know many of you in this room were involved in that response and what you bring to the table today should, in part, reflect the responses that took place in Phase 1. Then, of course, there is Phase 2 where the initial restoration activities prior to the creation of our staff and Executive Director. Phase 3, which I think we just completed, was a phase which started with the creation of the Executive Director's position and the staff so the Trustee Council had more guidance, and that phase consisted of creation of the Restoration Plan, which again, for those of you involved in that, I thank you, it's a very good plan. The final environmental impact statement which flushed out in some respects the Restoration Plan, and the other major planning documents, and, of course, a tremendous amount of activity in the habitat acquisition area. There was a lot of activities all of you know, and for those of you involved, again I thank you. We have much of our habitat acquisition activity behind us. We have a tremendous amount ahead of us, but so much of our review, planning, documents, so many of our initial offers have been made. Now, I think we are entering Phase 4, and I see today as the really official beginning of Phase 4. Phase 4, I will characterize as implementing a scientifically coherent, financially sustainable, and ecosystem based restoration effort. Four characteristics of Phase 4, we are going to have, of course, at least two new Trustees on the Trustee Council, and this is important because the state will be bringing a new Trustee from the Department of Environmental Conservation and the Alaska Department of Fish & Game, and they will be bringing a new and fresh perspective. I think they will be asking new hard questions. They won't have spent as much time in Phases 1, 2 and 3, and so I think it's very important in thinking about what you are going to be doing in the next several days, to be able to justify what you are doing to a brand new Trustee because that is what is going to be happening, we're going to be having new fresh questions and a new fresh perspective. We have a new Executive Director, Molly, and we're very excited about having Molly as our new Executive Director, and one characteristic of Molly that I think is so refreshing is that she pays attention to detail, she pays attention that things make sense. We're in a phase where it is, I think, less rhetoric and more down to details. Things have to be sustainable, things have to make sense, and I think Molly will be bringing that common sense perspective to what we're doing in this next phase. There is going to be a sharper focus on the Work Plan in Phase 4, in part because we have the new Trustees, in part because we have so much acquisition activity behind us, in part because we are going to have to be doing something financially sustainable. In the past, at least in my tenure on the Trustee Council, the Work Plan hasn't been a complete carte blanche, but we haven't given it strict scrutiny that I think we will in Phase 4. So, keep that in mind in what you are doing. It is going to be scrutinized more carefully. In general, I think the Trustee Council is going to be looking for more focused, scientific direction on what we are doing. I think we are going to be looking to Bob, (aside comments) and we're going to be having -- as many of you know, we're going to be hiring on staff a scientific liaison person. We're going to be looking to that person, and we'll be looking to the scientists for more direction, and we're going to be asking harder questions. Many Trustees have wanted a Science Plan, and I think we will be evolving into something slightly different than a Science Plan, but there is that desire. I think the desire is going to have, as clearly as possible, the questions answered that you are going to be asked to answer this time, and that is, what is the resource status. I think at our first Trustee meeting, I hope in February, that Molly or Bob will be able to stand up and just go species by species and say, this is our best analysis of the status of the resources. Again, clear, as precise as we can get, as clear as we can get. The second question, of course, is going to be what is limiting recovery for those species that are not fully recovered. I think what the Trustee Council would like there are some hypotheses. If we can't say we are sure at this point that X is limiting recovery, we would like some hypotheses as clearly stated as possible. Hypotheses that will help govern the Work Plan and the biology and other work that we're going to be doing over the next few years. Please come up with some very clearly stated hypotheses of what you think is limiting recovery. In my mind that's the most important thing that will come out of these next few days and we'll be focusing our research; then, what can we do about it. Of course, once you've stated your hypothesis, part of it will be research, part of it will be monitoring, part of it will be general restoration. We're going to want concrete recommendations. We're going to want to see progress in resource recovery and we're going to be looking to all of you to give us that direction, those recommendations, those programs and plans. To the extent I can speculate on what the Trustee Council as a whole will be looking for, those are my speculations. Let me give you a few personal speculations. Monitoring is still very important, and so as we go forward with research and with general restoration, in addition to habitat acquisition, it is going to be crucial to answer that first question, the resource status, to continue in our monitoring efforts. The ecosystem base research is crucial. As many of you know, my boss, Secretary Babbitt, believes greatly in ecosystem research. My specialty undergrad was ecology, and I think if we're going to answer these questions coherently, if we're going to answer these questions in a way that assure that the resources are recovered, we have to look at the questions and answers from an ecosystem based perspective. This may be a little more controversial, but one question I'd like you to ask and answer, if at all possible in the next three days, is it time to look more critically beyond the spill area for general restoration? I think appropriately we have focused in the first several years in the spill area, but as we look at general restoration activity, as all of you know, we have a very substantial budget for general restoration, is it time to look more thoroughly beyond the spill area for general restoration? I would love to have that question examined more thoroughly. Another thing I personally would like this group to help us do, as Trustee Council members, is help us separate routine agency research from that which is specifically required by the spill, and the questions we have to ask and answer in the spill. Again, as we try and achieve a financially sustainable research plan, we are going to have to make some tough calls, we'll have to look at some agencies and say good proposal, but this is more closely aligned with your routine research. If you can help us call those balls and strikes, we will appreciate that. Finally, I'm just going to re-emphasize this because from my perspective this is the most important thing that we will be doing in the next couple of days and over the next couple of years, please give us a master list of hypotheses and explain how they can be tested. I think everyone on the Trustee Council and key people on staff are very smart, we've all had at least basic science, if not more so, give us the hypotheses, tell us how they're going to be tested, so that then we can fit it in the specific research proposal for those hypotheses, we need that direction. Anyway, those are my few thoughts, and I again thank you all for being here today, we have a lot of important work still to do for, candidly, decades. I see this as the beginning of Phase 4 and look forward to your guidance to help us craft our research in a scientifically coherent, financially sustainable, ecosystem-based approach. Thank you.

MS. McCAMMMON: I think Deborah was very accurate in saying that the Restoration Plan represents a real step in the Trustee Council progress. The one thing I'd really like to focus on in my comments is on a table that listed on page 6 in the

Restoration Plan. If you look at this table, the left side reflects the payments that the Trustees have already received from Exxon, as well as the additional payments. What you don't see here, and this total is $900 million, is the possibility of re-opening for additional $100 million damages that were not known at the time of the settlement, so we refer to this as the re-opener clause. So, there's still the possibility of seeking an additional $100 million here. If you look at the right, the right-side is actually what is found in the Restoration Plan. (Aside comments) If you look at this spreadsheet here, you see that for the past expenditures for the Work Plan has already been spent somewhere -- I don't know what the total is for past expenditures, but with the estimated future there's going to be approximately $192 million to $222 million spent on the annual Work Plan. The Trustees have also committed to $25 million to the Alaska Sea Life Center in Seward. They've committed $342 to $372 million for habitat purchases. This includes past purchases as well as an estimated future purchases. Most of these were made in the form of offers that were given to various corporations in November and December of 1994. One of the things that we'll be working on this year is bringing all of these offers to fruition. This will require quite a bit of work just to get to the point of actually being done deals. In addition, in the last year, the Trustees, in response to enormous public support, public comment, made the first payment, $24 million, in establishing a restoration reserve. If they commit to $12 million a year throughout the life of the payments, we'll end up with a reserve of $108 million, plus interest, and we estimate that to be about $150 million by the time 2002 comes along. And, then as part of the settlement there were also reimbursements to the agencies for the work they did during the damage assessment phase, and then various adjustments totaling about $900 million. Now these expenditures here as they were outlined in the Restoration Plan are merely guidelines. The Trustees, if they get six votes, can modify these based on what they determine to be the most important restoration needs of the time. So, if it's determined that there needs to be more emphasis on habitat purchases at the time, they could end up putting more money there, they could end up putting more money into the restoration reserve, they could end up putting more money to the work plans. These are guidelines to be followed. This kind of a structure was developed based on extensive public comment, and it reflects what we call the comprehensive balanced approach. In other words, these are the various major tools that are offered for restoration and this is where the Trustees believe we are heading in the next few years. Now, our emphasis in the next four days is up here at the top part, which is on the Work Plan, and where we have down here, our estimated future, 107-137, again, this doesn't mean we necessarily have that given amount for the next six years to spend; however, this is a guideline, this is kind of some of the things we're looking at. Now, as Deborah said, my attention for detail, I guess that comes back from my reputation in the last year as mainly being the "Chief Nag" on getting things done, especially in regards to the Work Plan. In the next year, I think I will be continuing that role as the chief nag, and the kinds of things we'll be nagging on and working on, first of all really as Deborah said, really focus and hone in. What are we doing? What are we trying to accomplish with these projects? What does it all mean? How does it fit together? We're also going to be putting a new emphasis on budget review. How can we do these things more efficiently at less cost? How can we do more with fewer dollars. This year, so far, for the 1995 Work Plan, the $22.8 includes the administration costs. Right now we're at about 18.6 in projects -- research, monitoring, general restoration, those kinds of things. There's a potential for adding to that, if the Trustees decide to go ahead and make the commitment to forage fish, nearshore predators, some of the other projects. So, some of those will still becoming before the Trustee Council this year. But, if we're looking at -- let's just say, for example, $22 million this year, and then we're looking at how do we kind of set a way into the restoration reserve in the year 2002, there's a lot of decisions that have to be made between now and then. What will the restoration reserve be used for? How will it be structure? What kind of income will it generate? You can look at in a number of ways. You can look at it as a way of just spending $12 million a year for another ten years, or something like that. You can look at it as the ability to create a long-term legacy through some kind of a perpetual endowment, which would give you the ability to have five or six million a year guaranteed for the rest of our lives, and who knows how long. The habitat acquisitions and the restoration reserve are probably the two major, long-lasting, legacies that we'll see from the settlement between the federal and state governments and Exxon Corporation. So, as we spend the next four days, I'd like you to think about this emphasis on the work plan and how it fits into our overall restoration program, and keep in mind that our major goal here is to focus, to really think about what's important, how best, how wisely to spend the remainder of the money that's coming from the settlement and, we always call it at the office, telling it or describing it as some mom can understand it. This doesn't mean we're doing the kind of science mom can do, but we have to be able to explain to the public what we're doing and why we're doing it, and if we can't explain to the public that, then we're not doing our jobs right. So, I hope all of you will help in that effort to explain what we're doing and why we're doing that. And, with that I'd like to turn it over now to Craig Tillery, who is an Assistant Attorney General with the Alaska Department of Law. He is one of the Alaska representatives on Exxon Valdez Oil Spill Trustee Council.

MR. CRAIG TILLERY: That money was a long time getting to the state and federal governments. We were a long time setting up the Trustee Council structure. It's very important to me personally, and actually I think there's about a hundred of us out there that feel pretty strongly about this money, what it's going to be used for, and I think what Molly talked about a lasting legacy is something that all of us feel very strongly about. Personally, to my way of thinking, one of the most important lasting legacies that we can have is that that's going to be provided by the research and monitoring. I can tell you I've gotten into numerous discussions with people when they have talked about the importance of doing this project or that project, the importance of buying this land or that land, and I have always come back to the concept of, yes, but if you had knowledge, some of this stuff wouldn't have happened. If you have knowledge, we're going to be able to do better next time. I think that's where a lot of the research and monitoring, and a lot of this work plan activities are going to help us. When we first began to litigate the case, the most obvious -- well, it's the first thing we figured out is we're going to get a ton of money off of this case, we're going to have to be blind not to. But, how much? We didn't have much of a clue. The first thing we did was say, well, we know what's happening out there right now in Prince William Sound, sort of, we know there's oil on the shore, but we saw exactly what it was like before the oil got there, and we really couldn't get very many answers. Then, we said, okay, well, kind of what's going to happen -- what's happening right now, really, and we didn't get a lot of answers. Then we said, okay, well what's it going to be like in ten years, when is it going to recover? And, we got even a deadlier silence. I think this lack of knowledge about the Sound and lack of knowledge about the marine waters is something that needs to be corrected. It seems to me that the annual work plan is probably the first place to start. Careful planning of how we're going to use the resources we have available to commit to the work plan. However, I think that more important -- or not more important to the Work Plan, but the necessary follow up to the Work Plan is planning for the future, and this is something that Molly talked about and something that's particularly near and dear to my heart. The way to get there and the Trustee Council has taken very positive steps in doing that, is by the establishment of the restoration reserve. There are some questions about what that reserve is going to be used for. My own view is that it is intended to be used for research and monitoring carried out into the future. I think its -- we don't know how long it's going to take, but I think that future, for purposes of our present planning, needs to be an indefinite future; that we should plan that from now, that we have money, we have money coming in between now and 2001, and we need to plan to use that money wisely, but we also need to plan to use that money with the concept in mind that it doesn't end in 2001, but that in 2002, 2005, 2010 there's going to be a source of income that can be used for work plan kinds of activities. I would suggest that the place to start that type of activity, to lay the ground work for a successful 5, 7, 10, 20 or however many years it takes, is in a place like this room and a time like now. I would recommend to everyone that one puts aside one's own sort of special desires for particular studies or whatever that one looks to the ultimate view of the process, that one looks to the ultimate good of restoration, that we impose a kind of discipline that we need to go forward with a logical and coherent scientific plan, and that the work plan that comes out of this particular session sort of be a foundation, be a model, that we can use in the future. That's my view of where, sort of in a nutshell, how we got here and where I hope that we head. It's my hope that the people in this room are going to play a major role in getting us there. On behalf of the State of Alaska, I would like to welcome each of you and to thank you for coming here and wish you good luck because actually we're going to be looking over your shoulders and passing judgment, rightly or wrongly, sooner or later. Thank you.

DR. ROBERT SPIES: Well, good morning. I think most of you know me, for those of you who don't my name is Bob Spies, I'm the Chief Scientist for the Trustee Council, and I'd like to welcome you and thank you in advance for your participation in coming to this workshop. I know many of you are extremely busy preparing reports -- many of you are academic and agency scientists. You've got a lot of other obligations, so I appreciate the time you've taken, these four days to come and participate in this process. It's extremely important to us to have your input so that we can best manage the scientific program for the Trustee Council. Last year's workshop, I think, was very, very successful. There was a lot of enthusiasm, interaction between the public and the scientists were good, and between the groups of scientists, I found a lot of creative ideas coming out, people got excited about their projects and got recharged for going out in the -- charging forward into '94 with some very good ideas for the work plan formulation in '95. So, I'm looking forward to the next four days.

I'm also personally very pleased with the way that the Trustee Council has evolved. I think Jim Ayers and Molly McCammon have set a really good tone and good direction for this whole process in the last year. They are driving it more from the inside out, it's making more sense, and we're learning as we go along. I'm really pleased that Molly McCammon has replaced Jim Ayers as Executive Director. I think under her leadership we're going to have a continuation of that good tone that set under Jim's leadership. Recently the Trustee Council renewed the contract for Applied Marine Sciences and so on behalf of myself and Andy Gunther, the Assistant Chief Scientist, and Sue Chase, who many of you deal with on reports and over the telephone (indiscernible) to Alaska recently, that we're all really pleased to be a part of this process and look forward to working with you in '95. I'd like to reenforce some of the comments made by the previous speakers about the overall scope of the Trustee Council science program, where it's been, how it's evolving and where it's going. I think it's extremely important time and really valuable time that all of you are here can contribute to the shaping of this process in the next several years, and it's an appropriate time to start thinking about this. Again, we had a time of response, we had a time for damage assessment and then we had the early years of restoration. In 1994 and 1995 we've moved a little bit away from strictly population monitoring and started increasing the amount of research that was done on ecological processes from the standpoint of trying to understand injured and non-recovering species in the environment, and what particular factors in the ecosystem were restraining their recovery. I think that's an extremely healthy development in the scientific program, and we really have a debt of gratitude for many of you for kind of contributing towards that momentum. And, I think it's going to be important to try to keep that momentum going. So, you saw the figures that Molly showed, and we have to deal with a certain kind of fiscal reality here in terms of where this program might go, and we do have perhaps as much as $130-140,000,000 dollars for the remainder of the time when Exxon is going to be making payments to the Trustee Council, but we have to think beyond that. We have to think to the restoration reserve and what's possible. Personally, I would favor a type of fund that would fund ecological process research in perpetuity. Given the fiscal realities, that's probably a program that something around $5,000,000 a year. We have a program right now that is $18.5 million a year, plus we have two other ecological process packages that are being formulated, one in the area of forage fish, and extremely important area of research, and one in the area of nearshore predators, that's taking on some real challenges of trying to integrate some of the nearshore processes and what's happening with injured species in nearshore area. And, those together we might be looking at greater than $22,000,000, if they are all approved for funding this year. That remains to be seen. But, the point is in the year 2002 we're going to have to be down to a level of about $5-10,000,000, depending on assumptions that are made about the research program. That represents some sort of stepping down, and I've always been an advocate, if you're facing some sort of shrinking resource, to try to do it in a very creative way. We need your input over the next four days to start thinking about it. Try to step back a little bit from advocating, if you're a killer whale biologist, advocating just studies for killer whales without regard of the whole process. So, if you can be a little bit unselfish and help us, that would really be appreciated. If you don't help us make these decisions, they're going to be made for us at some stage. So, I want to thank you again for coming. I did want to say a couple of words on the adaptive management process here.

We're in an annual cycle here, where we're, in this part of the cycle, we're integrating and reporting on findings. We're holding this workshop to try to get people's ideas, to share ideas, and to try to get the creative juices going for what's appropriate for next year, revise some of our ideas as the data from last year becomes available for integration. We're going to be then soliciting through the announcement from the work shop project ideas and projects. Those are going to be evaluate in an initial round of review, including some peer review. There's going to be a draft work plan, as usual, another round of review, and then some kind of approval for the work plan and funding by the Trustee Council later this year. These all processes are going to be taking place during the summer, of course, the implementation of work then by many of the people in this room, and finally brings us back to next year the annual meeting for formulating the work plan for 1996. So, keep this process in mind as you move through. So, thank you again for coming, and I look forward to your participation in the work shop.

The next part of the agenda, we have two different talks, one on a very holistic type of look at a marine ecosystem by Alan Springer, and then we're going to move on to, I think, a very interesting outgrowth of the research here, kind of a creative paper on the use of pristane and marine ecosystem as a marker for some production processes. Two different kinds of talks, but I think very interesting. Hopefully, will stimulate some thought. So, I would like to introduce now Alan Springer from the University of Alaska, Fairbanks. I've heard Alan talk a couple of years ago at a National Research Council workshop on the Bering Sea. He gave a very impressive talk about how the Bering Sea works and I think you'll find a great deal of interest as we try to map our plans for ecological research in the spill area.

ALAN SPRINGER: Thanks, Bob. Bob approached me to give this talk. He listened in, as he said before, and after I'd agreed to do it, I started putting ideas and thoughts into a little folder that I gave the short title to of "How BS Works" and my only hope now is that everyone doesn't leave here afterwards shaking their heads thinking to themselves, well, I know now. (Laughter) I was just asking, also, what does this have to do with anything. Prince William Sound is somewhat distant from the Bering Sea and I guess that's an appropriate question and I think the answer really is that, there has been quite a lot of work done in the Bering Sea over the last decade or two and we have gotten as a result an idea about some of the important spatial scales, our development in thinking about the ecosystem in the Bering Sea, and also we have a sense of the time scales that we wish we knew more about and we now are only beginning to get a sense of. So, I think the Bering Sea, in that regard, provides an opportunity to see some examples of the kind of thinking that is probably appropriate for everyone in the EVOS program in thinking about the studies that you intend to do now and especially in the long-term, thinking in an ecosystem context, and carrying forward this to understand important physical, spatial and time scales in the Gulf of Alaska and Prince William Sound. This will be by no means an exhaustive list of examples, it may be an exhausting list, I hope not, but when you think about marine ecosystems there are any number of kinds of things that one can talk about and to it justice. It's the matter of courses in universities and degrees in life times, and so don't expect to get a sense of what the full gambit of things is in an ecosystem. But, again, some of the examples from the Bering Sea. The talk I gave before the Bering Sea Ecosystem Committee, initially we were convened to try to come to terms with the same question that a lot of people in the past have been trying to deal with and continue to deal with, and it's a pressing management concern right now, and it all involves these guys -- sea lions, which you are probably all aware of have declined specifically in the Bering Sea and throughout the range in the Aleutian Islands, the Gulf of Alaska, the Western Gulf of Alaska, at least. The fur seals underwent a period of unexplained decline from the mid-seventies to the early eighties, and kittiwakes, red-legged kittiwake and black-legged kittiwakes and other sea birds of the Pribiloff Islands, have gone through a period of unexplained declines, and there was a consistency in the timing and the location of these changes in populations of these species. They were all fairly related in the trophic web of the Bering Sea. They were all fish-eaters, they're all higher trophic-level species. The areas in which they live were the same, and the timing of the declines that they experienced was the same. And, not only that, but in addition to those three species, harbor seals in the Gulf of Alaska and sea lions in the Gulf of Alaska also declined. So, over time there have been efforts to explain each one of these changes on the basis of just the population individually. People looked at diseases, people looked at entanglement in drift net, people looked at shooting, and pretty much through a process of elimination of all these possible causes, one was left with such confusion that none of them fit well and the likely cause in the case of all of these was that there was a food shortage. Now, that led to speculation of all kinds of hideous things; ecosystem collapse, a sick ecosystem were terms that were often applied to this. As a result of that and as a result of the inability of a couple of previous conferences to explain these changes, there was this most recent one, which is still in progress and the report for which should come out sometime this winter, and yet it was another attempt to look at what we know about the ecosystem of the Bering Sea and what could be responsible for changes that we see at higher trophic-levels such as these. Well, the first task of the committee it seemed as if was to try to understand what is the Bering Sea ecosystem. It's easy to talk about, well, let's go get into this Bering Sea ecosystem, but what is the balance of the ecosystem. Is it that area that's important to sea lion around the periphery or is it the foraging area of the sea birds around the Pribiloffs, or just exactly what, and can we talk about the geographic area of the Bering Sea, the area between Pribiloff Islands and the Bering Strait and from coast to coast as being a relative entity, or not. So, these were the questions that we first began to wrangle with, and that I am still, and I think everyone else, concerned about where you have to draw the line or where you can't draw the line in talking about the Bering Sea ecosystem. So, I want to talk about some matters of spatial scale, and then I want to talk about some matters of time scale, and I want to talk about some history. And, these are all examples, again, of the kinds of questions and kind of view you have to take when you go about understanding any ecosystem, whether it's the Bering Sea or the Prince William Sound.

So, matters of scale. When you're out in the middle of the ocean in a ship, in a big ship as this one is, you're just -- and you are a dock in a monstrous place, even in a small ocean or a small sea, like the Bering Sea. It's laughable to think that when you drop down a one meter plankton net out there somewhere and pull it up that you know anything at all except what you caught in one meter of that little ocean, and if you try to think about extrapolate that to a kilometer down there or a hundred kilometers over there, you're still in the same ocean, it makes you think what's the relevance. Now, on the other hand, if you step way back and look at the Pacific Ocean and where the Bering Sea and the Gulf of Alaska are, they're up here. They're minor little bays almost in the Pacific Ocean, and then think -- by sort of understanding about local atmospheric circulation, mean circulation, Pacific Ocean, it's hard to imagine that what you see anywhere in the Gulf of Alaska, Bering Sea doesn't represent some sort of much broader influence, and it may well be just fine to be here, or here, or there, or there, anywhere, because the scale that you're talking about now is so much different. The other thing is that sort of reinforces that idea is that over the big scale, the major scale processes over the whole Pacific Ocean, the North Pacific or the central Pacific, are important in regional considerations. It's when you look at such far-flung areas as Japan, the northern coast of California, of west coast of North America and west coast of South America. This incorporates a large question of the Pacific Ocean, you see some very suspiciously coincidental changes in species that occupy this whole area and it's hard to ignore the probability or the possibility at least that these species are responding in vast geographic areas to these same kind of force and functions throughout that range. So, again, you've got to take a broad, you've got to look at it from a really, I think, a broad perspective and focus down. So, focusing on spatially in the Bering Sea, the local concern, from the local perspective in the Bering Sea is that it's not all the same. The map I showed you to begin with, the picture is all blue, and it's all water, and its myopic. You can't see any difference just on the surface. But, the Bering Sea is famous for having such a broad shelf, about half of the area of the Bering Sea is shelf and about half of it is basin. So, right there you're partitioning the Bering Sea into two important geographic kinds of areas, Continental Shelf and basin. Very different processes occur there and important different aspects of this over the total ecology. During the 1970s a lot of progress was made during the PROBES era and the OCS era in coming to terms with the nature of the shelf. Here it looks like the Continental Shelf. There's very little relief from the coast out to the shelf break, it's a very gradual slope and then there's a very abrupt precipitous decline down into the basin. But, it's not a remarkable Continental Shelf except for its breadth, but nonetheless, there's an important lot of spatial hydeginia (ph) over the shelf, that's really important to biology. The Continental Shelf is partitioned into a series of hydrographic domains through the interactions of tidal energy and wind energy and bottom depth, and those interactions, those physical mechanisms form these physical structural fronts that occur pretty much coincidentally with the bathometric intervals. There's the inter-front, up there's pretty much the fifty meter domain, or fifty year isobath. Here inside the fifty meter isobath you have a coastal domain. The outer domain is pretty near the 100 meter isobath and out here the shelf break. The physical partitioning of the Continental Shelf gives rise to important biological differences that are important economically and important to the biomass production of higher trophic levels throughout the area. For example, the coastal domain has very low levels of annual primary production, very low production of higher trophic levels. The middle domain has moderate level of primary production. It's not well coupled to zooplankton grazers. Most of that production hits the bottom, supports the bottom mass, benthic stocks, crabs, yellow sole. That's where those fisheries are. In the outer domain you have somewhat higher annual primary production, but the important difference between there and the middle domain is that you have efficient large zooplankton grazers that pass that energy, provide for a mechanism for passing that plant energy up to higher lodging. And so, in the outer domain you have whole different sweep, or certainly a different balance of biomass among the various consumers. So, much for that level of geography. During that same period, and more recently, we've learned a lot about currents, the flow regime of the Bering Sea, and local patterns incyclic (ph) production are really important, but so is rebective (ph) regime in the Bering Sea. And, currents are pronounced and important there. When you look at total flow field in the Bering Sea, you can see that -- and this is where considerations of the bounds of the Bering Sea became really important, thinking about the Alaska stream, how it sneaks into through passes and how it pours in through Near Strait into the Bering Sea. It carries a huge volume of water, in the order of 10 million cubic meters per second of water comes in here. Lesser amounts flow through here. It establishes a circulation in western Bering Sea, it isn't very well described. But, beyond that there's some important circulation along the coast, fresh water, low solidity, warm water in summer that is continuous with the Alaska coastal current that comes down here, goes through Umiak Pass. It's modified all along by discharge from Alaska rivers and streams, warm during summer, and it's continuous, and can be traced all the way up to Chukchi Sea, goes around the corner and into the Beaufort. There's another current that's somewhat larger that goes along the edge of the Continental Shelf, the Bering Slope current. A portion of it branches and goes to the north. It's origin is deep, it's cold, high solidity, nutrient rich water. It floods through the Russian part of Bering Strait and into the western Chukchi Sea, in contrast, markably with this sort of coastal fresh water warm jet, just to the east. An additional feature of the circulation that is only now being sort of appreciated for what its possible role is all of this are these eddies. Eddies have been known to exist commonly for some time. In the Alaska Strait and now have been recognized as an important feature of the Bering Slope current, and they also figure in, importantly perhaps, in production biomass. Primary production, maybe, and even higher levels, although we begin -- this story is only coming through, developing right now, but at any rate, there is a lot of important circulation that has geographic significance. The current that goes north, the anody (ph), the Bering Slope current -- the northern branch, to give you an example of the significance of the flow fields can have in local production regimes, this was discovered in a subsequent study up in the Bering Sea, the Ishtah (ph) project in the 1980s. As I've said, this water originates from the ocean, it comes form the basin of Bering Sea. It has nutrient levels that are similar to what can be found in the deep water here, which are very high. It's carried up onto a shallow shelf which you wouldn't expect to be of particular productive environment except for the fact of the origin of this water. When you look at the level of chlorophyll, it's a major annual primary production up in the northern part of the Bering Sea and the southern Chukchi Sea, so you can see what the role of that is. Levels of daily production in the center of this, of these real high areas, have been measured as high as the highest level previously reported anywhere in the world. And so -- it is a big deal. So, again geography, you can't deny. The eddies I mentioned we're just now beginning to get a sense about eddies, another meso scale process. I showed you one, the edge of the Continental Shelf. This (indiscernible). I mean you've got a nice color image -- an eddy, along the edge of the shelf here just close to the outer shelf front, which is there on the edge of the Continental Shelf. It runs pretty much like this, northwest, southeast, and a larger eddy at another time in the year, which is just off Cape Naveron (ph), just off the edge of the Continental Shelf. We look at those eddies, the cross-section to the eddies you can see how the chlorophyll values change as you cut through you can see them fingering, the banding there as the thing spins and really high levels in the center of it. Over here again, a very broad eddy with very enhanced levels of production that we're not clear about how this works, but there is evidence that it's important. The Bering Sea (indiscernible) people have recently discovered that it's uncommon to find groups of larvae pollock in the southeastern Bering Sea anywhere except in eddies, and so there may be a mechanism here where production is -- either primary production is enhanced or accumulated and facilitated (indiscernible). So meso scale process are very important. From the work that was done during PROPES and the OCS program and from the work that we did in Ishtar project, one can sort of draw a very schematic map of what the likely production distribution is in the Bering Sea. And, again it really illustrates the nature of the place, that it's not sufficient to go out there just anywhere and do some sampling and expect to understand what's going on. You have to be very aware of the nature of the shelf, the partitioning by hydrography, the nature of the currents, and those kinds of things to be able to see areas of important, primary production rates, of low production, and not only that but the levels of primary production are often very coincident with their biomasses accumulated and produced at higher trophic levels, and those are the kinds of things that lots of us are interested in. For example, Ted in his work out there in the 70s found that in that same area where we're calling for now the greenbelt, on the edge of the Continental Shelf, which seems to be particularly highly production with phytoplankton. That's also the region for zooplankton production. It's the highest in the whole southeastern part of the Bering Sea. As you take these transections, basin is low, you get the self edge and it's very high and it declines as you go in shore. We saw that up in the northern part of the Bering Sea that same current that is responsible for the very high levels of primary production, also carries an immense amount of zooplankton biomass up there, and those patterns of distribution of biomass zooplankton are probably reflected in the distribution of higher trophic levels when you look at them. This is somewhat hard to see, but these are fish distributions. This is where the commercial catches of pollock, Pacific cod, Pacific Ocean perch, and sable fish. If you look at where the distribution of the major catches of these fishes is, it's where the fishes are. That's where the fishermen go fish, they go to where the fish are. The fish go to where the food is and the food is, for the most part, in the outer domain along the edge of the Continental Shelf. These are pelagic systems. They don't fish yellow fin sole there, there from the middle domain. Going another step up, when you look at sort of the overall pelagic distribution of seabirds away from the colonies in the summer in the Bering Sea. They also fall out nicely. This average density summer long. Other than this, you can see how the edge of the Continental Shelf, the area of high primary production, and obviously apparently high secondary production, fish production, it's real important to sea birds. Not only that, but it's important to marine mammals. Back in the days when they had pelagics first sailing around the Pribiloff Islands, the area where they got all of the majority of fur seals was offshore, south and west of the Pribiloff Islands. They had an explosion around 60 kilometers around the island -- 60 miles around the island, so they couldn't fish, or they couldn't harvest animals, but nonetheless the direction they went was offshore, and when there were bowhead whales, the resident stock in the Bering Sea back in the last century in the 1800s, you can see that the bowhead whales were concentrated up here, off Cape Navarron (ph) along the western edge around the Gulf of Adnodear (ph) and up into the Chukchi, likely -- strongly related to the eviction of zooplankton biomass out of the Bering Sea and into these other regions. This is not news. This was known about in way, long ago, and writing about the fur seals during the pelagic fur sealing there in the last century. Frederick Lucas said that the most frequent feeding grounds is indicated by the logs of the pelagic fur sealers, like from 75 to 150 miles southward in the eastward and to the northward and westward to the Pribiloffs, some little distance outside the 100 fathom line, or where the bottom of the sea gets it roughly downward, from 500 to 5,000 feet -- the edge of the Continental Shelf, the greenbelt. That's where the fur seals were. The reason, he went on to say, that the reason they we there -- is an examination of the chart will show that there seems to be a direct relation between 100 fathom line and the localities where the seals obtain food. That the conditions affecting the feeding grounds can only be learned from a long and careful study of the depth and temperature of the water and the set of the currents, since these are prime factors in determining the presence and distribution of marine plants and animals, which may be called food minutes, and on which all higher animal life ultimately depends. So, people have been looking at this kind of thing for a long time, having insights about it, and it has remained until just recently that, it's sort of a (indiscernible) picture. But, again, I think what the point of this is, is that it demonstrates again that geography, the spatial scales of reference that one has to approach ecosystem studies from, or when trying to make sense of anything in the area.

So, time -- another couple of examples about time scales of reference. Unfortunately in Alaska, in the Bering Sea, Gulf of Alaska, our time scales are pretty short, with many vertebrates, the birds and mammals, our time -- the beginning of time for us is about 1975 when the OCS program began. And, there are some examples, fortunately, from before then, but not many, and for the most part sea bird, marine mammal research, a lot of the fisheries stuff goes back before that, but again, a beginning of time is then. (Aside comments) We don't have a sense of history like has come from the examination that Tim Baumgardner (ph) has done in the Santa Barbara basin. Looking at what the nature -- what a time scale really is. He's looked at 2,000 years of history of fish populations, of sardines and anchovies, well, I showed you the recent history of sardine population fluctuations across the

Pacific Ocean. Those are not recent phenomena. Those kinds of things have been going on for 2,000 years. Periods of high and periods of low -- abundance of both species. Well before the advent of human commercial fishing activities these populations were fluctuating widely in the environment, and it implicates a physical dynamics there that draws those things as the likely underlying mechanism. But we don't have anything like this for up here. We have some sense though about the variability over time in the ecosystem, and a lot of it apparently seems to be driven by processes in the North Pacific, especially in the Gulf of Alaska. These things are important for the Bering Sea, but originate somewhat south. This is sort of the picture of what the atmospheric circulation in the North Pacific looks like, that Aleutian low. Typically, you have low pressure, the Aleutian low, during winter in the gulf or the North Pacific depending upon the year, and it's replaced in the summer by a weak high. Well, it's the low that's persistent and powerful and it seems to have a major influence over a lot of biology out there. A lot of physics is driven apparently by the Aleutian low, in (indiscernible) biology falls. The Aleutian low is not a stable kind of thing. It doesn't live right where you see it, and it doesn't -- it moves to the west and it moves to the east, and it is stronger or weaker depending upon era, and as you look at the sea level pressures, deviation from long-term mean, for the last -- well, since 1930, the mean condition, the mean sea level pressure has wandered quite a lot, relative to the means, some periods of somewhat below, somewhat above, the long-term mean. When you apply the intervention analysis model to this, which Bob Krantz (ph) and his students have done, you can see that there are distinct, what they refer to as regimes, in the Aleutian low and pressure field over the North Pacific that is not regular entirely, but is very characteristic of intervals of generally above normal pressure, followed by intervals of below normal pressure. And, the most recent (indiscernible) has been considerably below the long-term mean, and that is to say that the Aleutian low has been particularly intense and displaced to the east. A sense of the difference -- of the magnitude of the difference in this mean atmospheric condition in recent times -- during this last regime which began statistically in 1976 is that sea surface temperatures have been more than a half of a degree below normal, low long-term normal, in the center of the drier -- sea level pressure has been, six millbars below the long-term mean. And, those are big differences, and they have led to important changes apparently in biology. One of the things that's been associated with this change -- with a strong Aleutian low in the -- sort of displaced to the east is that there has been a lot of storminess, a lot of high winds from the west, which have been cold winds. It has -- those two things have tended to perhaps lead to deeper mixing of the upper layer of water column, led to higher -- this is also theoretically, but to higher levels of important nutrients, and higher levels of primary production. If you -- the phytoplankton data are not as nice as we would hope, but beginning in about this time, the mid-70s, as I said, this regime shift, the statistical time of it was 1976, but these things are not abrupt entirely, the statistics are abrupt but the trend is not. But, beginning about in the mid 1970s, and a lot of the changes that we're looking at began, and compared a little bit of information from the prior regime when the Aleutian low was weak and displaced farther to the west, it looks as if a fairly strong case can be made that annual levels of primary production increased in this most recent period. In addition to that, if you compare two different intervals taken during the prior regime, then the era of (indiscernible) Aleutian low displaced to the west from two different time periods, 56 to 59 and 60 to 62, those are both in the last regime. Secondary production zooplankton biomass was low by comparison to the recent apparent abundance of zooplankton in this most recent regime, and we have a strong Aleutian there displaced to the east, and apparently high primary production. And so, these kinds of things are obviously important considerations for the Gulf of Alaska, but because of the way the circulation goes, it's important to the places along -- all around the perimeter, and ultimately into the Bering Sea. Changes like that in lower trophic levels in the basic production region -- phytoplankton, zooplankton -- may well translate to or explain -- in response to the changes in atmospheric and oceanic circulation may well explain the changes that have apparently occurred in populations of salmon, for example, in Alaska. When you look at -- when you take the trend in salmon catch from back in the early 1920s and do the same kind of intervention analysis on it, the statistical regime shift periods match almost exactly with those from the atmospheric intervention analysis and you find that periods of intense Aleutian lows, high primary production, high secondary production, in the same periods when -- high levels of salmon catch for all these species, which are presumably spawning along the coast of Alaska and eating and spending a lot of time out in the Gulf. So there seems to be some kind of energy trail up the food web in response to large scale atmospheric forces. Those are important considerations for the Gulf of Alaska, Prince William Sound kind of questions, as well as the Bering Sea.

Now, circulation again -- try to bring this back to the Bering Sea, the relevance for all of this -- let's get it straight -- that's the Gulf of Alaska, we're supposed to be talking about the Bering Sea, well, it has relevance to the Bering Sea apparently. Circulation comes around to the North Pacific current into the North American Continent, bifurcates with a part of this coming down bathing the coast of western North America, temperate North America, and the other forming the Alaska current, the Alaska stream, that comes around in here, and this -- the proportion of the waters that flow north or south were not constant, they've changed, and why they've changed is not exactly clear, but it has to do undoubtedly with (indiscernible) transport and with the nature of the atmospheric and circulation fields over the Gulf. And, the distribution zooplankton is the Gulf of Alaska apparently is not uniform either, but tends to occur around the perimeter coming in the flow, to the current coming around here, and then it seems to be carried along, as would be expected, in the two major forks of this thing. This would be consistent with Ted's view of (indiscernible) in the middle of Alaska gyre and on-shore transport during (indiscernible) zooplankton that are injected in the (indiscernible) flow out from the center out toward the edge. You would get doming between the dynamic center of the Alaska gyre, you get up welding, you get higher levels of primary productions, you get maybe downstream, as it were, transfers. But, nonetheless, the map -- many years ago they map the distribution and it appeared to be thus. Well, this stuff gets entrained in the Alaska current, in the Alaska stream, and when you look at the proportion of the water that goes north from this point compared to the part that goes south, and compare it with zooplankton distribution in the western Bering Sea, it appears to be very closely related. The more water that goes south, the less zooplankton there is ends up in the western Bering Sea. So there's the connection to the western Bering Sea and that's why when we were thinking about, well what's the Bering Sea, can we talk about, you know, the Aleutians to the Gulf to the Bering Strait, you really can't. You've got to think about the kinds of processes that are occurring in the Gulf as one example the role of currents and local production in the North Pacific in its relationship to stuff going on in the Bering Sea.

Now another time scale that we have for the North Pacific, in this particular case, the Bering Sea is just trends in water temperature, and there seems to be a pretty well pronounced -- like

18.6 year oscillation in water temperature that Tom Lawyer (ph) has done a lot of work on. It's come to be known as the VLF or the very low frequency temperature signal, and it's pronounced in the Gulf of Alaska, the Bering Sea and up in the Chukchi Sea. Pretty much everywhere north of -- I forget -- some degree latitude, 40 degrees or 45 degrees. Below there it kind of peters out. But, all through the Gulf and up in the Bering Sea it's a very clear signal, and he has explained it as a response to an oscillation in the orbit of the moon. This isn't, I think, very necessary wholly accepted, but it's correlates very well with the period of the Earth of mean and low tide, but whatever, that's a whole other story. The point is that water temperatures do vary, they are not constant. And this time scale may or may not be important to the Bering Sea, but it's interesting to speculate on whether it is or not. Up in the Chukchi, that temperature oscillation, we have a shorter record of it, presumably it tracks -- tracking time the same, but you only know about it from the early '70s. That temperature fluctuations in the Chukchi Sea appears to be very important. If you just look at or correlate one biological (indiscernible) which is kittiwake productivity, there seems to be a very compelling relationship between the amount -- the number of chicks that kittiwakes are able to produce in warm years and cold years. Warm years are good and cold years are bad, and it's not really surprising that that should be the case. These are -- these birds are feeding or nesting along the coast of the Chukchi, they're feeding in that Alaska coastal current that comes up, it's coastal zooplankton community, it has low levels of annual production, it has a suite of species that are very responsive to the normal characteristics of the water, the zooplankton are -- the fish predators the birds are feeding on, are also -- when you look at the magnitude of change that occurs up there between cold years and warm years -- this being a picture of the Chukchi Sea coast in mid-summer of a very cold year, 1976, and compare that with 1979,, you can see that it's a whole different world. It's not surprising the biology changes along. If you look at the relationship with kittiwakes again to water temperatures in the Bering Sea, it's just the opposite pattern. It's not intuitive why that would be, it is, as you know, based on what we think about the response is in Chukchi, but nonetheless during this last period, and as I said, we're hampered by the fact that at the beginning of time for us is right here, 1975, for sea bird stuff and for many other things. We don't really know what went on before that, but if you simply look at the relationships, since we know what's going on, since then as warmer temperatures after they bottomed out in the mid or early 1970s and began to increase the kittiwake productivity on the Pribiloff Islands began to decline and bottomed out by the time that water temperatures reached their highest levels in recent times. As temperatures began to cool again, falling on their cycle, the kittiwake productivity began to increase. Now, one would like to think that there's a cause and effect relationship there, and certainly if there is, it's mediated through the food web. (Aside comments)

If you look at the relationship then of -- one of the concerns that we have, as I pointed out earlier, is that kittiwakes were one of the species that have declined. If you look at the relationship with the productivity through, the kittiwake numbers on St. George Island where we had pretty good information, there may be a reason to hope that the numbers which did decline precipitously here during the late '70s, early '80s, bottomed out, may be responding to increased productivity and beginning to recover. Certainly they have been punctuated, but nonetheless coming back up, and maybe recovering, and so again, maybe there is -- maybe this is a compel -- a real biological corollary. If you go to the Gulf of Alaska you have additional support for the idea that there is a relationship between kittiwakes, in some way, and water temperatures. An interesting difference between the Gulf of Alaska and the Bering Sea is that water temperatures in the Gulf follow that same pattern as they did in the Bering Sea until about the early '80s when they peaked out. But then, unlike the Bering Sea which cooled as "it was suppose to" the Gulf of Alaska didn't, it got stuck in high and stayed in high pretty much every since. Now, there's some indication that maybe it's cooling off now. But, it's an important difference in the pattern of the physical events in those two places, and it may help explain a striking difference in the pattern of biological response, that is to say the kittiwake productivity in the Bering Sea seemed to follow the trend here, and it seems to follow the trend in the Gulf too, if there is a relationship, and that is to say it declined as water temperatures rose in the Gulf and it stayed nearly rock bottom during the whole duration of this warm trend.

So, the time scale, one of the time scales you can look at. As I said, it's probably mediated through the food web. If so, this may be one of the connections. -- capelin. This is becoming the now famous Capeland Davis set of Paul Anderson from Pavlock (ph) Bay. Nonetheless -- and it's a small area but it's probably may well be representative. If you look at the relationship of the sea water temperature and Gulf to capelin abundance, you can see a very striking relationship that as the water temperatures rose in the Gulf, capelin abundance declined precipitously here. Capelin, as it turns out, is an important dietary item of sea birds down there and may be representative of the changes that occurred in the food web. Not only that, if you look at harbor seals, which plummeted on Togiak (ph) Island, and lay that on top of the capelin curve, again, there was a very -- these two events certainly occurred at the same time, whether or not there's a cause and effect relationship, that is to say, the decline in the capelin precipitated the decline in the harbor seals, you have to be shown or proven conclusively, but nonetheless, there is a compelling case to be made that whatever was driving these things, drove them both at the same time. Finally, again, kittiwakes a similar thing in the Western Gulf of Alaska, kittiwake reproductive success seems to be fairly well correlated with capelin abundance. So, we get changes in proportion to capelin abundance and maybe not necessarily because of them.

Now, a final consideration, an example to give from the Bering Sea about scales of reference is the historical context. When you go out today to look at an ecosystem, it is the way it is because of its history. Events have occurred in the physics and the biology that have shaped the way the community is, the system is, its component populations, and its behavior. It is the way it is because of things that have happened in the past. Now, one of the phenomenon that has happened in the Bering Sea, that people paid particular attention to, is the phenomena of pollock. Pollock has been the target of a lot of speculation concerning the ecosystem and how the ecosystem operates. The phenomenon is this, that I'm speaking of, is this apparent increase in the abundance of pollock since the early 1960s. The commercial fisheries for wildlife pollock began about there, and from then on we've been able to get reasonably good, sometimes good data on pollock stock abundance. These are model estimates of the trend in the stock of the total population in Bering Sea, and as you can see from the early 1960s when the population was down around one to two million tons, it went through a general increase up to the mid-'80s to nearly, depending upon which model you look at, 15 to 20 million tons, or an increase of an order of magnitude of abundance. This isn't entirely unprecedented for fish populations which go through these kinds of things, but it certainly was for the Bering Sea. There is no other recorded time when any population of fish or any population of vertebrates, for that matter in the Bering Sea ever approached 20 million tons. Now, it declined some following that, it went down to maybe a low of seven to ten million tons, and it seems maybe it's been recovering. But nonetheless, this thing occurred during a period of important changes in the physical environment and other changes in biology and people have been looking at the role the pollock might have played in that. Using a conservative estimate of the abundance of pollock and their consumption, one can see that they do play a role in the balance of biomass that's in the Bering Sea and its availability of the consumers. The consumption of pollock increased during that period of time, and if you simply compare that to what the published estimates of production, of secondary production are for the Bering Sea, you can see that they were consuming an amount equivalent to the estimated annual secondary production of the outer and middle domain, and approaching the total estimate of production for the whole Bering Sea based on, again, what our current -- what out belief of the production values were. Well, they probably don't eat that much, but it points out that we've got a problem in reconciling production and geography. Nonetheless, I think even if you adjust some of these, then the pollock still is an import deal. When you look at the distribution of pollock in their range, you can see that it's a broad range from Southern California, around the North Pacific Rim, all through the Bering Sea. You can see the pots and all of that, which is like so many things, they're not evenly distributed throughout their range. They're concentrated in the southeastern Bering Sea, the majority of the pollock's population for the whole area spawns there. A huge amount of production spawning occurs along the edge of the Continental Shelf under this greenbelt area, and so the affected area -- the area that they are really affecting is not uniformly distributed, but is from a very small area. What could have caused these changes in the pollock population that may have had a role in structuring the ecosystem later on? People talked about commercial fisheries, harvesting adults which are cannibalistic and allowing for greater production. There is change in the physical environment that we know are influential in fish population dynamic. And then there's this -- again, another historic context, and that's the change in the numbers and the abundance of other species of higher trophic levels in the Bering Sea. The -- Lawton (ph), several years ago said about herbivores that they were caught between the devil and the deep blue sea and I think that could really be applied to anything out there that's not a top level consumer, that they're dependent on the production that supports them, and they're also worried about whose eating them. Well, one of the former big eaters in the Bering Sea and the Gulf of Alaska were whales, the large baleen whales and sperm whales, and there has been -- there was really a remarkable change in the whale population recently. A lot of people think about whaling going on in the 1800s. Well, there was a lot of whaling going on in the 1800s. They killed all the bowhead whales; they killed all the thin whales or the light whales out of the Bering Sea back then. But only recently since the Second World War was there resurgence of whaling and they got a huge number of whales. In the whole North Pacific they got approaching 300 thousand whales in a very short period of time. A huge amount of that whaling in the northern part of the Bering Sea -- I mean in the northern part of North Pacific, in the Gulf of Alaska and the Bering Sea, occurred during a 10-15 year interval in the early 1960s to the early 1970s. And, this rapid removal of whale biomass conceivably had a role in changing the balance of prey -- available to other species. You look at thin whales, you look at the docks. A whale was harvested in each one of those docks so there were -- were just one species taken from a broad area of the North Pacific. But, when you look at most of them came from, they weren't uniformly distributed. The majority came from areas that were important and those were areas where there was food, and one of the big areas for food was in the southeastern Bering Sea for pollock -- live and spawn, where there's a huge amount of production along the edge of the Continental Shelf. So that the role of the whales played in the Bering Sea wasn't uniformly distributed over the whole area, and indeed was concentrated in areas which is now occupied by pollock. And, so when you look at the -- let me just make it a rough estimate of what the consumption of whales was formerly before they were removed. You can see that if you take the three species that were most heavily harvested in the Bering Sea, the side whale, the thin whale and the sperm whale, and just sort of estimate what the annual consumption might have been. It approaches about 100 thousand tons per day of consumption of prey biomass that was freed up as a result of removing these whales. Well, a 100 thousand tons per day is about -- at the equivalent of 500 million tons of fish eats. And so, there was a release to the ecosystem at that amount -- at that magnitude, which may well have had a role, it may not explain recent changes, they may be involved in the recent changes, and it's something that you need to consider when you're talking about ecosystems. Beyond that, there were at that same time the whales were being taken out, there was unreplaced removals with additional amount of biomass in the form of dead fishes. The sperm whale reductions in the Bering Sea would have amounted to about 2 million tons in about ten years; thin whales, about a half a million tons; yellow fin sole off the middle shelf, nearly a million tons; Pacific herring, over a million tons; and Pacific ocean perch, nearly a million tons. These were all removals without replacement. That is not the annual production of those species that was being removed, and the stock was being maintained. Everything was taken. And so, at the end of this era, from about 1960 or the mid-'50s to the mid-'70s, about twenty years at the most, nearly five and a half million tons of biomass, of higher trophic level species was removed from the Bering Sea, very abruptly in terms of evolution and ecosystems and evolution of this whole thing. The role that that might have played is a historical phenomenon in what we perceive to be the ecosystem we see today, it can't be overlooked. And so, I guess in closing I'd just like to say that when you look at a place like the Bering Sea, and it all looks blue, it ain't all blue, it's various shades of blue, and it really matters where you are and when you were there in trying to decide what it is that's going on now and what you need to plan for off in the future.

DR SPIES: (Aside comments omitted) ... Now we move from the geographic system prospective down to the level of molecules and perhaps we'll back up a little to the trophic system is consideration of pristane and natural hydrocarbon trace energy levels in calanoid copepods, higher trophic levels in the Prince William Sound ecosystem presented by Jeffrey Short of the National Marine Fisheries Services.

DR. JEFFREY SHORT: I wanted to give this talk to you this morning because I wanted to bring your attention to a conjecture that was made in 1964 that, if correct, may provide a powerful new tool to explore the food web in Prince William Sound. In 1964 it was discovered that large copepods in the genus calanus, pretty much only those copepods, manufactured an alkane hydrocarbon called pristane and these copepods are the major modern source in the marine environment of pristane. The conjecture was that pristane would label virtually every part of the food web that was connected to these copepods through predation, whether directly or indirectly. Happily, the hydrocarbon database built up after the Exxon Valdez oil spill contains an enormous volume of data that clearly validates this conjecture of the Prince William Sound. In the rest of my talk, I will briefly review the bases for the conjecture and then I will summarize the results of the database that show pristane finding its way into such diverse departments as birds, marine and terrestrial mammals, and even plants and sea urchins, filter feeder bivalves, and fish. I'll conclude with some suggestions on how you might use this tool in Prince William Sound in the future or by studies, and in the northern Gulf of Alaska. I'll also add along the way some data that we've generated over this last year at the Auke Bay Lab that's filled in a lot of the gaps in this story.

So to get started, I want to begin with how this all came to my attention, and this all occurred in 1992 when I was working up data from a baseline hydrocarbon study we did in 1977 through 1980 when the oil terminal opened at the Auke Bay lab. We kind of felt their might be some pollution at some time in the future, so we thought it would be a good idea to determine what hydrocarbons levels were in Prince William Sound prior to (indiscernible), and that turned out to be a real good idea, began a four year study, and the data came and sat around on the shelf. The study was mussel contaminants until the Exxon Valdez oil spill and then there was a big crunch to work up the numbers to see what was happening. In the course of working up those numbers, I noticed something really weird, and what it was, was that in mussels, not in sediments, just mussels, in May there were some pretty astonishing concentrations of pristane in mussels at several of the stations we monitored. First showed up in 1978, and they disappeared in August, and then it was kind of repeated in 1979 and 1980, and after I started looking at hydrocarbon data in the Prince William Sound -- Exxon Valdez database, I noticed the same thing happening then too. Every spring and May there would be these high concentrations of pristane and all sorts of -- they would go away by August and they would be fairly undetectable through the next following March, then they would go way up again. I thought that was really weird. So, I did a literature search and looked for what was known about pristane and the marine environment, and to my great relief, I found this key paper written by Max Blumer. Now, to a hydrocarbon chemist, Max Blumer is probably a well-known name to you. He was pioneer in the field for analytical chemistry for hydrocarbons (indiscernible), and in the, you know, the biogeochemistry. He was very interested in the distribution of hydrocarbons, all kinds of hydrocarbons in the environment, and he did a lot of his work in the '50s and '60s. Lucky for me, he did a lot of work in the pristane -- he did a document on it -- half a dozen papers and many of them appeared in the Journal of Science, and the bottom line was he determined that there were few primary sources of pristane in the marine environment. One was calanoid copepods and the other was petroleum. He subsequently, after this paper, he demonstrated that calanoid copepods can indeed make pristane, and to this date they are the only known organism for which that has been proven. This is taken from his work, (aside comment), it's from 1964, and he found quite high concentrations of pristane in copepods. These represents eight-tenths -- almost one percent on a dry weight basis. It's noteworthy in and of itself. The other copepod species he looked at, including calanoids that weren't in the genus *Calanus*, but at least they were a magnitude lower and usually two or three orders of magnitude lower. He determined that either these copepods were making this stuff internally or bacteria in their intestinal tract was making it, it was one of the two. I kind of think probably their making it internally. The reason they make this stuff -- this is where pristane comes from -- the reason that they made this stuff, appears to do with density control. If a copepod makes -- for every milligram of pristane the copepod makes, it increases the animal's buoyancy. So, when it's undergoing a period of starvation down deep in the water column in the winter, it doesn't have to swim as much to stay there, to keep from sinking all the way to the bottom. If it makes a bunch of pristane then it can use 1.6 milligrams of additional lipid. So, it's an energy saving devise apparently that these copepods undergo. Now, these copepods are quite high in lipid content, which makes them a very attractive prey species for lots of things, and the concentration as we'll see later of the pristane in the lipid basis approaches 2.3 percent. Now, right now, I want to convert this number into parts per billion, and you'll see why in just a little bit. This would be 22.8 million parts per billion. The reason that I want to talk from hereon in parts per billion is because the -- in the ecosystem, when we get to looking at pristane in the Prince William Sound ecosystem, we'll be looking at concentrations in the order of 92 billion. This is where Blumer determined pristane came from -- it's derived from chlorophyll. These copepods eat -- they're herbivores and they eat hydroplankton and that means they ingest chlorophyll and chlorophyll pretty readily leaves right here to form phytol (ph). But, you can't just get from phytol to pristane by kind of hanging around and falling off the log. It takes a lot -- you have to go to a lot of trouble to do it. Specifically, you have to oxidize this with a carbocyclic acid, then you have to lose this when it decarboxylation, then you have to reduce this double bond, three steps inside the animal to get to this step. So, they go to a lot of trouble to make this, and it's unlikely that many other things also go to this similar amount of trouble, particularly in that this all costs energy to do. Again, it gets the energy back, of course, later when it depletes its lipid reserve. Well, all that work occurred on animals that were in the North Atlantic, so earlier this year, courtesy of Dr. Cooney and some people in our lab, we got some animals from the Pacific and Prince William Sound and analyzed those at the Auke Bay lab for pristane, and sure enough animals -- large copepods in the genus, either *Calanus* or *Neocalanus* had comparable concentrations of pristane on a dry weight basis. So, they're relevant, by the way, for us in Prince William Sound, and the work that was done by Blumer is direct and applicable. *Calanus plumchrus*, I want to point out, is the animal that shows up in Prince William Sound in pretty great numbers in May, early spring. Well, I want to focus on what an ideal -- chemical tracer this molecule is. First off, it's a hydrocarbon, so it's pretty inert, fact it's real inert. It's also highly lipidophilic, that means that it will go in the food web in the environment every place that fat goes. Secondly, it's got these branches at the end, on both ends. That's real handy too. When normal mammalian or fish metabolism encounters this molecule, if it didn't have these branches, it would start whacking off the carbons two at a time and the enzymes that does that is looking for two carbons in a row. The presence of this branch messes up that enzyme, consequently this is persistent in the animal. It hangs around for a long time. It resists metabolic (ph) degradation. Consequently, the situation here, it's not unlike that of DDT, once it's in the food web it concentrates as it goes up and it hangs around for a long time. But this is naturally produced. It's not toxic, its just kind of a passive label, but, it's like a little magic marker for every thing that eats it. Another very highly desirable feature of it, is it has a low detection rate. You'll recall that the -- in the garden variety *Calanus* that I showed you from Prince William Sound had about 8 million parts to a billion in it, and if we try hard we can detect about 80 parts per billion. That gives us five or six orders of magnitude, dilution that can occur in the food web before we can no longer see it. That means it can go through lots of trophic levels, and you'll still the difference. Finally, it doesn't break the bank to analyze the for it. On a production basis, this is kind of a rough calculation, but we figure we can do these with about thirty bucks a pop. So, it's not like VCMS where you have to have contact with some fancy laboratory in Newark to analyze these things. It's really a pretty straight-forward simple procedure. So, Blumer recognized all this stuff, and he ended his paper with sort of --and I'll read you the following quote from it. It is:

"It is possible that the transfer of pristane from *Calanus* to its predators, such as (indiscernible) and those species, and to larger carnivores can be used in studying the dynamics of the marine food chain," and this is from his paper an example of how he thought it might cycle through the marine food chain, and as you will see it turns out this not a bad first guess for having no data. This was done in 1964, and the trouble was is Max Groomer was a biogeochemist pretty much, and he sort of angle this out there for the biologists in the ecological community to pick up and run with it, and nobody did. The reason nobody did was because it costs a lot of money to go out simultaneously sample all of these different kinds of species and then analyze them all at once. Your talking millions of dollars, and who knows if this conjecture is right, so nobody did it, except for us. We actually did it, with the Exxon Valdez oil spill. During the oil spill, we collected 3,000 samples of fish, birds and mammals, that included over 50 species, it included more than 20 different tissue types that were analyzed, and lucky for us, pristane was one of the exquisite analytes that was measured. This is by far the most complete synoptic status of pristane for an ecosystem that I believe exists on the planet. Now, what I wanted to do with the remainder of the talk is kind of explore this aspect with you, but I wanted to start off with some caveats that we have to be careful with. Essentially, what we're going to be doing is, trying to -- optimum data foraging, and kind of doing some database diving to get data back that was collected for entirely different purposes, and because of that we need to be aware of some limitations in the database. First off, the tissues that were analyzed were not always comparable amongst different species. Species that were collected were often collected in -- at different times and in different places that were not always comparable. And then the amounts of tissue that were analyzed were not always the same, for shellfish, often there was ten grams of materials analyzed. That gives you a pretty low detection limit. Unfortunately for a lot of the same marine mammals, maybe only a tenth of a gram was analyzed. That gives you a detection limit around 100 times higher, which kind of compromises your ability to seek the lower end in those cases. That's probably -- I'm going to focus on three basic tissue types to brief our way through this 3,000 database. One will be stomach contents, because if you see lots of pristane in a stomach then it kind of indicates that it has eaten something -- that it ate something that came from a calanoid copepod eventually. The second tissue type is a (indiscernible) and that's because neither fat or blubber or in many cases liver tissue has associated lipid with it, that serves as a long-term storage depot for pristane, and it gives you more of a long-term dietary dependence indicator, compared to stomach contents which is basically what the animal ate that day. And then thirdly, reproductive tissues, because that gives you an idea of how critically an animal depends on this stuff during its reproductive phase. One of the first things I did when I was exploring this aspect was ask myself the question, well, who's the big winner in the sweepstakes, i.e., who has the highest pristine concentration of any animal. Well, we were surprised at the answer. It wasn't what I expected. The winner was the fork-tailed storm petrel -- it's stomach contents. It's stomach contents ranged up to 24 million parts per billion. A figure that, you will recall, is consistent with the highest value I showed you earlier for pristane and the oil associated with *Calanus* copepods, i.e. this suggests that the stomach contents of these storm petrels, or at least that one, was eating strictly calanoid copepods, and all of its oil was derived from that. Well, what I am showing you here is a large scale, because we have so much analytical depth to cover, and height of this bar is the median value of this many samples. So, half of 37 samples were higher than this; the other half were lower. I did it this way for a lot of reasons, one was because for most of these things there is a lot of variance on account of we're looking at animals often that were collected from different places, maybe even different years, different seasons, a lot of variability, but the medians tell a very interesting story. Also interesting here was, there seems to be a great dietary dependance of these animals, again reproductive phase. Their egg content had a median value that was nearly a million parts per billion of pristane, one-tenth of one percent. That suggests that a large energy for reproduction for these animals is directly dependant on predation of calanoid copepods. By the way, these 37 animals were collected off East Autiuk (ph) Island in 1990, during May, at the time when *Neocalanus plumchrus* food fish is the highest. So, that was the story for fork-tailed storm petrels. I then looked at the related -- direct predators on copepods. Now, what -- I'm going to show you four, your undoubtedly other direct predators on the calanoid copepods, your not going to see because during the Exxon Valdez oil spill, nobody sampled for of them and analyzed them for pristane. We just have to take what we can get out of that record. The other four species we saw in there were one short-tailed sheer water, collected as part of the criminal effort, 16 juvenile pink salmon, and 42 herring, and these are all their stomach contents. Now, notice that the highest values in the stomach part -- well first of the short-tailed sheer water, its stomach content is very similar to that of the storm petrel, and I think that reflects some of the feeding behavior. Among the two fish here, the juvenile pink salmon and herring, they have lower medians and lower upper limits to their range, but -- and I think what that reflects is less dietary dependence on the calanoid copepods. It's still pretty heavy because this is what's in their stomachs. The next couple of slides, I'm going to show you what's in other tissues in these animals. You've already seen what is in the egg tissues of a forked-tail storm petrels, and we only have one liver sample, but it corroborates the stomach sample for the sheer water. It's up around 800,000 too. Look at whole carcass values of the fish, these are critical values here. They're critical because they they're sort of -- the main reason their critical is because they what you would expect to find in stomach contents of a piscivore, if it was eating an herring, that is, not only does pristane-labeled copepods label everything that eats it, but for herring --

this is muscle tissue in herring. Note also that the variance is quite a bit lower. It's actually even lower than this one sample of these ten that counts for it being even as wide as it is, and the other samples they generally are very varied by factor too, or it's in herring that were collected from same places at the same time. So, these values here, 100 parts per thousand -- or 100,000 parts per billion, is what we would expect to see stomach contents of predators on herring. Among the avian piscivores, that is indeed the upper limit of what we see in many of the species. In the stomach contents of bald eagles, flat kittiwakes, common murre, the highest values range up to about 100,000 parts per billion, and that indicates that that day, those animals ate a herring, or something like a herring, that had a single concentration in its parts. The relationship to the reproductive areas is similar to what it was for the sheer water and forked-tail storm petrel. Concentration that were transferred to the eggs was rather soon, in the case of bald eagles and flat kittiwakes. The liver tissues are rather higher. The medians are higher and the highest values are rather high. They get up to around a million, and that reflects, I think, the lipid associated with the liver that is storing pristane over a longer term. Among the fish piscivores that were in the database again paperback, dusty rockfish, Pacific pollock, stickleback and general the median values were substantially lower than they were for the avian piscivores, and so are the upper limits, although the upper limits still is right at that 100 parts per billion values. The fact that the medians are lower, I think, reflects wider dietary diversity in these animals styles. And, the other reason that they are lower is because these are carcass values, these are the entire fish ground up and analyzed, rather than specific tissue. Finally, it showed up in some of the mammalian piscivores, actually all of it, basically all of the mammalian piscivores. Harbor seal and sea lion both had substantial concentrations present in their fat tissues. I think it's on account on they are mammals. Physiologists will let you know better than I can tell you. Let you know better than I can tell you whether there is a reason why mammal livers and large fatty (indiscernible) livers -- I don't know if that's true or not, but the values are lower, I know the highest values are lower (indiscernible). In any rate, this is kind of a summary of what the pristane levels tell us regarding the food web in Prince William Sound so far, and this pathway has to do with the direct predation. We start up here around eight million parts per billion in the *Calanus* copepods themselves. We lose about, in the order of magnitude (indiscernible) in the copepods once removed.

Now, you'll recall I started this talk talking about pristane in mussels, and it has puzzled me for a long time how does pristane relate to mussels because -- and the reason it's puzzled us is because *Calanus* copepods is a pretty big animal. They're much bigger than the mussel can eat, and that required a fair amount head scratching and then a light bulb came on one day, and after that we did some experiments to confirm the light bulb, and this is how it (indiscernible), pristane gets into the filter feeding community. Fish or other predators on copepods eat this animal, it has a lot of pristane in it, and we experimentally determined in the experiments in the summer at Auke Bay lab that, yeah, it was about 50 percent of it. We had that after all, a pretty short intestine. The other 50 percent was excreted in the feces. Fecal material is something that a filter feeder can pick up, which we also demonstrated. They picked up 10 percent of the pristane available in 12 hours. That is how the pristane gets into the filter feeder community. I was pretty excited by this for a lot of reasons. One of them was, on looking at the mussel bed in the Prince William Sound collectively in the oil spill, I noticed that, not only did pristane go up in some mussels collected at some places in the Sound during May, but it actually -- it was a phenomenon that affected the entire Sound. It's really a huge phenomenon. This is what the temporal profile looks like at Point Ellen (ph) in 1994. We set up a series of about 30 stations this year, and tried to collect mussels at all of them once a month, and got them from most of them beginning in March or so, and ending in July. And, there was a big spike that occurred in May. What I'm going to show in the next series of slides is a huge wave of pristane that flows through the Sound. It begins in April, ends about July, and affects pretty much everywhere. In the next slides you'll see that they're color-coded, that white means that there was less than 300 milligrams per gram of pristane in the mussels; blue is 300 to 1,000; yellow, a 1,000 to 3,000 -- kind of logarithmically states spaced columns, and they go up to this popegenta (ph) which is greater than 10,000. The maximum values we saw were up around 50,000. Before April you just can't find pristane in mussels in Prince William Sound. By the end of April, you can find pristane in lots of places in Prince William Sound, including pretty much every place. What's really neat is by May not only can you find it, but it is quite high, and high in interesting ways. All along the hatchery corridor, it's real high. Well, that makes sense to me because in order for these dots to get up into these higher colonies, you got to have two things, you got to have a lot of copepods and you have a lot of things eating them. Well, there is a lot of fish that come out of hatcheries and also natural production in this side of the Sound, and apparently there is a lot of copepods there too. And, this is one of the major sites of energy conversion from the copepod trophic level into fish, or into other direct predators that produce fecal material that these mussels can pick up on. The other two hot spots that was intriguing in the Sound geographically were, over here in Windy Bay, and consistently in Port Valdez. Port Valdez is hot every year that I've looked, and we have data from 1989 on. After May, things start to die out. Almost everywhere it's in lower concentrations, and by about July it shows pretty much low, it's certainly over by September. Not only do -- does this phenomenon occur in *Mytilus*, which I wanted to show you, it also occurs in the other filter feeders that were collected in the database -- barnacles, butter clams, little neck clams, scallops and (indiscernible). Most data here is from little neck clams. We've got 186 observations. The median value is not very high, but again, you have to remember these clams were taken at different times of the year, sometimes they were much higher than were. (Aside comments) Primarily -- at last these are values that you would expect that are commensurate with the values you just saw for the (indiscernible).

The filter feeder predators have stomach contents that are what you would expect of something eating something that doesn't have much pristine in it. So not only does pristane show where you expect it to, it also does not show up where you expect it not to -- a very good sign. It's a little bit higher in the liver tissue, that makes sense as well because this is a storage tissue that would tend to concentrate whatever they do. And, then finally it showed up in lots of other things that were kind of a miscellaneous species, such as the crabs and the patapancreas. Sometimes they would have very high values. I think that depends on whether or not they ate a herring carcass that day. It also shows up in a bunch of other (indiscernible) animals. In their stomachs here I see traces of pristane are found in these animals, (indiscernible) whales, killer whales, etc. That's kind of a misleading term -- phrases, for example, in the killer whales, there were only three observations, two were below detection limits, one was 35,000 units lower. The two were below detection limits -- were below because the detection limits were around 20,000 because of the low sample size. So, killer whales actually could have quite easily detectable pristane concentrations in their blubber, and so could some of these other animals, although brown bears really did have traces in their livers. An interesting animal was the sea otter, which had quite low concentrations in its -- even in it's liver tissues, and especially in the stomach contents and livers. And, I think that again is consist with feeding habits of the animal. So, here is a kind of a summary food web extended now to the filter feeders and the filter feeder predators related to typical characteristic pristane concentrations that we might find in those animals. The grey line here means that way that pristane is transported is through absorption, primarily fecal material derived from these predators.

So, in conclusion, I'd just like to point out that, the pristane measurements is in the food web in the Exxon Valdez database are consistent with thermodynamics and known copo-relationships. (Indiscernible). Basically all that means is that if you would set *Calanus* as the single source of pristane in the Prince William Sound, by and large, the only concentrations you see everywhere else are low, that's just consistent with (indiscernible). The way they're lower is also consistent with the known trophic relationships that we know existed in Prince William Sound, and so I believe that this critical keystone species in the food web of the Sound has been colored with a magic marker, and so has colored a bunch of other species with magic markers throughout the entire food web. And, I think we'd be crazy not to explore this. Ways that it could be used in the future, I believe, are, first, resolving predation issues, for example, pollock. It would be a simple matter to catch the pollock, analyze the muscle tissue, and see how the pristane results compare with the yard sticks that we already have given to us by the Exxon Valdez database. We could see how these numbers compare with herring, that we know eat a lot of copepods, for example. Another, I think, very exciting possible use is as a way of taking the pulse of the Sound from one year to the next. It doesn't cost a lot of money to produce those slides I've just shown you, how the pristane pulses in mussels throughout the Sound. It costs a lot more to just get the mussels than it does to analyze them chemically for the results. But, what that pulse tells you from one year to the next is, how much energy went from copepods into their predators that year, and it would make a relative measure of energy conversion from year to the next. I remember last April, I believe a woman, Chris Blackie (ph) was her name, I'm not sure of that, she was from the fishing community, mentioned that we should be looking for ways to find appropriate, surrogate measures of the environment that are practical to measure and that would be useful for monitoring over the long-term. I propose this as well. I think it meets all the criteria. In addition, I think it can provide a way for locating, inexpensively, once again, critical marine habitat, by comparing geographic variations of pristane concentrations in mussels, and I've also looked at data that I haven't presented here, but it's data collected for the Kenai Peninsula, and the same phenomenon occurs out there as well. I don't know how far it goes out in the Alaska Peninsula or down towards Southeast Alaska. It would be sure fun to figure out. Whether we should or not is something that should undergo pretty careful scrutiny.

DR SPIES: Thank you, Jeff, for that very fascinating talk. I think it's a good example of some of the things that have come out of this process. (Aside comments - break)

Our program dealing with the status of the resources, and the first talk entitled "Oil Distribution and Long-Term Toxicity," is by Stan "Jeep" Rice from the Auke Bay Laboratory, National Marine Fisheries Service.

DR. STANLEY RICE: Basically, this starts off with an oil event, so I'll backup and start there. I won't spend very much time there, but we've heard two non-oiled talks, we're going to hear some non-oiled talks in the future, but this deal now will have a fair amount to do with oil, and some nagging questions that hang on with that. So, let's go. Basically, obviously in '89 there was a lot of oil damage to the habitat. There was some damage to quite a few different species and whether or not they're impacted, and that brings on these questions now. It's five years past that, at least 1994 is five years past that, and the question -- how much oil is still there and is there any long-term damage to that? Is the oil toxicity still a problem? That second question, is the oil toxicity still a problem, breaks down into two categories, actually. One is, is the oil that is still there causing new damage five years down the pipe, and there is some evidence that could be the case, or, if we have long-term damage, is it because of oil exposure back in '89 or '90. So, nevertheless those two questions break down to is there is there still a toxicity problem out there, and it's still kind of nagging us in a couple of areas. Well, let's look at the first question, how much oil is still out there? There are three basic studies that have been focused on that in the last couple of years, basically subtidal surveys by the Auke Bay lab, shoreline surveys by the Alaska Department of Environmental Conservation for the most part, and then there is also the oiled mussel bed surveys and joint programs going on in ADEC. Well, in 1994, the subtidal surveys kind of winded down considerably and that's because the surveys up through '93 had not found a lot of oil left. Basically, the water's quantity of oil get go to the subtidal, at least here to the beach zone, I should say. However, that's a tremendous geographic area with a lot of topography to the bottom, etc., a lot of depth, a lot of dispersion mechanisms operating there. So, the area is large, but even though the most amount of oil went there compared to the beach, the concentrations are very low, and it's hard to believe that those concentrations have tremendous impacts on various species and what-not. So, for that reason in '94 this project didn't really get up -- go anywhere. It's in the winding up stage, supporting those conclusions. I think in outlying years there is some rationale for some limited surveys to some part -- part -- different places that we saw, Sleepy Bay, Snug Harbor, Northwest Bay, or something of that sort, every third year, fourth year or fifth year, something of that sort to get some continuity, but it's an ongoing activity. This is going to be high profile study any more. The second survey type was by ADEC, looking at shorelines, looking for this sort of thing, digging holes to see if oil comes out and, of course, it does in some places. Starting back with their study done in 1989, which was multi-agency type of response-type thing. They looked at 2,591 miles of oiled -- excuse me, it's kilometers, metric here -- shoreline between Prince William Sound, Gulf of Alaska, and the Alaska Peninsula. They determined there were 225 kilometers of relatively heavily oiled. So, those in succeeding years received more attention than the other. Immediately after '89 or actually starting in '89, of course, there was some heavy duty clean-up of those oiled shorelines. There was about 10 percent of the effort was put off to 1990, so there is oil clean up both in '89 and '90, but by far the dominant amount oil clean up is in '89. That had a tremendous impact on the oiling of the shore, it got rid of a lot of oil -- I mean, good progress in that area than ever before that. So, what did they find in succeeding years based primarily on that physical oiling that Exxon did, and also the natural processes here. It's very natural that the oil actually decreased with time. These are easily documented. In 1993, the extensive of surveys have been constricted down now for economic reasons considering visiting 45 previous oiled sites versus a deterrent from earlier surveys. So, it's (indiscernible) oil. The surface oil though has been stabilized into a tarmac type of situation, relatively inert. However, the subtidal oil that they find relatively liquid. That's some weathering in it, certainly, but a much more molding surface than tarmac. So, there is still oil out there according to those shoreline surveys. In '94, this part of the project was combined, in part logistically and physically, with some of overlap with the mussel bed study we'll talk about next. They looked at, they treated 14 sites -- of the surface oiled sites that is, broke them up, tried to disintegrate them, disperse them, etc. We worked on them a little bit more. These sites will have to be examined in '95 to see if the (indiscernible - coughing) net impact, I mean positive impact, (indiscernible - coughing). In '94 ADEC also looked at about six sites in the Chenega area where there is significant subsistence use, and hopefully those will be treated in the succeeding year. Looking at '95, there are activities that need to be, we want to examine the treated sites that we did last year. Two, to try and treat the six sites in the Chenega area to look at those subsistence beaches and try to upgrade, the methods need to be worked on, determined, etc., and lastly through this project there has been quite a few complaints from the local residents on Kodiak Island that there is still a significant amount of oil in the Kodiak Island area, and so this project will need to be (indiscernible) surveyed on Kodiak Island, and determine basically, whether those are candidates for further clean up in outlying years or not. That will probably be our goal.

In addition to the shoreline surveys, which have sort of a wide geographic sense and non-biological aspect, there is also the mussel bed project, restoration project, which started back in '91. It was noticed that oil in mussel beds is still quite significant, as is noted in one grid survey. The mussel beds in '89 and '90 were not targeted for clean up because the logic that flowed then was that these are beds of too valuable biologically for predators, they stabilize the substrate, we can't loose this biomass, it may cause more damage, and the mussel beds should clean up by themselves. By '91 this didn't look like it was working too well. We had surveys in '92 and '93 which confirmed natural processes were not going along as well as we had hoped, certainly at the pace that we would want for those mussel beds to clean up. This is the return, for example, in both '92 and '93, that the sediments of hydrocarbon levels, for example, in 52 of 68 sites exceeded a 1,000 micrograms per gram. That is significant, you can see that oil, you can smell it, and if you are foolish enough to put it in your mouth, you can taste it too. Not a problem. This is physical amounts of oil, it's not chemical, nor below human sense of detection at all, by these (indiscernible) observed, etc. '92 and '93 some natural reduction had occurred, but not at the pace that we would want. The pattern was inconsistent, some places had quite very little change, however, they do have some change. And, we did attempts and some minimal manipulations. What that means is we dug some trenches through these mussel beds. The idea that if we open up the mussel beds that the tidal action will flush the hydrocarbons from underneath those underlying sediments. That worked, in spots in (indiscernible) a trench in about a foot, but as far as cleaning the bed and dispersing the oil underneath, it's ineffective, and so something bigger and better had to be done.

That brought us to '94 project, which was basically to be more active in restoring some of those case sites that were not progressing through natural action at all, or not well enough anyway. And, so the goal here was to go and restore selected mussel beds. In summary then, and this is done in part with ADEC, of course, while we're also doing some shoreline activities, 12 mussel beds were actually restored at 5 sites. Almost 19 cubic meters of sediment were removed and dispersed so that the oil would have exposure to the environment and would start getting metabolized by bacteria and diluted and that sort of thing. And, that translated to about 38 tons were removed. Now, how did we do that? It was done physically, it was done manually, we contracted with the Village of Chenega, and 38 tons translates -- I calculate out to about 1,500-50 lb. buckets. So, this is not a restoration activity I recommend, but it was accomplished. It took several time cycles at different sites, a couple of different periods to accomplish that. I said it was 1,500 buckets that would be removed, we take these buckets and put them over there, disperse them and break them, etc. so the water can have access to them. It's also 1,050 lb. buckets of clean sediment comes back. So that's 3,050 lb. buckets, so we used a fair amount of effort into this, needless to say, and obviously, if you're going to do, then you can't tell it on scale, let's do this next ten miles of beach this way, pick and choose exactly where you're going to do it, where your high priority, high concentrations are, etc. Conceptually, this was what was done. Number one, the mussels were temporarily removed over 50 feet or 100 feet or so, and 8, 10, 12 centimeters of oil contaminated sediment in next layer down were removed, they were dispersed, they were raked. Tidal action would then flush through the next tide cycle. Then, this 1,500 buckets of clean sediment were brought back, and then mussels were returned on top. These are low energy beaches, so it was hoped that the mussels would attach, and, again, we had pretty good success there. The next question is, well so what? Did we do any good? Now this is the past, prior to the restoration activity at just one particular bed, show you an example of where the concentration fluctuated somewhere around say 20,000 micrograms per gram. Sometimes they were higher, sometimes they were lower, but not a lot of progress there, and remember in 1994 of April, that's five years after the spill, so nature has not done a lot to this specific site. So, then we come in April or so, right after we took that one sample, and we replaced that sediment and dispersed it, and so what did we get? Well, we took a sample immediately underneath that replaced area, and we get a value and it's not nearly as high as these other values. The replaced sediment, of course, is clean, so that is done 12 days later, and they had not absorbed new oil from the underlying sediments, so that looks pretty good. Then, we look at the dispersement now, and, of course, it's going to be high initially, and by 12 days it'll get started with significant hydrocarbons from this sediment. So, 12 days after did this activity things were pretty good. The real question now is when we go back in April or May of 1995, one year after we did this rather physical exercise, what is the progress? Did we do a good job? Did we spend our money wisely? Did we waste much labor? We'll answer those questions for you then. Right now, we're encouraged though, 12 -- actually that's the measurements of some sites 25 days after the (indiscernible). This appears to be -- to work on a limited scale. Obviously, you can't go and do it on a couple hundred kilometers.

In conclusion, just for these two or three studies, one is that there is some oil still out there. There are needs to do limited surveys, for example we'd look at -- ADEC will look at Kodiak regions strictly to find some bad spots or not out there. Limited treatment is still needed, particularly around subsistence -- high subsistence use areas, and maybe some other places that are -- oiling increasing, and the treatments need to be tracked because we need to find out if these efforts are really doing what we want them to do, what we think they should be doing, so we'll determine that. So that is where we're at, the status of how much oil is out there -- that's where we're at right now.

Moving on to the nagging questions then, is oil toxicity the problem. We've shown some oil on the beaches, so that begs the question then, is there damage to those mussel bed predators, for example? Another good question then is, are we still having new injury occurring from this old oil that is still there? Is there long-term damage to herring or is there long-term damage to king salmon? Those would not be new injuries, those would be old injuries coming from '89 or '90 exposures, but are they still out there, is that a problem -- those are nagging questions. We live with those now.

Mussel bed predators, there are several -- there is the nearshore ecosystem study, for example, which is on the books, and hopefully is going to get off the ground. There are happening -- species studies in the past couple of years, looking at otters and they've noted, for example, that juvenile otters are having some level of difficulty, at least a couple of years ago. But, there are no directed toxicity studies at those species, so there are species- oriented studies, but no oil toxicity interaction. That's kind in the background, and there is the reason why there are no directed toxicity studies. And, the reason is, it's pretty difficult to link to oil. These are not you're -- you're free to chose an oil -- study exactly what you want to study to answer this sort of questions. The reason is that the otters, birds, etc., they all metabolize hydrocarbons. So, if you were to get a direct hydrocarbon linkage, you'd have to measure their stomach contents within 4, 5, 6, 8, 10, 12 hours or so after they have eaten some sort of oiled contaminated food. So, you're sampling limit is pretty darn small, and you've got to be pretty lucky. All these species, of course, have a significant foraging range. Some of them are pretty darn large, there not sessile animals like a mussel or a barnacle at a particular site. So, if you go to an oil site, you're going to get an oil contaminated mussel. That's not the case, obviously, with these varied animal predators that have a big foraging habitat. Secondly, even if they were to forage in the really massively oiled mussel bed site, they are also foraging a whole bunch of other places, so they're getting (indiscernible) contaminated food. Lastly, just sampling animals, of course, and birds, there is only a couple hundred harlequin ducks that have been on the westward side of the Sound, so having a sampling program that's going to sample a couple hundred is obviously is out of the question, and it gets even worse when you get to otters and killer whales, so we're not going to do a lot of that, and so that also makes them a poor choice of animals to do surface studies. Nevertheless, it's a nagging question, the species studies -- ecosystem studies -- may get at some of this indirectly, but not a lot of hope there in a way of really answering these questions. We can still look at recovery of those species in the ecosystem instead. Can't get at this linkage problem very well.

Next, problem I'll look at is Prince William Sound herring. Is there long-term damage with them? There was certainly damage in '89, those impacts on the '89 year class didn't recruit very well at all, and of course, we say well there is ups and downs in recruitment in all this other ecosystem-type functions, and yes, that's true. Nonetheless, it's a question that kind of nags at you. Did oil cause some long-term damage or not. We really didn't focus on oil toxicity herring after '89 and '90, that kind of went away, so to speak. There is still some herring studies. In '93 we had the population crash, with standing biomass of herring, which was not due, of course, to fishing pressure at that time or harvest. The reasons are kind of unknown, and that then raised the question, well, was it oil related, was it disease related, it oil that caused the disease, all sorts of questions. The crash then of '93 then stimulated both species and ecosystem studies in '94 to start again, and they would be continuing in '95. It also stimulated the oil exposure toxicity project, what we're doing at Auke Bay lab. So I'll talk about that part; herring people will talk about the other studies, the ecosystem studies later in the day.

Okay, so what did we do? Well, we looked at long-term damage. This is not an ideal animal for a couple of reasons, but nevertheless we're giving it a shot. In 1994 -- there are really three parts to this project -- in 1994 we only did one of the three parts, and that was to expose adults and look for damage in resulting larvae, we looked at eggs too, but the end point was looking at larvae (indiscernible) result, but only the adults were exposed. So, did we transfer enough hydrocarbons to the ova, from the ova to egg, when it's spawned, etc? Did it do something? The second part of the project, which is a follow up on data on exposed eggs. We know we can cause damage there and it is in the literature, so we'll do it a quantitive fashion and see if we can repeat some of the observations that were made in field-caught samples, do it quantitatively. We'll also deal with the component which deal the reproductive viability, to different sites grab a bunch of spawn, take it back to the lab, unit the male-female parts there, then rear the eggs and larvae for viability. We'll look at what year class at individual sites, we'll have different individual sites. Now, the fish that are there in '95, of course, some, meaning those since '95, which are only a couple of year classes at best, will not have any exposure to the oil. The oldest year class will have been exposed to oil, maybe, when they were, you know, juveniles or sub-adults, or young adults in '89. So, that's why I want to look at this across age classes, not just go to a site, because we're going to differentiate age classes there.

So, where are we at right now? Well, this is the one-third of the project that was done in 1994. The adults were exposed, we'll measure -- we did measure uptake, we then spawned the animal, and then we measured survival of the larva, length of larva, egg viabilities, and a whole bunch of -- 15-20 different measurements there in that suite of things. We, of course, looked for abnormalities in the development of the larvae, and one of the primary influence was looking at cell division aberrations, and this is a way to look at long-term damage. We really would like to look at genetic reproductive tissues, but it's just not practical to this species, so we're looking at somatic tissues, fins for example, to see if there is any chromosome separation involved (indiscernible) that sort of thing.

Okay, what did we find. Well, we concluded from the study that oil did accumulate into the adult fishes, it did accumulate into the ova, not a problem with getting hydrocarbons there, not a problem getting the hydrocarbons into then the spawn, the eggs. Okay, the adults were stressed, we have some interesting disease-related results there which are not conclusive in terms of causing disease, but is certainly an intriguing and kind of -- kind of -- well very interesting, to be honest, but their not conclusive. We also determined that we could not really measure any really significant effect on the eggs or the larvae resulting from those exposed adults, and we thought we could, we certainly loaded up those adults, but we really didn't see anything ending at the end of yolk absorption with the larvae we were measuring. We did have, by chance, some eggs that were deposited in the tanks during oil exposure which we didn't use for any of these other experiments, but they are messed up so that we're confident that when we go to the eggs exposure this coming year, we'll get some messed up larvae, but we'll see that in a quantitative way.

Okay, moving on then to the next species, which has some indications of long-term damage, and that would pink salmon. Pink salmon did have impacts on the eggs in the fall egg digs in '89. Not surprising. A lot of oil around. These are intertidal spawners, so those eggs do have a shot at getting some oil exposure during high tides. We also -- both ADF&G and NOAA looked at fry growth in '89 and found significant effects that marine fry groups, for example, not in 1990, but nevertheless there is exposure there in '89. It was pretty intriguing. In outlying years of '90, '91, there is still a little bit of egg mortality from these oiled streams, and then we had the population crash in '93. All those then had a stimulated event, continued effort in the field study by ADF&G to look at elevated egg mortality, so that continued through '92, '93, '94, etc., and it also stimulated a large lab of laboratory oil exposure to pink salmon eggs. The sea -- and the purpose of that was to stipulate or corroborate those field results of ADF&G was having. I just summarized the '89, '90, '91 stuff that Brian Bues, Steve and Sam Sharp produced in those years. And, looking at the lower scale first, it says intertidal zone, low, medium and high, I've taken the quantitative numbers that ADF&G had just that lower intertidal zone was not really low, just the lower part of the salmon spawning part. Salmon do -- pink salmon do spawn in the intertidal zone, sometimes up to 75 percent of the biomasses spawn there. They also spawn in a limited fashion in the fresh water zone. A lot of -- majority of Prince William Sound streams are geologically relatively young. There's some earthquake damage in '64, so fresh water habitat is relatively limited, especially compared to Southeast or other habitats. In that case, so what did we find, well, or what did they find rather in '89? In '89 you find oil and unoiled there. There is definitely an impact of oiled streams. They had elevated egg mortalities, and this is understandable, this is during the height of the spill, or, well, immediately after anyway, the summer. By 1990, notice that both the fresh water and the lower intertidal zones parts are overlapping quite significantly with the controls, and so the significant differences have really diminished, and only in the upper most part of the intertidal zone, so called bathtub ring, do we still find some elevated egg mortalities, and that is nice and logical. Well, then we get to 1991, and we have big time separations, and this is what caused the stimulation of one continuous effort, plus the last -- this is the big wow as some people would say. This is the surprise. This is the evidence of long-term damage. Why would you have this separation of oiled versus unoiled streams in 1991? Those animals were spawned in 1989 -- remember we have an odd and even year, two-year lifecycle -- so the guys that spawned in 1991 were the ones that reared in oiled gravels in 1989. So, that raises the hypothesis then that those guys then had an oil exposure, caused some sort of long-term heritable damage, and they passed that on, and the eggs then in 1991 -- the '91 brood then, were having trouble. The other very significant portion of this evidence is that when you look at the fresh water portion, you look at the fresh water portion in these other years, '89 and '90, they're not significantly different from the controls, but there is big time separation in the fresh water zone in '91. That can -- there is obviously no oil up there. You can only explain that as the intertidal animals that were exposed in '89 are now pushing up into the fresh water zone and passing on that heritable damage there. That's one explanation, and that's the working hypothesis that supports the continuation of this study. There is one glitch to that, and that is that the oiled streams are not quite the same environment that the unoiled streams were. We're not talking about eastern side of the Sound or the western side of the Sound, we're talking about north facing oiled streams versus south facing unoiled streams, even though they are in fairly close proximity. (Aside comments) That lead to what's called as the AFK experiments, ADF&G then. And that -- the function of this was to rule out that environmental difference as a cause for that heritable damage. So, what they did is on Day One and sometime in August or September, they went out to four different streams, two reference and two oiled streams, and took some samples, brought them back to the AFK hatchery, flew back, and spawned them, put them in the hatchery. A couple of days later they went out to another set of streams, third day they went out to six streams, and the last day they went out to four more streams, and basically the oiled streams separate out, this is significantly different. The environment here now is identical. It's done in a paired fashion. The rearing environment is identical. The only difference between oiled streams and unoiled streams is the origins. So, we ruled out, or ADF&G has ruled out environment, and they've shown that there is still -- this is '93, still a significant difference between oiled streams and unoiled streams, even when environment has been ruled out, so that again puts a lot of credence then to that heritable damage hypothesis.

Just to follow this up just a little bit more, they also measured elevated egg mortalities down here in the lower graph from those same streams, and you get basically the same pattern, the same separation as the spawn taken back to the hatchery. So, that corroborates the hatchery experiment. This project was done in '94, but they don't have any results of that, but in '94 the oiled streams -- remember, that is the even year cycle -- didn't show any elevated egg mortalities in the oiled streams. No difference as compared to '92, '93, etc.

What should be done in the future? There needs to be some monitoring of those odd year broods for sure. That study then, both the field part, the '89 through '91 and continues to get stimulation -- stimulated laboratory oil exposure to see if we could, one, get the same sort of results to corroborate those field observations. Big question, can we produce long-term heritable damage by exposing to oil, and you have to remember now those field results that we have just seen, both the wild part, so to speak, oiled versus unoiled and the AFK hatchery partner, those are unprecedented results. You'll not find anything like that in the literature, so that stimulates then laboratory corroboration.

So, what we've done here, and just using the '93 brood years as an example, we've taken gametes, we fertilize them and then we incubate them for approximately nine months in oiled gravel. They have voluntary come out, we've measured a bunch of things, what they look like in emergents, some uptake and, you know, variety of size the emergents, timing of emergents, etc. In April of '94 those animals came out, we counted them as they came out, we tagged them, and then we grow them up to maturity, and then in the fall of '95 we'll spawn them and look at the viability of their progeny, and this is obviously a long-term project, we have some '92 brood that we started, and we have lots of sort term direct effects, but we failed to rear them to maturity, so this -- but that was a small group. This is the big group, this is the main group of experiments. This is the large number with many, many replicas, etc. This is the one with statistical power that will be significantly different or not, depending on how the results come out. So, what have we learned to date? To date for results we have survival to emergents in various doses, and we have a dosed related relationship, more oil, less survival. This is not surprising. It's important that we got this, we couldn't really go on with the project if you don't get this, but this is not a big deal. The big deal will actually be, of course, when we spawn these survivors, and do we have a dose relationship to survival then. But, at the intermediate checkpoint we have appropriate results. And, that's not surprising, anyway, the salmon egg incubation is approximately nine months, you have a hatch there halfway or a third of the way into it, but you have this large yolk -- larva yolk being lipidophilic, of course, in taking in those hydrocarbons, then redistributing the hydrocarbons in the tissue of the embryo as it develops slowing. This is a chronic type of oil exposure, so maybe you do get these long-term damages. Some other things that we've done, and this is a bigger deal, is that we've measured the marine growth rate. We've done this for two different broods now, and we get a dose-related effect. I've selected some data here, for example, that just looks at the August-October time period, and the significance of this is that those animals came out in April, so they've had April, May, June, July, August, so they've had five months of rearing in clean water. They've been tagged so they've actually been dumped into one common pen, so they're getting fed and whatnot, and we have growth rates, there is a dose-related growth rate from April to July and July to August, etc., this is the last growth measurement we have. Five months after they've been in clean water, pooled in the same containers, etc., we still have a dose-related relationship on growth. So, we have some long-term damage here that is occurring. That then gives evidence, support to the concept that maybe when we spawn these guys a year from now, that we'll have some more long-term damage measurements to make. There is a couple of minor secondary parts to this project. I've indicated one from the '92 brood year, we used some gravel, and it's an upper dose, it's equivalent to about the two there, in that neighborhood, and even though that's been used for a year, and then used the second year during the '93 brood, it still has an effective toxicity to it. So, even though that gravel is weathered for a year prior to use of the '93 brood, it's still causing a significant decrease in survival -- I didn't show that data -- a bunch of other parameters, plus a delayed effect on marine growth. That's a pretty interesting fact. Also, another secondary experiment and that we suspended eggs above the gravel so there is no direct contact with that oiled gravel in a little stainless steel cup-type thing, perforate it, the effluent of the gravel flows through that and we get dose-related effects like that, so they don't need to be in direct contact with that oil. They will still get it.

What's next? Conclusions. Long-term exposure causes damage. Again, that survival is decreased, the growth is decreased, there's pathological damage increases, all that's short-term, neat, not a big deal. We have direct oil contact is not needed, that's significant. We have the marine growth is decreased, that's a pretty significant finding. The big question though is F-2 viability, which would be in September of '95, and obviously we haven't got there yet, so we don't know that answer, but that is the big deal, and whether we corroborate those ADF&G results or not. And, so what should be going on outlying years? Well, the ADF&G field parts, especially for '95 and '97, in other words that odd-year brood needs to go on. Need at least two cycles of where there is no elevated egg mortalities. That, in a way, will be corroborative that oil did cause that damage as we get diluted with time, we should return back toward the control, back towards zero. The lab study will continue to its completion here. And, there's also 50 a second lab-related experiment, and I won't go into a lot of details, it will parallel the other one, except its focus will be on marine survival and straying, and there are a lot of straying issues and questions, and I haven't delved into that at all. There are a lot of straying questions that how significant was it in '89? How important is this stock to this particular small stream? How -- what's the integrity of that stock, is it a bay, is it -- how far can they move, 50 miles or whatever before you start getting damage? There is lot of straying-related questions. There is pros and cons that the oil caused straying? Did the hatcheries cause straying, etc.? Anyway, this study will delve into those significant questions. And, with that, I'm done.

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DR. SPIES: (Aside comments omitted) Dr. Ray

Highsmith of the University of Alaska Fairbanks, talking about the intertidal nearshore system, and then Dr. Leslie Holland-Bartels from the National Biological Survey will also be contributing something to this presentation.

DR. RAY HIGHSMITH: As Bob as indicated, there have been some changes since when I was responsible for talking about, and I'll try and just quickly recap it here. Sea otter recovery monitoring has been moved to marine mammals. Chief coverage of subtidal sediment information. The PI for the black oystercatcher interaction has been out of state the entire time I knew I was suppose to talk about it his work, so I don't have any materials for that, but he did tell me that for a fix that -- where the adults fed in oiled locations, they had slower growth rates, and replacement eggs were smaller. Also, the adults tended not to distinguish between oiled and unoiled prey when they were feeding, but they did spend less time foraging in oiled sites. So, that's all that I know about that study at the present time. I don't know if Brad has prepared a statement or not, but if he has perhaps he can fill in some of this tomorrow. So, I'm going to talk mostly about this Herring Bay experimental and monitoring study. This is a study that has been done by Mike Stickel (ph) who is here from the Juneau Center for Fisheries and Ocean Sciences, and myself. And, finally, we've been asked to give about ten minutes for discussion of the nearshore package. I thought it was going to be done by Jim Bodkin, but it's really going to be given by Leslie Holland-Bartels. (Aside comments omitted) The study that I'm going to talk about then is focused on Herring Bay. Most of you have probably seen this sort of thing and know Prince William Sound quite well. Here's Knight Island, and Herring Bay is on the north end of it. Here's a drawing of Herring Bay with the oiled shoreline outlined by the heavy bar, and the unoiled shoreline being shown with the thin margin, and you can see oiling along here, and the lack of oiling back in here. Because we were using natural oiled and control sites, most of our control sites that are back in this area, the thought is that ice was in here when the oil actually entered the bay and protected this part of the bay from oiling on the shoreline. It's a lovely place to work and a fairly sizeable bay. Here's a sketch of the intertidal zone which I'll be talking about, and primarily focusing on the area in the high tide zone and the mid tide zone shown here. The dominant organisms in this part of the intertidal tend to be old snails like periwinkles and lipids, barnacles and mussels, and one dominant algae, the rocky fucus, and it has its own -- greatest abundance in normal locations up in here, but can occupy a large areas of space further on down into the intertidal. Our measurements of the intertidal begin at mean, high, high water down in here, and we talk about first meter vertical drop, second meter vertical drop and so on. These are true meters of vertical drop, straight down. So, you can have a horizontal run associated with each meter of vertical drop, so the actual beach length might be 20, 30, 40 meters on a shallow beach, or shallow sloping beach. A steep one, you might have 2 or 3 meters of horizontal run for each meter of vertical drop. I'm going to start with the seaweed fucus and spend a fair bit of time on that because we've also done some restoration work with it, and then conclude with some of the intertidal vertebrates. Here's a schematic of the lifecycle of fucus, adults here. They produce -- or release fertilized eggs that fall in sticky mucus strands, that do not travel very far from its own plant, and if they're successful in settling and getting attached and recruiting, then we get growth back into the adult plants. The cycle takes about three years to go completely around the loop. Here's some data on large plants, only tenth centimeters in length. If we looked at 1990, '91, '92 and '93, the green bars are the control sites and the purple bars are the oiled sites, and you can see that fucus was just about gone in the intertidal zone, particularly in the upper parts, following the oil spill and clean up. About in 1992 we see a good recovery especially in the lower zones of these large plants as recruits have grown and moved in with this larger size category. Even here in the upper part of the intertidal zone, we see this coming together of the densities. We do have some data suggesting that these plants at the oiled sites are still not as fertile as those and not producing as many eggs as those at the control sites. Here is a plate and it's put out to -- and it has fine groves in it, which entrap or capture or collect eggs, and so place here in adult plant like this, and we check this daily, we expect to have a fairly steady supply of fucus eggs. And, of course, we place these out at various locations at both oiled and control sites, and here are the green bars for the control sites, and you can see these asterisks indicate significant differences at .05 level. In fact, the more eggs there tend to be available at the control sites, particularly in the upper part of the intertidal zone. By '93 the significance -- it was no longer being found in the lower parts of the intertidal zone. It turns out that July of '93 was a bad egg month all across the board. It was apparently warmer that year, which may have had an effect. If we look at distance to nearest fertile plant, and this is important because the short dispersal distances of these eggs from the adult plants, we can see that there is some convergence now by '90, but still at '91 there was still big distances to the nearest fertile plant from germlings and so on. As this converged, then we have -- we would expect to find a greater supply of eggs and also perhaps an increase in canopy that shelters the germlings from desiccation. Desiccation seems to be a severe problem, now particular in the upper parts of the intertidal, for survival of germlings. In this figure, we have an adult plant here, and we have distance scales here to make a grid. In other words, distances away from the adult plant, and here are the number of eggs, and you can see from this that hardly any of the eggs dispersed more than a meter away from the adult plants. So, the sticky mucus and so on -- the way that the eggs are released, apparently is a mechanism to retain young nearby the adult plant. It also makes it very difficult then to colonize locations that are far away from the adult plants. Desiccation seems to be a real problem for the germlings. Here we have percent cover of germlings, and down here we have desiccation rate, which is the evaporation of sea water in grams per hour. And, there appears to be sort of a threshold at about two-tenths of a gram per hour, after which you pass that you get just about 100 percent mortality. So, it appears that the small plants simply do not have enough mass and so on to retain water under drying conditions. Here is some growth rates, and in general, the growth rates are fairly similar between oiled and control sites, with the exception of these two categories of smaller plants in the upper intertidal where we do find significant differences and they grow faster at the oiled sites. A possible explanation to that would be that there is better water flow and, hence, nutrient supply at the oiled sites, or that there is more elbow room for growth and for capturing sunlight and nutrients, less competition. (Aside comments omitted) The adults were heavily impacted, particularly in the upper intertidal by the spill and especially the clean up. I didn't show data on that; we have data on that from the coastal habitat study. This is Prince William Sound from sheltered rocky habitat to first meter of vertical rock. The large biomass in blue are control sites, red are oiled sites, and you can see the biomasses are reduced at the oiled sites. The dots represent cover, and still even up to '94 where we've tracked some of the sites, you can see that the cover is still not quite equal between the two. Settlement rates tend to be lower in oiled sites, partly this is been an issue of distance from the adult plants that produce eggs, and also perhaps a moisture issue, lack of canopy and so on. Recruitment has enhanced under the canopy. I did some data on that, but we know that they dry up quite readily, and so they need that or groves or barnacle tests, something to provide moisture or retain moisture in the habitat. They do not do well at all on bare rock or smooth rock. In fact, it's almost total mortality there. As for growth rates, at least in the upper portion of the intertidal for smaller pact categories, we did find faster growth in oiled sites. Again, it may be water flow. We're looking at that in animals as well.

I'm going to talk a little bit about restoration. This is a place in 1990, a place called Wessel Beach, was bare, that's been cleaned, and there is essentially nothing left. The upper distribution of fucus is right here. You can see a bit of it right along the edge of the water. This is about the third meter vertical drop, and the -- normally the highest bunch of fucus plants would be up -- along in through here, and you see there is total absence. (Aside comments omitted) This is a computer put-together of slides and data, so here was the upper distribution of fucus in 1990. In '91 it moved up about a half a meter, '92 another half meter, '93 just about another half meter, and '94 it moved back down to the '92 level, but '93 was a warmer year and it may be that there was a desiccation problem. As these plants tried to move higher and higher in the intertidal, and that one would expect that to be a major consideration, something we'll have to deal with. We put out an erosion control mat, sometimes used for seeding locations, and we put some of these in the intertidal and some were seeded; some of these mats were seeded and some were just out for natural recruitment to occur. Plants do settle in the mats. It turned out that it didn't make much difference whether they were seeded or not. The end result was about the same, with the majority of the plants being in the lower end of the mats, and one would guess that the lower end of the mats retained moisture -- where moisture accumulated. We see good settlement of plant attachment, and it's our hope, of course, that these will grow into adult plants and release eggs and so on, that will recolonize that location, that the fabric will erode and disappear. So, there will be restoration in the upper tidal, and, of course, this is one way of testing the desiccation hypothesis.

I'll move now into the -- some of the intertidal animals. I don't have time to talk about animals in detail -- to present detailed data. Here is a time sequence for the high intertidal periwinkles (indiscernible). This are kind of means for course texture science. And, the time scale here is an arbitrary one. Measurements were made more intensively following the spill, so 1 through 7 is 1990, and up to 14 is '91, and then just two visits a year in '92 and '93 and '94. So these are in couplets. In each couplet, the left-hand bar, the open-bar is the control site and the right-hand bar is the oiled site. The pattern then revealed is that the significant differences as indicated by asterisks tend to become less frequent with time, although we still do find them on occasion. The upper two meters of vertical drop, one would say that the recovery at least is well under way, and if we look at the same limpets but in sheltered rocky habitats, we do not draw the same conclusion. Here, we keep finding significant differences and much higher densities at control sites than oiled sites in the upper two meters of the intertidal -- or the upper two meters of vertical drop, where they're, of course, much more abundant. If we look at total limpets, at course textured locations, we still continue to see some significant differences out here, even in 1994. Most sampling we did in '94, for example, was the first meter vertical drop, and so on, but down here at the third meter vertical drop, there are actually more limpets in the oiled sites than there were in control sites. So, this part of the intertidal then appears to be doing quite well at all locations from a limpet standpoint. However, if you flip to a sheltered rocky kind of patch, you get just the opposite impression. The frequency of significant difference is declining with time in the upper parts of the intertidal zone, but in fact is not showing that at the third meter vertical drop. So, one of the things with which we've discovered throughout this study and the coastal habitat study is that the results are not consistent across regions, across tidal heights, across species. There is an awful lot of variability. We've also looked at mussels in Herring Bay, trying to use this as an organism in which we could explore the role of water motion, having the initial hypothesis that places that were oiled probably were places that also normally tend to have good water flow, better than that at control sites, and that's how the oil was distributed in the manner it was. And, so that water motion would do things like bring in added nutrients, food for filter feeders and larvae for those organisms that are distributed by planktonic watering. Here we have mussel growth rates -- they end here -- we've not finished analyzing this. This is '94 data -- haven't finished any statistics and so on, but here we see growth rates at about 400 tagged mussels, and the left bar is the oiled side and the right side is the control side, marked by the sea here. You can see that growth rates are not all that different between oiled and control locations. There is some jumping around, but it's reversed over here at this site. These are size frequency distributions. Here is an oiled site, and here is a matching control for it, and if we looked down here it has accumulative size frequency distribution, and we can see that the oiled site has a lot more smaller individuals in it than the control site. These animals then would be in the neighborhood of about three years old, so here it's May of '93, so these would be roughly 1990 recruits, suggesting then that recruitment and survival was pretty good at the oiled sites, better than at the control sites. If you look at the end of the summer of '93, we can see from this accumulative graph that mortality among the smaller individuals is proportionately higher at the oiled site than at the control site, so the two curves came closer together. In the spring of '94 they are closer together yet with recruitment and growth feeding into this, and so, it appears that the two locations are starting to behave more similarly, and this is also an encouraging sign from a recovery standpoint. Keep in mind that these are frequencies and not absolute densities or numbers. The recruitment and the number of individuals is higher at the oiled sites than at the control sites. Here are densities by animal size, by mussel size at the oiled site, 1522, and the lighter the bar the larger the organism. So, in the spring one finds the population tends to be dominated by fairly low density, large animals. I think most bodies numbered by 20 if you wanted the number per square meter. By the end of the season, we can see that the population is dominated by these smaller individuals that have grown into measurable size categories here. While the previous graft ended at 450 individuals, so the scale shifted here, and so here we're at about 40,000 mussels per square meter. This is also still at the oiled site. If we look at the maps of control site, again, in the spring the population was dominated by low densities of larger individuals. By the end of the season the recruits and the smaller animals running to larger size categories, but still the densities are way less than those at the oiled sites. So, it appears that from the accumulative frequency curves and from the size data just presented that mortality rates among, especially smaller individuals, are higher at the oiled sites, but so are recruitment rates. So, everything -- the data you would use to build a life history table is moving at a higher rate at the oiled sites. Again, we think this might be due to the greater water flow at those locations. Mussels tend to recruit unto Phaeophyta algae and then secondarily move onto rock or into the mussel bed. Here it's just a -- some graphs -- graph from this algal cover. No particular difference between oiled and control sites, but the density or cover of Phaeophyta algae is fairly high, especially in the lower parts of the measurement area, which would give us data in the intertidal, here. If we look at the mussel beds density and the Phaeophyta algae, so these are recruits, these are very small mussels. These go in couplets across here, so here's oiled, and here's control, and so we can see the density of mussel recruits in the algae is much higher at the oiled site than at the control sites, but the pattern is not consistent all the way through. (Aside comments omitted) If we look later in the season from May of '94, unfortunately there were so many mussels in the one sample, we don't have it sorted yet. So, it's not that they aren't there, it's just that we don't have the data ready for this presentation yet. If we look at 1522 and 1522C, which is the end couplet of the series, we can see the density of mussels following recruitment during the season is higher at the oiled locations than at the control locations consistent here. Up here it's -- we require a zero here. So, again there is more evidence that recruitment, so they're operating on a higher rate at the oiled site. These are some plaster of paris dissolution blocks that we put out. In this case to two parallel locations. We have more data than this, and the consistent pattern in higher dissolution rates at the oiled sites indicate a greater water flow than at the control sites. These are aerobars. They aren't shown here because they are so tight that (indiscernible). This pattern is not 100 percent consistent, and other times we've done this -- on occasion it appears it reverses this pattern. These are only out for two or three days at a time so, what one needs is a little better spread over time to see how consistent this pattern really is.

The rocky fucus has tended to recruit hardly at all, so some improvement has been shown and a lot of this is thought to be due to the distance from adult plants and because of the short dispersal distance, and perhaps the canopy issue too. Recruits favor crevasses and barnacle tests over other substrate types, and bare rock is least favorite. I didn't show a slide on it because of time constraints, we didn't have that data. If they settle on barnacles, it appears that they don't make it to large plants, maybe because the barnacles die and some of it rips off by the waves. Desiccation is a major source of fucus recruitment mortality, and we did show data on that, that don't tolerate evaporation if it's much greater than about two-tenths per gram per hour. And, in the upper intertidal, the smaller plants tend to grow faster in oiled sites, maybe it's a nutrient issue, maybe it's an elbow room issue. The recovery of intertidal invertebrates varies with species and habitat types tends to be greater lower intertidal, but it's not consistent. For example, in the limpets in the rock shelter rocky, we found just the opposite pattern. The mussel data suggests are more dynamic, that things happen faster there and on a larger scale; there is higher recruitment rates and higher mortality rates. Some evidence that we have now indicates water motion is greater at the oiled sites and, therefore may in fact, be the factor in increased recruitment rates for those things that are distributed by planktonic larvae, or growth rate such as the fucus and the intertidal filter feeders. I don't know if we have time for questions, like to turn it over now to Leslie Holland-Bartels.

DR. LESLIE HOLLAND-BARTELS: i was asked to give a brief overview on Project 95025 which is a nearshore package that was developed based on the outcome of a workshop last April, and emphasize it's an unusual package, it's certainly not the most unusual package. It's been a challenge to try to put together in a disciplinary manner, and I prefer to say that we're a -- now a good red wine or blended, but I would say -- I feel fairly good that we're good pot of stew. That the parts are still recognizable, but we're blended, and it's getting to a point where we're very competent in that area. The project received planning money in '95, and so we're in the process of developing a detailed project proposal for submission to the Trustees later this spring. It's a cooperative project, ADF&G, Fish & Wildlife Service and National Biology Service, and has twelve plus cooperators and scientists. Just as a brief starting point, this is where we started last year's workshop. Basically laid out seven hypotheses on what was restraining nearshore recovery. Those can be divided up into trophic factors, recruitment, physical factors of oil. These were the hypotheses that were the basis upon initial cleaning effort. I think you've heard from Ray and from Jeep earlier on the issue of trying to assess what are the factors restraining recovery in nearshore ecosystem are quite complicated because we have a highly complex and dynamic system. So, we chose to focus on a suite of apex consumers and top predators and their key prey items, primarily because we selected a suite of predators that were injured themselves, and we felt that they could represent an integration of potential constraining factors in the system. We selected four top predators for a variety of reasons, that I won't go into right now, but those are broken into benthic invertebrate feeders and nearshore the demersal fish feeders -- sea otters, sea ducks, river otters -- all damaged resources. And, we're taking an integrated approach looking at production and biochemical immune system markers for this suite of predators, as well as trophic interactions. Our approach is fairly simple to diagram, and basically nearshore vertebrate predators have not recovered -- why haven't they recovered? Simply is it food and/or is it all of it? It's easy to say, it's so hard to assess. We've taken the approach of not wanting to throw the baby out with the bath water. A great deal of work has been done in the species that needs to continue, particularly in the area of demographic population assessment and health parameters. So, those are two areas that we're working in. The difference is that we're working across the suite of predators in an integrated manner (indiscernible). The trophic area is an area that's new to this project, and I think Ray's presentation indicated some of the complexities of dealing with trophic issues in a nearshore environment. We do, however have a wealth of information that documents that predatory forces in a nearshore environment can and often does overshadow other forms of population structuring, the competition and space, in structuring invertebrate communities. We also know that the top predators in the nearshore were damaged, that there were significant mortalities, and so the top predator population structure was changes, and that we might suspect that this would be reflected invertebrate population changes as well. We can use the documented structuring of invertebrate communities to assess if predators are recovering, and with other tools why they may not be recovering. This figure is just general information. Sea otters, for one, have been documented as a top down structuring predator, particularly in relation to sea urchins. When sea otters are not present, urchin populations in the shallow areas are high. The size distribution of the urchin population is traverse, and it's leaning more towards the larger individuals. When otters are present urchin numbers are decreased in the foraging area of sea otters, and also diversity, the breadth of the size class of invertebrates is significantly reduced (indiscernible) gets smaller. So, there's documentation for sea otters and many other predators in the nearshore system that they do in fact structure invertebrate communities. So, specifically for the project, if we -- we can hypothesize that a number of pictures might be seen when we go out in the field between an area where otters are present and population is deemed healthy and in areas where otters have not recovered, this is an example of -- if biomass is similar and the invertebrate population is similar, then we would conclude that predation pressures are equal and recovery is occurring. However, if area and invertebrate densities are higher, the area where recovery hasn't occurred is lower, and yet the invertebrate structuring is similar, we would conclude that food is there and recovery of the top predators uncertain. And then finally, if we have a diverse size class of prey in the environment where otters are present, and the biomass is high, then we would conclude that predation pressures are not equivalent and recovery is not occurring. So, this is kind of the basis of our trophic approach and study. As Ray presented, there are lots of complications to this picture that we're incorporating into this study as well, and that has to do with how do you interpret the size class and the biomass picture that you get from invertebrate community, because that is in fact affected by inner, annual variability in recruitment patterns. It's affected by other invertebrate predators, sea stars for example, and other top predators in the suite that we're looking at, and in the case of benthic invertebrates, sea ducks are a good example that can in fact alter the invertebrate composition, before we get to sea otters and the final picture. This is the approach that we're going to be taking and trying to model what we see in the end, whether predators are present or not present. What we hope to end up with is not a linear decision tool in this case because of the complexities of nearshore environment. We hope to end up with a matrix based on a suite of top predators as different windows to the question and a suite of tools to examine the issues of recovery and integrate the multi-species approach. And, we've laid out a large number of objectives under each one of our basic strategies. One objective, prey variability and competition for prey is constraining recovery, and oil is constraining recovery, so that's the tack we're taking. And, although PI's will be available at the present in the sessions tomorrow, I encourage you to get further discussions on the details, and I do have draft documents if you have specific questions.

DR. DAVID IRONS: The sea bird projects that I'm going to review are going to be a little bit different review than some of the earlier ones today. I have seven projects to talk about, so I'm going to quickly go through -- the seven was the highlights of the 1994 results from the 1994 studies. Some projects I'm going to talk about are the pigeon guillemot project, marbled murrelet project, common murre project, and harlequin duck project. All four of these species were injured species -- were called injured species by the EVOS. Also, I'll be discussing a fox removal project that was done on sea bird islands, and a survey of Prince William Sound sea birds project. And then, in 1994 there was a forage fish project that went on, and part of that involved birds and I'll be talking about the bird portion of the 1994 forage fish project. In 1994, most of the focus of these projects were on single species, and we're going through a change here where in '95 many of these projects are going to become part of a single project, the sea bird portion project, that is more of an ecosystem-based project. So, we're going through a change here, and as Bob said, Dave Duffy will be talking about the sea bird forage fish project when I finish. Basically, the question of the sea bird/forage fish project is, is food limiting the recovery of injured species? First topic I'll review is the pigeon guillemot project. In 1994, Lindsey Hayes monitored the foraging behavior and reproduction of guillemots to examine the effects of food and predation on reproduction success. In '94 we studied guillemots in two areas, Naked Island and Jackpot Bay. Pigeon guillemots in the Sound, as well as several other species, they have declined since the mid-'70s, and the declines were likely -- occurred -- some of the decline occurred before the oil spill, and that's true with guillemots. But, here we have some early counts for the Naked Island complex, in '78 and '79, and post-spill was around 1200, and still in '94 the population has not increased past the 1200 marked. So basically there has been no increase in the Snake Island population post-spill. As summarized -- the conclusions from Lindsey's study, the population has been down since the '70s. Reproduction success has been lower on Naked Island since the spill than it was before the spill. Chick diets will differ -- different now post-spill than they were pre-spill. In the late '70s they ate more sand lance, now their eating more gadids, more pollock and cod. And, there are also some differences between Naked Island and Jackpot Island. More eggs are abandoned on Jackpot Island than on Naked Island; chicklets were different. More herring and/or smelt were eaten at Jackpot Island than Naked Island, and the birds flew further to forage Jackpot than at Naked, and the chicks grew at a faster rate at Jackpot Island than at Naked Island. So, in regards to our new slant here of the sea bird/forage project, please remember here that it seems like the diets have shifted from the late '70s to the early '90s in that they used to eat sand lance more -- go off sand lance more in the '70s than they're eating now.

Next, I'll summarize the marbled murrelet results. In 1994, Kathy Kuletz, Dennis Marks, and Nancy Nasland worked on marbled murrelets in the Sound. As with guillemots, marbled murrelets have declined significantly from the 1970s. The murrelets are showing no population recovery from '89 to '94. In 1994, the objective was to define foraging patterns and habitat of marbled murrelets in Prince William Sound and to try to come up with a productivity index. Marbled murrelets are very difficult to work with, and for some species it's easy to determine what the productivity is, marbled murrelets it's not, and we recognize we need to come up with a better productivity index, so we were working towards that in 1994. Look at foraging areas, they, this had (indiscernible), they radio-tagged 46 birds, marbled murrelets in Prince William Sound. Marbled murrelets have been radio-tagged, but never in this large of number. They radio-tagged marbled murrelets in two different habitats. One, Naked Island and the other is Port Nellie Juan. These are distinct in that Naked Island is more of a shallow water area and Port Nellie Juan is a deep fiord. This is an example of -- this is how some of those marbled murrelets traveled to go forage, some went clear up to Naked Island complex, others went out to Knight Island complex. Basically, we have distance traveled and if we look at -- from the nest, we may not have located as we wanted to, but the ones from the nest sites traveled an average of almost 20 kilometers from the nest, and then between the capture sites were similar, almost 20 kilometers at both Knight and Naked, and then between sites they traveled -- consecutive sites go down about 11 kilometers, 13 and maximum between 2 points is up around 20 or more. The greater one, where a murrelet traveled between two different sites was 94 kilometers. Now, what this information is, it's the first time it's been documented that marbled murrelets really travel this far with the radio-tag onto in, to go and forage. This is very important as far as determining where the effects might be found. There is no significant travel between, by bird, between Naked Island and Port Nellie Juan area. But, most demonstrated difference use of available habitat in two area. In Naked Island the birds selected shallower areas, more than expected, based on availability, but at Port Nellie Juan the birds used the areas in proportion to their availability. They were found in many deep water sites at Nellie Juan compared to Naked Island. As far as juvenile surveys to determine the productivity index, juveniles -- the patterns that they found were similar in the two areas between Naked Island and Port Nellie Juan. Juveniles were observed in low levels from July 22 to August 8, and then the increase remained high until September 1, then they begin to decline, so what's nice is there appears to be a period there where juvenile are somewhat constant and high so we might be able to have a window to monitor the number of juveniles in the area. There are also able to radio-tag one juvenile that left -- well, it was on the nest, and then it left and stayed in the area for about two weeks. So, it matches the other data that they found.

Okay, next will be the common murres. During the summer of

'94, Roseneau & Kettle did restoration and monitoring study on common murres at the Barren Islands, and basically, murres at the Barren Islands have not shown a population recovery since '89, they're still down low. However, nesting chronology and productivity indicated that the reproductive timing and success of the murres are within the normal bounds, through these two colonies. One real interesting observation that Dave made, especially in light of perhaps expanding the area that we're going to be working in, and long-term differences as far as the ecosystem goes, is in '94 they found huge amounts of capelin around the colonies in Barren Islands. And, as you saw earlier, this is a fish species that is very important to sea birds in Alaska, and has been scarce in this area since the late '70, but there are large schools of capelin were present throughout the islands -- near the islands, through mid-July to late August. And, also there was an awful lot there -- there were 150 to 200 humpback whales there. They were probably foraging on these fish also. Also, last year there may have been a lot of capelin around the Barren Islands because, again, hump back whales for foraging large numbers on the islands; however, the schools that were identified, or were seen were much deeper, in deeper waters, and it corresponds last year, '93, kittiwakes, which are surface feeders, failed miserably, and the murres did well. Murres can dive and they could have dove for this school last year to forage on them. This year the schools were at the surface and the kittiwakes did well too.

Okay, harlequin ducks, the next species I'm going to talk about. The harlequin duck people felt like they needed a better data on the breeding population in the Sound, and so in '94 Dan Rosenberg did an experimental harlequin duck, marine bird survey, and they were successfully in developing criteria techniques that classify males into three age categories, and it classified old ducks by sex during the molt. So, what they plan on doing would be, you know, how the (indiscernible) compare the seasonal changes in numbers and distribution by age and sex so that they can do surveys to, in fact, compare the population structure and trends between oiled and unoiled areas in Prince William Sound.

Next project is the fox removal project, which is the restoration project for sea birds. During the summer of '94 Ed Bailey and his crew conducted a fox eradication program on Simeonof and Chernabura Islands in the Shumugin group. The objective was to restore population of native birds, particularly two species, injured birds from the oil spill, the black oystercatcher and guillemots, that occur on these islands. Basically, they went out and killed every fox they saw, so they think they were successful, but they need to go back next year just to be sure there are no fox left in those islands. They also conducted surveys of oystercatchers and guillemots on these islands and other control islands to provide the basis for evaluating the response to birds after fox removal, and this has been done on several other islands, and generally there is a dramatic increase in bird population once you remove the foxes.

Next projects I'll discuss is the sea bird survey. In 1990, '91, '93 a summer and winter survey of Prince William Sound sea birds was done. In '94 -- winter of '94 -- only '94 winter survey was funded, so I'll be discussing today basically the winter surveys that were done in '94. The purpose of these surveys is to monitor recovery of injured species in the Sound, and this is basically the only Sound-wide survey of marine birds. This gives you an idea of where the transect are, about 250 randomly chosen transects, and this is the distribution of the birds. The shoreline ones are circled and the coastal pelagic are squares, a block was done of it. Basically, this how the -- where the transects were and where the birds are. This is total population of marine birds in the Sound in the winter time. The idea of this project was to look to see if birds were recovering. To do that, what we did, we wanted to compare the oiled areas to the unoiled area, and we said we'd have recovery when the birds in the oiled area were increasing at a faster rate than in the unoiled area. So, what we did was a homogeneity of slopes test for populations in the oiled area and in the non-oiled area to see if the slopes were different. Basically, after the '94 survey they had four data points, as I pointed out, and -- which isn't much, but that's all we have, and no species had shown a recovery at this point, in 1994. However, there were two species that showed a reverse trend, that is that the unoiled population increased faster than the oiled population, and these two species were goldeneyes and mergansers. Need a lot of data, so it's kind of hard with your eye to get the fill for it, but basically, the unoiled has grown up faster than the oiled is. So, that's showed significantly kind of growth in goldeneyes and mergansers. One problem with these results is that we use .05 to determine significance, and then we went and ran 45 tests on 45 species, and so by chance alone we expect two species to show up, and that's what we have are two species showing up. These differences may not be real; however, goldeneyes showed up the same difference last year in 1993, so the chances, the probability of goldeneyes showing up twice in two years are very, very small. So, I suggest this effect -- goldeneyes increasing faster in the unoiled zone is true, is real. Also, we have harlequin ducks which did not show any significant effect, but again you can see it's not consistent, but there is more space between the oiled and unoiled area in later years than there is in early years. And, bald eagles show a similar trend, but, again, this is not significant. In addition to looking at recovery, we looked at changes in total populations, and we found an increase in total population of harlequins, goldeneyes, mergansers, bald eagles, black-legged kittiwakes and gulls. So, this is -- the whole population in March in the Sound is increased. Basically, gulls and kittiwakes don't winter in the Sound, and most gulls don't winter in the Sound, so the effect here could be just a change in phenology, so I'm not going to discuss these two. So, that brings back to -- harlequin, goldeneyes and mergansers and bald eagles. But, it increased, but the increase have been larger in the unoiled area than in the oiled area. Why would they increase in the unoiled area? Well, there's two reasons why the birds might be increasing in the unoiled area. One is that, for some unknown reason, unrelated to the oil spill, is that the populations are just on an upper trend. The other reason is that the fact the populations in the unoiled area may have been affected by oil spill and we are seeing a recovery, even though we wouldn't define it that way. So, the good news is that some species are increasing in Prince William Sound; the bad news is they are not increasing in the oiled area as fast as they are in the unoiled area. As I said, no work was done in the summer of '94, but I'll quickly summarize the work from '89 to '93, for the summer work. Basically, no species showed recovery in the summer and, as opposed to winter where we have population increases, we had no population increases in any of the species in the summer time.

Next project I'll talk about is the forage fish project, the bird part of the forage fish project. In 1994, a forage fish study was initiated to assess the abundance and diet and composition of forage fish in Prince William Sound, and after Dave Duffy gets down, Al Tyler will talk about the forage fish project. But, I'm going to go over the bird part of the forage fish project now. Bill Ostrand was project leader for the bird part of the forage fish project. Basically, the objective -- this is the main components to look at, food availability, from the bird's point of view. 1994 was a pilot study and the main objective was to do tests of techniques and collect data to help us design a better study in 1995. Enough (indiscernible) are similar to the other survey used on the other marine bird surveys. This is an example of what -- we collected data at the same time that the fish data was being collected, we made bird observations. (Aside comments related to slides omitted.) Basically, this type data, what we want to do is characterize the differences of schools of fish that birds are and are not feeding on, and so, you know, the species, the distance and size of the school (indiscernible) surface air, characteristics that we looked at. In the August cruise -- cruise tracts -- in the August cruise, and the symbols here show where bird sightings were. Yellow were low sightings, blue was more a black was more and red was most. But, generally there is a trend here towards more birds being closer -- these are only birds that were on the water or foraging birds, and there is a trend towards birds being closer to shore. And this is what we found from kittiwakes, the idea that -- and also an initial pilot forage food study done in 1990 that there are more foraging birds close to shore than offshore in Prince William Sound. So, when we see a large scale phenomena, like Al Springer was talking about earlier, in the Sound we might be looking at a smaller scale phenomenon. It might be, you know, a scale of a few miles we're looking at. It might be something that -- nearshore where the birds are feeding versus offshore where they are not, and, of course, it's related to where the fish are.

Okay, so to summarize the sea bird projects, no injured species have demonstrated recovery to population levels. However, murre reproduction success is normal. There have been new effects that are potentially, there is all the oil spill, that we are seeing five years after the spill, for example, the effect of the benthic invertebrate feeding goldeneyes. There are several pieces of information in various bird and mammal studies that suggest that both benthic invertebrate feeding and forage fish feeding, birds and mammals, are not recovering. The reason for the lack of recovery may be different for the two groups, and hopefully during the next four days, these pieces of information will be brought up in these several studies and help us determine why recovery is not occurring and determine where to go next. Thank you.

Now I'd like to introduce Dr. David Duffy to discuss where we've been going with the seabird/forage fish project in 1994.

DR. DAVID DUFFY: It's always nice to give a talk before you hear any data -- you're not constrained by reality. I basically have a problem that the sea birds are not recovering after the oil spill. On the other hand, we know that diet data taken before the oil spill, considerably before and after the oil spill show a great deal of difference. And, if we're going to understand the lack of recovery, we really need to be able to have a basic understanding of the distribution, abundance and quality of the forage fish the birds prey on, and we need to know the effect of these attributes on the sea bird populations and reproduction. So, a group of ten investigators or groups of investigators got together, and I was brought in about a month ago to help work with them to put together a program that was already pretty far advanced. So, in some ways, I'm sort of the outsider that is brought in to ask the dumb questions, being new to Alaska, and in other ways most of this was pretty far along, and I have a relatively easy task, which is sort of nudging things close together. We really have two basic research questions -- one is, is food limiting the recovery of sea bird species, and the other is, what determines the distribution and abundance of forage fish. The first question about the limiting effect of food really can be broken down to three factors: is food truly scarce, or is it merely unavailable, or is it of low quality, which I think Scott Hatch calls the "junk food" hypothesis. Basically, for the first one, is food scarce, we're going to approach this through acoustic surveys of the Sound and compare that with sea bird diets and the effect on reproduction for several sea bird species. In other words, do acoustic surveys show fish be present and, if so, are the birds reproducing normally, and what are they taking of what's available in the Sound. The second, whether food is present, but simply not available, is a matter of teaming up the acoustic observations with observations of the birds from the same survey nests. For instance, birds might find that fish are there, but they are too deep. A kittiwake can't go down 10 meters. A bird has a limiting foraging range, perhaps a pigeon guillemot may not be able to go 40 kilometers to where food is abundant. Also, fish schools may simply be too small for some species that require denser and more predictable sources of prey. To tackle this, it's going to be a lot of direct observations of the birds as they are foraging, or as they're not foraging, coupled with the acoustic surveys. Finally, to look at the quality of the fish, most of this will be done at the colonies. Several observers or researchers will look at the effect of different diets on growth and survival of young. At the same time we'll be analyzing these fish in the labs, see fat content, protein content and other qualities. So, from these we will be able to knock out one or two of these hypotheses about the role of food limiting recovery. But, underneath this there is sort of a equally interesting and perhaps more important question of what determines the distribution and abundance of forage fish. And, if Prince William Sound is really going to be healthy, we need to know what normal variability is, we need to know some of the processes that are involved in determining the distribution and abundance. And, unfortunately, it turns out we lack a lot of the basic data -- basic natural history to have a stab at this. So, in our first couple of years we're going to be collecting fish, as well as looking at the distribution. We'll also be looking at their diet, their size characteristics, reproductive state, and daily intake for a series of the fish that the birds feed on. After several years of data, we hope that we can go and sort of back cast to several hypotheses about what's controlling different fish species. For instance, is there a predatory role of one fish that suppresses another, or is one fish out compete another? Is there cannibalism? Or, are there simply physical forces to which different species appear to respond in different ways. Taking these all together, we want to address some of the, sort of scenarios that Alan Springer presented earlier, about how ecosystems -- the Prince William Sound ecosystem or the ecosystems in the northern Pacific may be structured. Are there -- our data, do they fit with some of these scenarios? Is it possible that we can generate predictions about the effects of over-fishing, the effects of whales, the 18.6 year cycle? Can we just count some of these based on data that we accumulate from both fish and birds. I think we have about three years to get an idea of the initial answers to these and we only have six years or so to -- which statistically is a very short time, so we're eager to get going on this, and I think the combined effect of using both birds and direct monitoring of fish is going to be a very powerful tool to untangle some of what's happened in Prince William Sound. Thank you.

DR. SPIES: As I understand it, Dr. Al Tyler of the University of Alaska Fairbanks will have a short presentation.

DR. AL TYLER: This is the fish portion of the sea bird/forage fish project, and the funding for this work began last August. We immediately got into the water and carried out a preliminary cruise to investigate ways of using hydro-acoustics for this project, and then planned the more serious cruise in November, a second one, where we used the larger vessel that's capable of towing different kinds of nets and simultaneously sampling water properties as well as carrying out the hydro-acoustics. The main goal of the project is to make estimates of the abundance and distributions in relation to sea bird colonies. The question is whether or not there is a fairly even distribution of forage species throughout the Sound, or in fact a very patchy distribution, and then what does that need in terms of success of sea bird foraging. We also want to know about the inter-annual -- possibility of inter-annual in forage fish abundance. If the sea birds begin to, or continue to decline, are the forage species simultaneously declining, and so can that provide an answer -- possible answer for changes in sea bird abundance. Now, the second goal is to look at insight into forage fish biology and mechanisms of abundance change. The species that we are considering are juvenile pollock, capelin, juvenile herring and probably sand lance. There is a possibility that eulachon may be one of the species that we will investigate as well. The purpose of the research cruise is originally was just to take a much needed series of hydrographic measure simultaneously with the sea birds and the forage fish under them, and then to provide specimen for cooperative work with U.S. Fish & Wildlife Service, ADF&G, National Marine Fisheries Service and the EVOS SEA project. The other purpose of the cruise was to allow sea bird biologist to continue a series of sea bird census work simultaneously with the work that we're doing on forage fish. The hypotheses, first of all that a change in total forage fish biomass influences sea bird productivity, and second, and perhaps simultaneously, a shift in food quality influences sea bird productivity. There may be species composition changes along the forage species. So, one of our tasks was to look at the general abundance, that's using hydro-acoustics for fish and invertebrate forage. A second one is net sampling of those forage species to determine species composition and size spectrum, and, along with that net sampling to provide specimens for some of the other sub-projects. Now, it's possible that the shift in forage availability, as distinct from general abundance, maybe what is what is influencing sea bird productivity. So, the distribution into high density patches and the hydrographic effect factors that are influencing those distributions, then became a very important aspect of this work. And, similarly to the first hypothesis, we need net sampling to determine species composition for this -- to investigate this second hypothesis. The principal investigators -- one of the investigators on this project, A.J. Paul from Seward, University of Alaska Fairbanks, IMS; Lew Halverson, School of Fisheries and Ocean Sciences, Juneau Center; Ken Coyle, Institute of Marine Sciences, Fairbanks, Richard Thorn, Biosonics in Seattle; and myself as coordinator of the project. Just to give you an idea of where the first cruise looked for forage fish and tested gear, there are several zigzag patterns throughout the western part of the Sound. (Aside comments omitted) The species that we found during this cruise were mainly adult juvenile herring close to the bottom and in a scattering layer at about 20 meters depth, mostly juvenile pollack. The temperature maximum was also at about 20 meters depth according to this cruise. Now, it occurred to us if we did find abundant changes from place to place in forage fish that it would be better if after several years we had some ideas about why those changes were taking place. First of all, we are just making estimates of those changes and an estimate of a decrease during a two year period, three year period, may be wrong. Maybe it wasn't a true change in abundance, maybe it was an estimate of change in abundance, so in order to back up those changes, we've decided it would be important to look at explanations for those changes. There are a number of different productivity measures that we can use to make simultaneous measures of what might be happening. If various productivity measures went down at the same time that the forage species abundances went down, then we would have reason to believe those abundance changes for the forage species. So, we divided the project up into two parts, and at this point, this part of the study is not in our budget, we do have some approximate costs for each one of these portions, but the first portion is productivity to energy budgets. A lot of this laboratory work, determination of daily rations, caloric intake, weights of consumed prey for the forage species during selected periods. So, part of this is field work and part of it is lab work. We like to know what the daily rations are in the field to try and make an estimate of that from a combination of field observations and adjusted rate work in the laboratory. We would also like to do an energetic study with field and lab work to determine fish energy budgets in terms of their body growth, their metabolism and egestion. Along with that we could determine growth rates in the field and in the laboratory that would feed into our understanding for the lab energetic study. There are several condition indices that can be applied in field work and verified in the laboratory. For example, just a simple weight of fish divided by the cube of the lent is one possible index, but there are others. The semantic index is similar to the condition index and is important to the use, as well as the (indiscernible) semantic index. In reproductive biology there is an interaction between the development of the gonad and the changes in the weight and fat content of the liver. So, we'd like to also then push into looking the reproductive biology, more as a measure of productivity than as a predictor of year class. We could look at processes that --of gonad development, for example, we'll plan to do a study of -- histological study of the rate of development of olo (ph) sites and condition some work with the fecundity of the fish in different places. If possible, later on in the study, it would be interesting to compare a non-oiled with oiled stocks of these forage species. So, we anticipate then that the productivity measures will become a very important aspect, and contribute to our understanding of the population dynamics of these seldom studied species. Thank you.

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DR. TED COONEY: Thank you very much, Bob. It's a daunting task really to evaluate in a short period of time where the SEA Program has come this past year. I say SEA Program because we are always introduced as the Prince William Sound System Investigation, which essentially is a collection of about fifteen projects dealing primarily with pink salmon and herring. But, a core of that group -- eleven of these projects fall into the category of Sound Ecosystem Assessment, a program that arose from an intensive planning effort that took place in the fall of 1993 in

Cordova, and produced not only the name of -- the acronym -- but essentially the project that you see here. This is a complex ecosystem approach to understanding production trends in pink salmon and herring. It's complex because the system itself is complex. The project is large, eleven projects and fourteen investigators. I acknowledge their assistance and their contributions this year because I'm not going to ask them to each troop up here and talk about their own individual work. I'm going to try to thumbnail that work today, and I hope that I'll be able to give you at least a grand overview of that project. We're being asked in this workshop not only to evaluate the status of the injured resources, but to some extent to evaluate the status of projects, and I offer this SEA Program diagram as a mechanism to think about where we are now, where we hope to be in the future, and where we may be further down the line in terms of integrated field studies, long-term monitoring, and restoration measures -- all buzz words that we have in this -- commonly heard in this association. Let me walk you quickly through this, I think it's important maybe as a model to help us think about what could be approached with this kind of an example. Sound Ecosystem Assessment is a hypothesis-driven, integrated, research project that involves looking at a variety of the elements of the marine ecosystem. This integrated list of field studies has this year, and will continue I suspect, to generate the SEA database. It's this information arising from the cooperative work of the individual products then, or projects, that lends itself then to an analysis -- correlated analyses -- a series of other analyses, that produce then one of the major products of this kind of an approach, an understanding of mechanisms that control a function of the marine ecosystem. Monitoring for the most part tracks populations. Ecosystem approach that's being exemplified by the SEA program attempts an understanding of the mechanisms behind the production histories. Because if that can be done, then we can come to this next phase, which essentially gives a modeling effort to describe the relationships forced by the oceanographic variables and the biology itself, and explore several what-if options that provide the Trustee Council an opportunity to say, if, for instance, we reduce the pollock population to help pink salmon in Prince William Sound, how will that ring out through the rest of the ecosystem. What we're suggesting here is may be inappropriate to go directly from restoration measures from the results of individual projects. But, there is a trap that runs through -- a rationale that runs through, and a tool that is within our grasp to develop that -- already working on, one and two dimensional aspects of the simulation model. These things develop over time. This was SEA '94, it will be SEA '95, I hope. The database, correlated analyses and some aspects of processes and mechanisms have been developed. Experimental confirmation which is very difficult in the natural pelagic ecosystem. It's more the problems of people that work the beaches that can scrape barnacles of rocks, that can put enclosures here. We're in a system that doesn't lend itself so much to that kind of manipulation, but it does lend itself to some. There are some things that can be done with fish enlarged tanks in laboratories. There is a natural experiment that Mother Nature brings to the system every year that we can take advantage of and capture the essence of the outcomes. Simulation modeling then, we'll develop following that, and, lastly, the what-if options, long-term monitoring, driving additional simulations, watch -- once variables have been identified as important in terms of the major mechanisms, and processes and then a period of restoration measures, which are activities that Trustees or their agents will take -- may or may not take given what we find here. We don't propose that the investigators will be here doing these restoration measures. We do propose that this restoration tool, however, is the province of the investigators. This then lends itself to a notion of how this stuff is phased, and when I talk today about some of the results of this project, which started this last April, and we do have results. When we think about this within our group, we're thinking essentially that Phase I of SEA is the '94-'95 season in which most of the emphasis is placed on SEA process studies, understanding the mechanisms essentially that direct the flow of modern energy in this particular system with outcomes favorable for not fully injured species, herring and pink salmon. That as time moves along, this SEA process studies are diminished, SEA modeling efforts begin to come on as these mechanisms are understood, and SEA monitoring which couples to the modeling is essentially brought on board as well. And, somewhere along the line information will become available in sufficient quantities and believable enough so that it will be possible for some EVOS restoration processes -- is a function of understanding of how the system is wired up and works, will be probable. So, we see SEA over time moving in this direction from heavy-duty process studies to begin with, into a more of a model system, a system that's monitored for specific variables and a system that begins feeding restoration information to the Trustee Council. Here is an example of an experiments going on since 1965. These are scattered plots, top of -- hatchery releases, fry out-migration and returning adults. Here are fry index for wild pink salmon against returns, and what you see here is kind of a restoration experiment in a way. That as the fry out-migration increased, and as the fry index increases from natural stocks, there is a tendency for populations to respond in a positive way. But look at the variability around these lines. Less than 50 percent of the variability is explained by the regression. Something else is happening in the environment, and just putting more fry out into the system does not always assure that more fry will come back. That's the kind of black-boxing experiment that I think we want to try to avoid. Try to understand what's in this cluster of variabilities, so that responsible and informed restoration activities can improve.

Well, brief history of SEA '94, ecosystem approach to pink salmon and herring production failures, that was the reason for the long list of studies. This is the first of the ecosystem approaches attempted by the Trustee Council. SEA '94 was derived from an extensive planning process that produced the period you'd see planned in the fall of 1993. I might mention it took the SEA planners a shorter period of time to write the SEA plan than it did for the Trustee Council to give us money to do the plan, (laughter) and I think there's something there that we need to explore sometime in the future. SEA was planned as a multi-component in the integrated and hypothesis-driven inquiry. What this means is we didn't just take a clustering of available projects and slap a name on it and say go to it guys. Essentially, this thing was planned from the bottom up, and while it does include a couple of projects that have been ongoing as part of damage assessment, the other programs, if you look at them, at least are new, and are essentially there because they relate to the kind of work that's necessary in an ecosystem approach. And lastly, as I emphasize in this kind of structural aspect, that the whole program is designed to provide ecosystem level information to Trustee Council, as a basis for informed restoration of the species. If you manipulate the ecosystem in this way by fooling around with herring, with pink salmon, with pollock, what are the likely outcomes, and are they favorable or not, and is this a useful tool. We think it probably won't be. Our approach to developing our thoughts about the SEA program was to come together with information that we have available and would allow us to structure a carbon budget for Prince William Sound, and we did this, and essentially, nobody is saying that these numbers are exactly right. In fact, we may be off by a considerable -- but the trends, I think, are important to look at and they essentially forced the way we thought about the problem. The trend is that if energy flow in that system is primarily from phytoplankton from macro-zooplankton, to (indiscernible) and older juvenile fishes with a major link between zooplankton -- macro-zooplankton and apex predators as well, and that if you structured this thing right, you've got the biggest animals here, medium size and smaller, and the eating is sort of growing in this direction, 0-class fish, herring, pollock, cod, juvenile salmon are not eating these guys, but there is a mechanism for them to eat back into the system, and if macro-zooplankton is playing the kind of role that I think Jeff Short suggested it is, and our evidence suggests as well, but the rehability at this level could essentially drive (indiscernible) switching that would, during times of weak zooplankton force more predation on the 0-class fishes, the younger fish in the populations in these areas. Any ecosystem study has to attempt to try to sideboard the investigation. We'd be stretched from here to Hokkaido if we followed the life history of the salmon, and who knows what little we will have learned by that. We essentially look to the literature and in the history of fisheries oceanography and made the following assumptions that place sideboards on the project. Only history will tell, I suspect, whether we've made appropriate choices here or not. First of all, it's well known that the free swimming and drifting early stages of the pink salmon and herring, that death by predation rather than starvation is probably the major source of mortality. Now, there may be a bigger case for starvation with herring larvae, but certainly for pink salmon juveniles predation rather than starvation is most likely to be the source of mortality, and the rates of loss are modified by ocean (indiscernible). That makes it an ecosystem study when you take the survival histories of the target species and imbed them in the ocean, then effects like temperature and flow fields will influence the way that rates of loss are exemplified in the system. Too, that failure is the normal event in these systems --very high mortality of early life stages. Most of these fishes and the in vertebrates as well produce huge numbers of eggs and larvae. The probability that any one will survive to reproduce is astonishingly small, where the probability that a few will make it is very high, almost certain. Essentially for pink salmon then, there is a critical time in the early life history of these fish, and we think that in the pre-swimming stage, that is the first few weeks of marine residence, and for herring the first few weeks of marine residence as well as time extending through the first two winters, we hypothesize as being important in setting the recruitment success of herring populations. What this means is that it sets time and space limits on the sample. Now, we're looking at the early life stages of these fishes, we're not looking at other than the early life stages. That means we can stay in Prince William Sound, at least initially, we don't have to go outside, we don't have to go to the Gulf of Alaska. Lastly, in a specific case that we have some information about -- from the summer, we thought that physical influences, including freezing, dry and wave actions result in high mortality in herring eggs deposited on beaches, but that additional analysis to predation -- or to predators such as birds can be very significant and that just illustrates a relationship of one of many that we're looking at. If macro-zooplankton play some role in that system, and if the system is noisy, then what I expect to see that the macro-zooplankton stocks would be variable as well, and this precious set of beings, the Prince William Sound Aquaculture Corporation, that the people that raise and release pink salmon in Prince William Sound studiously through the years have collected during the spring time zooplankton samples. It's the only really non-fish database that we have for the region. It's critical inasmuch as it provides some indication of what levels of variability are in terms of total zooplankton collected with a particular mesh size of net during a particular time of the year. These are spring collections made with fairly fine mesh plankton nets. What you see here is that there is some variability. A couple of times, a lot of zooplankton may be some kind of a trend working its way through here as well. Oceanographers are correlative in sort of nature, so we're always pulling this, that, and the other thing up to see if it's related to anything else. And, just because it's related doesn't necessarily mean there is a connection there, a mechanism. It could be coincidental. We did find a relationship between the log of the zooplankton, the (indiscernible - coughing) that was that time series I just saw, or you just saw, and the average information up-welling index measured south of Prince William Sound. This is a measure of coastal convergence, and up-welling is more -- it's force more strongly with the negative values here, or it's actually down-welling this force -- this would be stronger onshore convergents, this would be weaker -- and so we see this relationship that explains about 70 percent of this time series and we interpreted this to mean that there were sort of end points in the flushing regime of Prince William Sound, that under strong onshore flushing, stronger negative up-welling in the seas or strong down-welling, more of the Sound would become involved in flushing, forced by water, crushed against the coast, squirted into the Sound, and then flushed out through Montague Strait. We call this the "river state" because it was the more active state of system. During those few years when the onshore transport in the critical months of April and May, and they are critical because this is the time that these young fish are coming into the system, hatching as larval herring or migrating in or being released from hatcheries of salmon. During weak onshore flow, then we thought that the flushing rates of the Sound would be much diminished, the flow rates might be relegated further to the south and the water would be replaced less, and as a result zooplankton stocks that grew here would not be flushed out, as they might be under the case of the river-like condition. We further then suggested, as a guiding principle for this study that, if you look at both ends of those spectrum, and understanding that there is some gray area in between, that for the river case, the case in which macro-zooplankton was weak in Prince William Sound either seasonally or each year, that one would find most of the energy flowing to higher levels, this would be older (indiscernible), older herring, other older fishes and apex predators being focused on these predators, and the critical early life stages of 0-class or 0-class fishes. But, very little energy would be flowing out from macro-zooplankton to these other higher trophic levels, and under this condition, the system looks like it shifts to piscivory (ph) and very probably the early life stages of 0-class fishes because their of the spineless, they take the heaviest hits under river.

Under lake, we think the opposite is happening. Macro-zooplankton, which Jeff has been able to show, in our collection of summer show as well, then macro-zooplankton is feeding the older predators, larger predators, as well as the 0-class fishes, and the links between these small fish and larger fish are diminished. So, we characterize that as the shift in the nutritional strategy, the system is (indiscernible), and obvious then that under macro-zooplankton sheltering, early in the life history, the 0-class fishes should do much better than under cases of river-like conditions when macro-zooplankton is not here to shelter these young fish from predation.

So what about the results this summer, of SEA '94. We were funded on the 11th of April, the Trustees gave their final blessing to the project. On the 18th, the infamous vessel Alaska Beauty sailed away from Cordova to the western part of the Sound and for an intensive period beginning around the 21st of April and extending through the 21st of July, a multi-ship investigation was present in Prince William Sound. This is a plot from our sea lab buoy in the region showing that the production cycle had already started in terms of phytoplankton by the time we were there. We did catch the declining wind of that production cycle period of zooplankton abundance, second bloom, etc., and some of the measures, particularly the physical oceanography, which was conducted on other vessels, extended on through the month of September. So, a huge data collection effort began at about the time the system was warming up in the spring time. Most of the effort was concentrated on the western side of Prince William Sound. This was very purposeful. It has since been a bit of a criticism to the project that we only did the west-side story. Well, it was very purposeful that we did do the west-side story. The reason for that was we hoped to increase the probability of seeing pink salmon in the stomach of predators by fishing regions where the largest pink salmon populations -- juvenile pink salmon populations would be occurring. It would be on the west-side. Releases from hatcheries at Wally Nurenberg, down here at Evans Island, the AFK hatchery, so, we moved the effort into a region where there was going to be a lot of juvenile pink salmon, we laid out series of stations that essentially covered water coming into the Sound, sort of central basin, water flowing out, and then the characteristics of the water mass in that west-side region. We had a deep monitoring station at Long Island, that goes down to roughly 800 meters, catch vertical distribution of organisms, and we also had the continuous monitoring sea lab setting up by Naked Island as part of the data collection scheme. So, what about the summary of these observations. This isn't everything. In fact, it's not even a complete list at all, but I wanted to give you some idea of the kind of immense data set that we're staring at. If we were not here today, the SEA investigators would be -- have their nose to the grindstone, working this data up. We're in a massive sprint now to get the integrated DPD done so we can get into the field again in '95 a little ahead of the production scheme. We're looking at an end of year report that's going to involve some huge analyses of some of this information, and we all realize that to make this study next year better, we have to know what we got the first year. This is the prior year informing the next year's study, it's an axiom -- what you do in science. You don't just go out for four years and come back with the data and then say, wow, you know, in year two I wish I would have done this because year two is already gone. So, we're trying to do a lot in a short period of time and, well, it seems to be working. Let me just check down this list because it kind of gives you a feeling for size of the database. CTD, this is a (indiscernible) Temperature with Depth, a standard instrument for physical oceanography. The SEA program and some cooperative fisheries and oceanographic studies CTD work that was conducted from 1990 on as well, combine to give us about a thousand measurements of productivity activity, of temperature depth within the region, seasonally, within seasons between years. Huge database. The SEA lab buoy has been sitting out near Naked Island since the fall of 1991. It's a weather data, sea temperature, and we have a barometer located at ten meters below the surface. For '94 we've got data continuously from March through October, for other years we've had -- the year before we had the same kind of database available to us. This is continuous, every hour the SEA lab gives us surface weather, winds, temperature, etc., and measures of plankton florescents -- just below the surface. In a biological sense then, what we came away with was roughly a thousand measurements of phytoplankton and nutrients, different depths, different parameters being measured. Zooplankton sampling conducted specifically by SEA and incorporating the hatchery plankton watch program provided about 320 discrete samples of the upper layers, of 50 meters or upper 20 meters, upper trawl for micronekton, slightly larger organisms, thousands in that range, at about 68. Most of these were discrete depth, opening-closing samples. Large mid-water trawl, this was the mother of all trawls as far as most of us that work around it another (indiscernible - coughing) concerned. And, I'm sorry I didn't think I had the time to show the slides of how you deal with this thing on deck. Maybe in subsequent days if we run into a short spot, we'll show a few of those slides. But, the big mid-water trawl was really the new thing in the Sound. We knew we had to catch big predators, and we know that in order to do that you had to strain a lot of water, so we got -- we had a mid-water trawl that is, you know, tens of thousand on the side, hauled for forty minutes to an hour. So, literally straining close to a million cubic meters of water each time it's being hauled. Seine sets, Mark, can you tell me roughly the number of seine sets that we got this summer, I wasn't able to get that information from you. (Mark - around 500). Okay, close -- somewhere in the range of 500 seine sets. Acoustic transects, 88 transects were set up in the region, they were run at multiple times, but the most conservative guess is that we got close to 1500 kilometers of the acoustic transect. At a second propulse, this generates mega bits of data. Dr. Thomas has probably gotten one of the largest acoustic data sets ever collected in the region, and getting through that is going to take a little bit of time as well. Offshore -- nearshore collection of acoustic information as well, maybe 300 kilometers of nearshore transects, sort of out to the region of the tidal forced fronts that are around these areas. Area bird surveys, 1300 kilometers of that; small boat bird surveys, 170 kilometers; predator stomachs collected, and a good share already analyzed, about 7,000. Experimental fry released, about 15 million fry were grown out -- the larger than one gram live weight, as an experimental part of the SEA program this last year. The reason for that is -- the reason to believe that in the latter part of the declining zooplankton bloom, that fish that were larger than about 60 millimeters in size would enjoy much higher survivals. The late released fish from the PWSAC hatchery had traditionally always had much lower survivals than fish released into the plankton bloom. We always used to think that was because fish released in the plankton bloom had lots of food. Now, we think that fish released into the plankton bloom are sheltered from predation, and that predation sheltering works up to a certain size. After that, the fish are big enough to escape predators. Lastly, about 500 stable isotope samples were worked up, and for purposes of another tool used to evaluate ecosystem structure and how things are wired up.

So, here is Prince William Sound this year, 16th of May, and this was one of the few days it was pleasant in the month of May. Dr. Spies, I think, was about to arrive. I have to mention that Dr. Spies and Dr. Gunther have been awarded honorary CPI status. They went to sea on the Alaska Beauty, and as a special treat we had two T-shirts made up on the ship, but unfortunately the engineer and the AB used them to wipe up hydraulic fluid that seem to be constantly leaking, and to diaper up the decrepit crane and so I'm sorry we don't have those and hand them to you, but please consider yourself part of this crowd. (Aside comments omitted) This was the time of year when the warming was beginning, and you can see around the edge of the system a little bit warmer than not at the edge of the system. So, here's a tool, ABHRR, satellite temperature, we're using routinely to get the clouds apart, pictures of upper layer temperatures. This doesn't work when you get below the surface, and so CTD information allows you to plot temperature versus solidity. This is a routine analysis for oceanographers, and essentially what this plot tends to show is that the water moving from outside Prince William Sound, in terms of the coastal flow in through Hinchinbrook and then leaving -- it's cooled as it moves through the surface, that water -- for reasons of -- and is also -- probably changes its solidity to some extent. It's freshened to some extent and it's cool. The acoustic (indiscernible) profiler which is an extremely sophisticated echo sounder can be used to reconstruct the direction which currents are flowing. I'll let Dave Salmon tomorrow tell you the details of this particular thing, but I show it because it indicates that water at the surface and water at depth are often flowing in different directions. This is one of the major finds this year, and it's a surprise for Prince William Sound, that at the surface we have a counterclockwise cyclonic flow in through Hinchinbrook out through Montague, but in some parts of the central basin, at least, the water below about 125 or 150 meters is flowing in the other direction. We haven't anticipated that this would be the case, and it has ramifications from the distribution, particularly organisms that live near the surface, at sometime during their life history, and then move to deeper waters. As I mentioned, we were lucky to have a mechanism available to us that logged continuously the flowometry (ph) in the upper ten meters of the water column. This is a measure that's been used by oceanographers to give a feeling for standing stock of plants, and I show this to indicate that in 1993 and then again in 1994, we got indications of when this major bloom occurred early in the year, and it was in the month of April. But, I draw your attention to the fact that there was a lag time. This year the bloom occurred about two weeks later, so here is a source of noise that is coming in just in the two years that we looked, that from the bottom up, that may be driving variability further off in terms of things that eat these plant cells and then pass their energy further up the system. It is noisy, the timing of the event -- we've had an opportunity to model as a matter of fact. If we take one of those years, let's take 1993 for reasons that we think it may be a little more normal, if we take all of the hatchery data from the AFK hatchery, which is sort of downstream from that, and the zooplankton data and we plot that, you can see that the zooplankton maximum occurs after lag periods -- a little lag time -- and sort of right in the middle here of this first and second bloom, suggesting very tight coupling between the zooplankton stocks that arise from deep water, and then begin feeding on the phytoplankton bloom over here. These guys are not out here somewhere, but they are coupled up very tightly, and, in fact, some of these low values may be forced by grazing, the diminishing herbivores of the phytoplankton bloom is increased, one, by nutrients dropping out of the system, and two, by grazing losses to (indiscernible). Lastly, a plot from Lake Bay showing the diminishment in nutrients, nitrite and nitrate and chlorophyll, measure of the standing stock phytoplankton. This was out here after the team arrived. The major bloom was here. Later in Lake Bay, on site, we began making measurements -- caught the diminishment of nitrate and decline in chlorophyll. This is something we would expect -- it's nice to have documented here. Macro-zooplankton time series -- what about '94. Well, '94 turned out to be a year that popped up that was a lot like 1990, at least at the AFK hatchery, and let's see where it fits in this relationship between wind forcing and zooplankton abundance. It fits within the cluster of points, and suggests that last year was a little more lake-like than river-like. We're always waiting for surprises, and here was a dandy that came up this year. The time series in front is the zooplankton one that you've looked at here on previous slides, but when you look at additional data from Port Valdez and the Long Merverk (ph) Hatchery in the northern part of the Sound, suddenly years that are river years are not river years other places in the Sound, at least in the northern part of the Sound. So, here was a readjustment of our thinking about what's really going on. Lake River looks like it may apply to the southern part of the Sound not all over the Sound. We got some ideas about this and we'll talk about that later. We thought that copepods would make up the largest percentage of biomass in the region, and they did, but about 80 percent of the biomass consistently of calanoid copepods. We look at large and small copepods, we find that the large copepods have this bloom early on in the season. We'd seen that from hatchery data as well, and we do the taxonomy on this group, we find that it's made up of this *Neocallanus* species that we heard about today in Jeff Short's talk, big copepods, sort of the elephant of the copepod worlds, sitting there being food for larger predators. That the reason that this is interesting to us, is it formed a little, sort of, before, during and then sort of after period to look at whether this copepod was being consumed by little salmon, by larger predator fish, etc., etc. It formed kind of an experimental basis for us. If we look at *Neocallanus* specifically then, this is the big calanoid. Okay, here is what it looked liked -- sampled from the

Alaska Beauty in the upper 50 meters, at the time it was here, on through about the first of June, peaking around the middle of May we would have expected that. Show that wasn't just an artifact, that at the hatchery collections as well, just from that northern station over primarily the deepest waters of the region, here again is this -- this is occurrence of that copepod, scaled a little bit differently, but still occurring middle of May to beginning of June. Present in great abundance, absent again later. We knew from literature what should be happening here, that early in the season these -- the early life stages of *Neocallanus* should be in the surface waters feeding, and that later in the year it should descend in depth. That means that when it goes away at the end of that spike, it's not just being washed out of the Sound, it's actually swimming back over wintering depth. This shows two months later, in July, here is the biomass of that animal. Now, everything is below 150 meters. So, growth from copepod one stage to stage five occurred during the months of May and June, and settlement over wintering depth took these animals out of the surface of the levels. Well, we had proposed a lot of relationships that we were going to look at. (Aside comments omitted) This is what we thought we might see. This is what a kindergarten kid could do something about and could do, just draw arrows between things that were -- the big things can eat the medium size things, the medium size things can eat the little, but little can't eat of the things that are larger. And, so there were a lot of (indiscernible) 0-class gadids, maybe we'd see herring, some other 0-class fishes. We knew 0-class pinks, these critical life stages were going to be there, and we were wondering what was going to happen out here. So, here are confirmations and some surprises of what happened in SEA this year. First of all, we were able to confirm that 0-class pink salmon are consumed by older herring, their consumed by adult salmon and dolly varden, and they are really hit by older gadids, most of the older gadids were plankton. Other fishes turned out to be older squids. We hadn't realized that we were going to find squids in this, but early in the season, that was in May, large mid-water trawls, particularly taken in the evening hours, often caught squid in greater number than they caught pollock. So, squid are a very voracious predator are out there. Squid have this little beak, and as they are eating everything, their -- it's making a mush of it, so if you look in the stomach of squid then you just see this mush. It's not like looking in the stomach of a pollock. So, extraordinary measures are going to have to be taken to see what damage the squids might be doing to some of these 0-class fishes. We don't know, but we suspect that there may be something going on there. (Indiscernible) older herring feeding on macro-zooplankton over gadids feeding on macro-zooplankton, this was spectacular. In the month of May, larger pollocks, seven years old, as long as half your arm, great big mouth on these fish, and their stomach contents, 80 to 90 percent composed of these little tiny copepods. It was during a period of time that the copepod bloom was in water -- that where the eating was going on. How they do it, I'm not sure. Is it gill raking? Are the copepods compressed in a layer that allows these guys to go through and just gulp as they're going, we don't know, but we're going to find out. It's crucial to understand it. We also saw that the 0-class gadids were eaten by older gadids and pollocks, so here already we're beginning to get a feeling for what the major players were, at least last year, pollock, herring, squid -- we have no idea what kind of a player it is, and then these early life stages. We didn't find a lot of juvenile herring. We didn't look long enough, I guess, or in the right place, at least I don't know about it. This isn't the final depiction of this relationship here, or this wiring diagram. As we're speaking, our minions are working at looking at more stomachs, etc., so this is likely to get more complex as time goes on. Here was an interesting result of regard for length frequency of juvenile salmon in predator diet. This is walleyed pollock, the predator, and Pacific herring, the predator. These are the size frequency distributions. (Aside comments omitted) Sixty millimeters and beyond seemed to be about finding any of these little guys. So, right away, we're wondering if once these little fish have grown up to about 60 millimeters, that's about a gram or a gram and a half, whether their nimble enough to avoid most of the predation. Could be that they move somewhere where the predators aren't, and that's why the predators aren't picking them up. There's all sorts of alternate hypotheses, but it looked like, at least the data shows that we didn't find larger pink salmon in the stomachs of pollock. When we looked later in the year, we expect to find larger, but didn't, nor did we find larger pink salmon -- larger than about 60 in the stomachs of Pacific herring. This is the really interesting result, I think, that shows that time series now -- periods like -- this was April up into -- late April, early May, late May, June, late June and July. What you see here is a percent in the walleyed pollock diet of large copepods, and notice in time period two, time when that large biomass of copepods was present that average, somewhere around 65 -- 55 percent of the food by weight in the stomachs was these large copepods. At the same time, look at the percent of 0-class fish, age 0-fish in the stomachs of the pollock. We think that this is a fairly suggestive of, or notion of prey-switching, that when zooplankton is abundant, either seasonally as it is every year, or from year to year, that there will be a diminishment in 0-class fish because these copepods are serving as a dietary supplement. So, as the condition, or as a major result of this year, we see some confirmation for our idea that as these copepods, early life stages, come to the surface, grow and form up a big swarm covering at the surface, that the fry that are entering either from hatchery releases or naturally from streams and small rivers in the region enjoy a period of predation sheltering that disappears when the allogeneic behavior of these copepods takes them away from the surface at the end of May and early June, and that should then open up a period of increased and intense loss, particularly if the little salmon here haven't grown to this magic size of about 60 millimeters. If they pop out here when the sheltering zooplankton stocks have gone to depth, then they're likely to get hammered. Well, the remaining figures will illustrate some of the work that's been done. A number of stable isotope analysis generally show a relationship that was to be expected essentially between carbon and nitrogen, ratios of carbon and nitrogen, such that here at the lower end of the plankton communities, farthest in are the fish communities, and there some blending in between. This is not exactly straight-forward data to talk about, there's a huge amount of information on the slide and I ask that people are really interested and want to come tomorrow, Tom Kline, as other investigators will, talk in more detail about what they found. But, this was a soothing result from the stable isotope work and something that we expected to see and was basically confirmed. These are the results of the pen-rearing fish at Molly Nurenburg (ph) and AFK, released from a couple of pans, numbers, as I've said before, right around 15 million fish overall. Some were released at about a gram and a half, others released at about a gram. That's the experiment that's ongoing now. Those fish underwent some mortality at the time they were released relevant to late released fish in the past and they're going to be back. (Aside comments) So, at any rate we won't know what the result of growing these fish larger was until we can harvest them, or they are harvest in the common property fishery this fall. I mention that we had a lot of acoustic transects, just kind of a rendition of where those acoustic transects were in the Sound. This was the southwest Sound survey. We also had extensive transects in the northern part of the Sound. These transects were occupied a number of times during the period early or late April through late July. Here's a kind of visualization of these transects showing the data placed now in -- this is Montague Island, so we're in Montague Straits, can sort of worm your way through the topography, the (indiscernible), gives us some opportunity -- later when we get them in three dimensional aspect of this thing for data analysis in that regard. Here's the kind of data that Gary collects in hundreds, thousands of mega bites, I suppose. The ship that's moving -- measuring the target strengths, the collective ability of sound from organisms in the water column, understanding that as the organism gets bigger there almost always reflects more sound. Gives them an opportunity to look at big and little transects collecting, as I say, huge amounts of data that will later be reconstructed into an understanding of the distribution of large fish, little fish, plankton, etc. One of the projects this was year was to look at avian association with spawn. It's not a nice thing apparently to go out and shoot a lot of sea birds. Those of us that form (indiscernible) on copepods are not too concerned about that, but if you're going to find out whether herring are eating -- whether sea birds are eating herring eggs, then, you know, look with glasses or notice associations, and that's sort of where we are now, and I know that I said that sort of in jest, but there's some problems with that -- the emphasis of that kind of measurement, we're sort of between a rock and hard place in some of these higher trophic levels, trying to confirm specifically what we're going to see. As we watch them and we try to draw conclusions about what they're feeding on, in some cases, subsistence harvest -- see what they're feeding on, but in many cases these birds are protected. We're not suppose to be out there blasting to look in the stomachs and that causes some problems. This was a good way of, at least approaching.

I'm getting close to the end now, and I just wanted to talk a little bit about modeling, that was an important part of the project that is the simplest slicer, kind of the integrator of everybody's results. Here, using the SEA lab data, are models driven principally by the physical oceanography and the weather that pretty much captured the essence of the spring phytoplankton bloom. It's not good after nutrients had disappeared, but two things are caught here, the general feature of the bloom is caught, and more importantly, the offset of '93 and '94. That suggests to us that we're collecting the right variables and we understand the system enough to be able, at least get crude models of this, that will be refined as time goes on. Another kind of modeling exercise, the simple sort of one dimensional model in time where zooplankton can be consumed by fry, pollock can consume zooplankton, they can consume fry as well, and then you can set up certain conditions to look at what happens as, for instance, zooplankton populations are diminished. Turns out that the next thing to be eaten is fry by pollock, and so you can do these sort of simple prey switching, and you can begin to look at these systems with simulations that are available for the PC, this was done on Matkin (ph). We are all going to learn to do this kind of thing since we think it's an important tool to address these. The concept issues start simple, and you get more complex, and you match it up with what you're measuring in the environment. These things get large and time consuming, but presumably much more interesting in terms of the information available and much more interesting in terms of the ability to test notions about what a particular restoration strategy might do to a system. This is just -- again, a kind of a diagram of what we think are -- one of our end point modeling efforts will be. This is the present status of herring populations, pink salmon populations from hatcheries and wild stocks. The SEA program would like to take the credit for restoring these populations this past year. (Laughter) I know that a lot of the work we did lent itself directly to that, and I hope the news releases will so indicate.

Let's not be lulled into a sense of security here about what may be going on with the pink salmon. It is true that the return this last year was the third highest. It was a return from even-year fish. We've heard today that the odd-year fish were probably the ones that were most affected. Let's look and see what happens this year with the odd-year fish, and then let's let the resource managers decide what criteria will be used to determine whether we have healthy pre-spill populations or not. These time series, gathered well before the spill, show that these populations can go through a variety of changes that were independent of or not forced by anything that happened in the oil spill. So understanding and reaching some conclusion about what's recovered is going to be not attributable. Thank you very much.

DR. SPIES: (Aside comments omitted.) Our next item is on herring, and that talk will be delivered by John Wilcock, Department of Fish & Game, Cordova office.

MR. JOHN WILCOCK: I'm John Wilcock, I'm a herring biologist with the Department of Fish & Game, and primarily I'm going to speak about the time series of herring abundance that Ted just presented. One of the problems with herring -- slightly different from pink salmon is that it's really hard to tell how many critters you've got out there, and that's no mean problem just trying to decide that whether to figure out whether you've restored population, or whether you've actually, indeed, got damage. The main herring project, the spawn deposition project, has been ongoing since '89 with no data gathered in '83. It was an outgrowth of our department's typical stock assessment program. Typically, the department uses three techniques to assess herring biomass, and two of them occurred during the spring when herring migrate towards their spawning grounds, that's aerial surveys of fish abundance, aerial surveyors with the department in a -- usually a 185 or a Super Cub, fly around and look for schools of fish, estimate the abundance and add them all up. Later, after spawning occurs, they calculate the amount of spawning that occurs which is estimated from the amount of milk seen in the water, the water gets real milky and it's very obvious from the air what is going on. In spawn depositions, scuba divers count the eggs and then back out -- calculate the number of spawners that were required to lay that many eggs, knowing something about the biology of the animal. The third technique typically happens on fall herring, winter aggregations, around the Green Island area or Knowles Head. And combining all of these pieces of information about herring abundance that give us some indication of the biomass, along with the age structure population, it is all rolled into a big modeling exercise to give our best guess at what we believe the current population level is. The pieces off of this graph to pick out are the spawn deposition program, which is our main biomass tool, and the aerial survey escapements, is our longest continuous time series of information that has been collected since the early '70s in the initiation of roe fisheries in recent times. If you look at this, you can see that the abundance indicated by spawn deposition is somewhat different from that indicated by aerial surveys in the last few years. The other technique, using sonar acoustics during the fall is something we've just begun recently to do again after it was done in the past with very limited funding and real shirt tail, seat-of-the-pants sort of studies. In 1993, the fall of '93, we got an estimate of 20,000 tons and with discussions with Gary Thomas at lunch time today, I guess our current estimate of -- 20,000 for 1994 is down to 5,000 to 6,000 range right now. Using the sonar, the spawn deposition and the aerial survey all rolled in together, constrained age constructor, you can't have new fish show up that presumably died in the previous year, our biomass projectory (would be that orange line), showing general increases through the -- right up until

1990. Serious declines in 1993, no fishery harvest, only for the gillnet fishery and some of the other user groups. The primary harvest was curtailed, no fishery in '94 and none is expected for '95. To give you a scale of what the harvest is like, we needed threshold biomass (indicated by the black line) in order to initiate a fishery, (and the bars indicate how much harvest), shooting at a maximum of 20 percent harvest of the population in any given year. In 1993 was the first year observed of the decline in herring. Not knowing what had caused the decline, we did as much sampling as we possibly could, looking for a needle in a haystack, to try and figure out what it was that caused the decline. One of the unusual things about '93 was the reports from fishermen and from personal observations of biologists on the ground of white spots on the fish. When we looked at these a little closer, it turned out to be external lesions, pretty serious. In 1993, these moderate to severe external lesions amounted to about 20 percent of the population, and the question what caused the lesions. In 1993 a virus was identified, viral hemorrhagic septicemia, and it is unknown exactly whether it can cause mortality or lesions. The laboratory studies to find out whether it can indeed cause lesions or mortality have yet to be accomplished. In 1994, observing a decline and nowhere near the population we had hoped would show up, we did another intensive sampling round and did one of the most complete disassembles of 233 fish, I've every been witness to. We selected probably 30 different tissue samples from every herring we disassembled. It was long 16 hour days, and some of that has got to be some of the best information ever collected about herring. They went kicking and screaming into the lab to be dissected. All of those were collected near Rocky Bay, right during the peak of spawning in 1994, and in addition to observing the VHS, the pathologist, Dr. Gary Marty, who will speak in more detail about herring in coming presentations, identified about 30 percent of the sample to have ichthyophonus, a fungus that is a well-known killer in Atlantic herring and from 1898 to 1947 there appears to be typical 16 year cycles associated with occurrence of ichthyophonus and Dr. Marty will speak more about what is known about that one. The question is, is this a cause of the Exxon Valdez? It isn't truly known at this point, but one indication might be this. The age five and six year old herring, there on the X axis are the 1989 and '88 year class in 1994. (The bars in front are the age composition of the entire samples. The yellows bars in back are the age composition of those fish with moderate to severe external lesions.) The interesting thing is that the moderate to severe lesions are not greatest in the six and five year old fish. If anything, they're slightly smaller than either young fish or old fish.

Given those trends in the population abundance -- so what are we planning to do in '95. Primarily, since spawn deposition is our principal biomass assessment tool, we will be conducting a spawn deposition again in association with avian predation study. Our ongoing aerial surveys, although there won't be a commercial fishery, the department will indeed be out there with aerial observers and counting fish schools and assessing distribution of spawn. A new project to be started in '95 will be looking at the processes in survival. Juvenile growth and habitat partitioning is Evelyn Brown's project, and -- I'm sorry, Brenda Norcross's project in association with Evelyn Brown, and a bioenergetics with A.J. Paul. There's a genetic stock ID component because we don't really know exactly how these fish distribute and what their migration patterns are. Hopefully we can answer some of these questions. A raft conjecture about stock composition and stock modeling has been written and is being distributed to anyone who is interested, for comment, for review, for criticism, and is solicited and welcomed. In addition to those processes in survival, there will also be a reproductive impairment and disease study planned. The disease study is to be awarded through an RFP process, which I assume will be completed prior to herring spawning season this year. And, reproductive impairment study with the Auke Bay lab, and Jeep gave give you the basics of that earlier. That's pretty much what we've got going for 1994. It looks like a herring decline and our plans for '95.

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DR. SPIES: (Introductory comments omitted)

MR. MARK WILLETTE: I was asked to review the '94 pink salmon projects with a particular emphasis on status of recovery of pink salmon. Because most of the projects I'm going to be talking about today were conducted by other people, I'd like to acknowledge this, in particular Sam Sharr (ph), Brian Bue (ph), Al Geiger, Jim

Seeb, Vincent Seeb, and Steve Pottle. The projects that I'm talking about today were conducted by those people. The injury to pink salmon can be classed as short-term and long-term. Short-term injuries occurred at the embryo stage, primarily in 1989 and 1990. Mortality was apparently due to direct oiling effects. Also, in 1989 there was reduced growth, due to exposure to oil, which likely caused reduced survival, although the evidence for this is indirect. Long-term effects are hypothesized to be due to genetic damages, which may have resulted in reproductive impairments and ecological effects have been hypothesized, particularly in regard to the '92 and '93 returns to Prince William Sound. This is the -- these are the oiled and non-oiled studies sites that were used for the project that I worked on, which examined the effect of oil on the growth and survival of juvenile pink salmon in Prince William Sound. Juveniles were collected in these areas in '89, '90 and '91. Growth rates were lower in the oiled area in -- both oiled areas, those where fish from the Molly Nurenburg hatchery-reared and fish from the AFK hatchery-reared, and they were not lower than expected in the non-oiled areas. These expected growth rates were calculated from characters in each of the areas. The growth rate of juveniles, in 1989, was related to their survival, and survival was measured by recovery of coded-wire tagged fish one year later, and this relationship was used in conjunction with the growth rate differences in previous figures to estimate the effect in terms of the number of adults. This figure shows the oiled and non-oiled study sites of streams that were used in the embryo mortality project which was run by Sam Shar and Brian Bue. Embryo mortality was elevated in the intertidal zone in 1989, and in 1990, primarily in the upper intertidal zone -- in the upper intertidal zone, which is called the bathtub ring. In '91 and in '92, investigators were surprised to find that mortality rate were actually greater than in the previous years and occurred in all tide stages -- tidal zones. It was this evidence which lead to the hypothesis that there may have been genetic damages, and subsequent laboratory studies conducted at the AFK hatchery, in which embryos were taken from oiled and non-oiled streams and various incubators, corroborated this hypothesis. In '93, the mortality difference had declined somewhat, but was still significant, and the most recent year's data shows no difference between the oiled and non-oiled study sites at all tide levels. This is a summary of results from a life history model that was put together by Hal Geiger and others. It attempts to estimate the lost production resulting from oil and effects in '89 through '94. This is expressed in terms of brood years, so the '88 brood year would be the juveniles that out-migrated in '89. As you can see, the greatest effect was on the '88 brood year. The number at the top of the bar there is the estimated loss production in the southwest district, and it's expressed in millions, and it's 1.6 million. It's important to note here that this is the only -- the estimated loss production for wild salmon, that there likely was also significant mortality for hatchery produced fish, probably in the millions. The effect for the '89 through '92 brood years was substantially lower, and for the -- particularly '91 and '92 brood years, these effects are presumably due to genetic damage. This is probably a familiar figure by now, it's the time series of pink salmon returns to Prince William Sound. This is the wild stock and hatchery stock. Obviously, pink salmon production fluctuates tremendously from year to year, and because of this, studies such as the embryo mortality project that I just summarized are really essential in order to detect oil and effects, because simply looking at changes in populations are not going to show us any of the oiling effects. Since 1989, we've had some very strong returns, obviously, these returns are, for the most part, I believe, driven by environmental conditions. In '91, '92 and '93, the returns were rather unusual. Maybe you may already know about this, but in '91 the return was rather large, but the fish came in very late, and when they did come in they all came in at once, and they were very dark, and as a result the market for the fish basically disappeared, and there was significant economic loss to the fishermen in Prince William Sound. In '92 and '93, the run largely failed, and the thing that really surprised people about this was that the hatchery returns had failed as well. This had never really happened before. The fact that the hatchery loused up, populations are fluctuating rather coherently here, indicates that conditions in the ocean are likely causing the changes in population size to a large extent. I say that because the hatchery fry are -- the pink salmon in the hatcheries are protected during the egg to fry stage, and so changes in survival rate are due to conditions in the ocean. The return in 1994 was actually very near average. The survival of the hatchery fish overall was five percent, which is exactly the long-term average, and the return of wild stock was 8.2 million, and the long-term average since 1960 is 8.5 million, something like that. There is essentially four means by which the Trustees might achieve resource recovery for pink salmon. Various projects that are being conducted now, as we get into these different categories, are the Alaska Department of Fish & Game that is primarily responsible for the resource protection part of it, and there are seven different projects that are involved with this; the Trustee Council has funded projects which are primarily devoted to promoting resource recovery; the embryo mortality project is designed to monitor recovery; and the SEA program, which Ted talked about earlier, is looking at factors limiting recovery. So, I'm going to go through and briefly summarize the '94 results, those that there are, from these four projects.

The coded-wire tag recovery project is designed to provide in season stock composition data. This is needed to protect wild salmon. It also estimates that wild and hatchery return, as well as the survival of hatchery salmon. In 1993 approximately one million juvenile salmon were coded-wire tagged at four hatcheries in Prince William Sound. This is from a total release of about, I think that adds up to 550 million. Believe me, it's no small task to insert these tiny coded-wire tags in one million fry that are about an inch long. These are the fishing districts in Prince William Sound which are used for management of the fishery. The southwest district here is where most of the adult salmon enter the Sound, and it's also a mixed stock area where exploitation rates on wild salmon can be fairly high. In 1994, in-season stock composition data provided by the coded-wire tag program indicated that the wild stock contribution was rather weak, even though the catches overall were quite strong. This information was used to set the management strategy for 1994, and as a result the southwest district was closed and most of the harvest occurred in the northern part of the Sound, and so, the coded-wire tagged program certainly, I think, contributed to protecting the resource in 1994. These are comparisons of the in-season stock contribution estimates to the post-season estimates. In-season estimates are derived from the detected tag method, which involves passing the heads of fish which are collected from the canneries by a tag detector. All we know is how many tags are out there based upon the number that are detected. So, the post-season estimates are derived after the tags are actually read, and we have the data from each of these tagged totals, so you can see the estimates match up pretty well, so this supports using the detector-tag method for in-season management purposes. These are the survival rates of the pink salmon that returned to the four hatcheries in Prince William Sound in 1994, which was derived from the coded-wire tag program. It's very interesting the trend in survival in 1994: rather low survival at the AFK hatchery in the southwest, and turning upward to the Solomon Gulch. The wild stock production pretty much mirrored this pattern of survival of the hatcheries, which again indicates that perhaps, at least in 1994, the patterns of production in different parts of the Sound were determined by conditions in the ocean. The otolith mass marking project is evaluating tetracycline marking for use in wild pink salmon populations. The ultimate goal behind this project is to use tetracycline marking to examine strain of wild salmon in Prince William Sound, which will contribute to our knowledge of the stock composition. The primary goal of project in '94 was to look at the minimum immersion timing and temperature needed to produce and detect the mark. It is important to know what minimum immersion time and temperature is because these two factors have a very strong effect on the feasibility of using tetracycline marking on wild pink salmon in remote camps because you can only deal with so much water and you can only heat so much and so forth. This is the basic study design used for the tetracycline marking evaluation. There were four immersion time and three temperature combinations resulting in 12 (indiscernible) groups. For each (indiscernible) group there was five replicates and one control, and in each one of these replicates there were 600 fry. We really do not have too many results from the otolith marking project because they haven't all been worked up. Approximately ten have been analyzed from the highest group, and all of them were marked. The otolith marking project next year is going to involve mass marking of all pink salmon in the hatcheries in Prince William Sound using thermal marking, and this is expected to replace the coded-wire tagged program in future years.

The pink salmon genetics project is also designed to obtain information on the stock structure of pink salmon in Prince William Sound. It's important that we note the stock structure because the stock is the base of unit used for management. One of the things that this project is going to do is compare results obtained in the '90s to those obtained in 1976 by Jim Seeb and Lisa Seeb, who was Wishard (ph) at that time, and in '76 study there was 37 sites that were sampled. In the 1990's study, I believe, that was 18 sites that were sampled, the odd-brood line, and 43 sites that were sampled in the even-brood line. These are actually spawning aggregates, they're not particular sites. Approximately 50 percent of the samples that have been collected in the genetics projects have been lab processed to date. There is data available that has been analyzed for only ten sites, and the results from these ten sites indicate that there are not differences between intertidal and upstream spawners at two streams that have been looked at. However, in the '76 study there were no differences in those streams either, whereas there were intertidal and upstream differences in other streams in the '76 study. Also, there is clear genetic heterogeneity among the 10 study sites that have been analyzed; however, it's premature to say whether that is geographically based.

The final project I'm going to talk about is the water fall free fish pass, which is down in Afognak Island, it's located here in the north end of the Island. It is going to replace an existing fish pass. The gradient on this fish pass is too steep for pink salmon to use effectively. The proposed pass will involve three sections with two resting ponds and the project will provide access to 24,000 new squared spawning habitat above the barrier which will provide for approximately 24,000 pink salmon spawners. And, in summary there is no difference in embryo mortality between oiled and non-oiled areas -- or streams in 1994. The pink salmon return in Prince William Sound is very near average for both the wild and hatchery stock in '94, and there is a rather interesting gradient in survivals of hatchery salmon and also wild salmon, with high survival abilities to poor survival in southwest. That's all I have.

DR. SPIES: For those of you wondering how long we might be, Judy Bittner has volunteered to combine her comments on the archaeology section with the workshop subgroup on archaeology which is going to be held on Thursday, so that will buy us a little more time ... (aside comments omitted). The next resource is the sockeye salmon and that will be addressed by Dana Schmidt of the Alaska Fish & Game office in Soldotna.

MR. DANA SCHMIDT: I'm going to assume that most people are somewhat familiar with the sockeye projects, that have been going on since, approximately five years now. Just a little brief overview of what the primary ones, although the different studies have been initiated for different reasons. The sockeye programs are different in the sense the impact on sockeye salmon both in Kodiak Island, Upper Cook Inlet and Prince William Sound was not a direct cause of oiling. It was caused because of the disruption of a management system that was in place, which allowed relatively large numbers of sockeyes to escape into their parent system and in essence overwhelm the rearing areas, that created the subsequent effects on the trophic status of the lakes where they rear at, and consequent effects on recruitment. The one exception to that is Coghill Lake in Prince William Sound, which is a study that was initiated (indiscernible - coughing) overescapement impacts, as it was looked at replacement fishery for other damages in the Sound, and trying to rehabitat a system that was declined for apparently other reasons. Coghill, I'll describe first, is a unusual lake in the sense that it is a merometric lake. It has a salt water layer that isn't -- we're not quite sure what the dating of it is and when it came in, it might have been in 1964, and may have been associated with the earthquake. The salt water layer at the bottom of the lake, it's sort of a nutrient sink, that is, any nutrients that come in on the form of carcasses or from other upland sources, once they hit the salt water layer they are lost to the primary production of the system than in the surface water layers. Consequently, their productivity is usually less than other systems and they continuing go down hill. The story of Coghill is kind of interesting. We have two -- or actually three sets of data on this graph. The return for spawner data is the upper graph. As you can see it's run -- this is a log scale, but it is run somewhere around -- as high as up in the 30s, but more running typically in the range of three to five, which is typical of most of our sockeye systems, and all of a sudden you hit this cliff here in 1985, and this is the brood year of 1985. Where, in fact, the rate of return from those spawners was very low. You can see this is the harvest where the total return back, and this is the escapement values for the different years over time. The cause of nature of this collapse is certainly -- didn't correspond with an effective oil -- it predates oil effects, but in fact is -- it was very catastrophic. It caused essentially concern about the sustainability for this stock to be -- to continue. Escapements have been typically in the very low numbers and there have been other active hatchery work going on as well, in terms of working the stock. What I'm going to talk about is the fertilization program that was initiated in 1983.

The fertilizer was added to the area that you saw on the original slide, during the course of the summer. Fertilization is not a new function in Alaska. The lab which I supervise has had 16 projects going on in different parts of the state. We have eight active ongoing, and we have quite an extensive database that we have developed these procedures from. Fertilization projects in the Canadian area, down in Vancouver Island, have been going on for as long as 25 years, and have been able to sustain or improve the fisheries, the sockeye salmon fisheries, from those systems quite significantly. We have several publications out on other systems if people are interested about details as to effect of nutrient enhancement on sockeye salmon production. Coghill is interesting in that we know that these nutrients are kind of a one time thing, that is, most of them are going to be lost to this merometric layer, and we are looking here at the effects of return for smolt per spawner both pre and post-fertilization. The total number of smolts being produced from the system is represented by that graph, and the total number of smolts per spawner after fertilization has made a real dramatic increase. Whether this is simply a spurious correlation or not, we also have information on the chlorophyll levels and on the zooplankton, which are very indicative of a response typical of what we see in other lakes that have been fertilized. This is the Coghill A levels near the surface in a time series starting from 1990 and going up to 1992, and this is upon the initialization of fertilization. You see primary productivity is increased significantly. Anyway, we feel this project is quite successful and we want to continue it for a typical five year -- five year stint, and we will -- we're planning on having the project in this coming year.

I'm going to jump now to other areas. This is on Kodiak Island. Red Lake, there are several systems in Kodiak that have had overescapement during 1989. Two of the lakes that we're actively study, plus an additional control lake, are Red Lake, which one of the larger sockeye salmon systems on Kodiak that contributes approximately 15 to 20 percent of the island's sockeye salmon commercial harvest. Not too distant from that lake, maybe about ten miles away, that drains into a different bay, into Olga -- Mosier Bay, is Akalura Lake. Akalura is a small system that is -- has been depressed for many years, but demonstrated a recovery during the '80s, and now has subsequently gone through a collapse corresponding with the large escapements that were put in there in

1989. This is just an overview of what has happened since we started smolting the system in Red Lake. We had -- a couple of things I'd like to point out, first of all, there is still a mixture of age classes. This is the number of years that sockeye juveniles are living in the lakes. We see a shift from age -- of about 50-50, this is 32-66, and 40-45, and all of sudden there is a big jump up here to 96 percent age two. This is a typical response we get either in adjustment to a severe climatic conditions or to a density dependent response from rearing, and it creates -- and in this particular case, it matches up very nicely with the density effects from the oil spill. The larger numbers of smolt that were produced from the thing, from the system, we attribute to a decline in the zooplankton population, which has been reported over the last couple of years. This has subsequently recovered, and consequently we have seen relatively restoration of numbers. In '94, however, we see this jump back up again, which is -- that is somewhat concerned over what we're seeing also, another depression of the zooplankton population this past year and though escapement levels have remained relatively constant throughout the system.

In Akalura Lake, this is a similar set of data. You can look at the age class composition of 14 percent and 86 percent. This system has always had relatively smaller smolt and would typically they stay over two years in fresh water during the duration of the study. However, we've seen just the opposite effect in 1994, we've seen a shift in composition. However, the numbers here are relatively small, compared to what we had hoped for this system to produce, so -- although we think the lower densities might be showing a response, it still a pretty dismal output in terms of future fisheries developing off of the stock. This is was particularly clear this year when we had very poor escapement. This escapement goal was approximately 30,000 and we only had 13,000 escape into the system, suggesting we're getting really weak years off of the post-oil spill years, and we expect this system to be somewhat depressed for the next several years, and having a difficult time meeting escapement. Hopefully, however, the plankton community appears like it may have been responding to the lower densities, so we might get a rebound. We're going to get now into the Kenai River, and probably the Kenai River studies have been in the newspaper over Christmas period, and most of you probably looked at them. In the first talk, we have several different studies that are being funded. Some of them are related directly to trying to understand the system and the density dependent effects of too many fish on the system. The other studies are designed both doing inventory of the stocks in Upper Cook Inlet for possible future enhancement, as well as enhancing our ability to manage the systems. In addition, we are doing studies to help doing in-season stock separation through genetic stock ID's. The goal of the salmon stock separation, there are two projects involved, and only one is listed in the program, it's 94255 and 94504, is to restore the Kenai River sockeye salmon to improve stock assessment, and to obtain this goal several objectives were developed. One is to obtain the genetic data of Cook Inlet sockeye salmon, and the other is to develop a model to estimate the proportion of Kenai stocks and mixed stock fisheries and mixed stock fisheries in Cook Inlet, and finally to provide more accurate estimates of abundance of Kenai River sockeye through hydracoustic techniques. The 1994 results, some of the highlights of what was completed with this, and this study is going to be talked about a lot more tomorrow and is available to -- from Lisa Seeb who is the principal investigator, Kent Tarbucks (ph), I have very limited to do with these studies, has been the principal investigator of the sonar work in Cook Inlet, and the development of these tools to -- are totally related to improve management of fisheries, so the subsequent weak returns we expect this coming year and the next year can be more effectively handled by the department to achieve the highest level of escapement of managing the fishery that we've -- and the problems we expect from it. The genetic data were collected from all major spawning locations in Cook Inlet, and I'll show it's extent in just a minute. They found that significant differences do occur in the Upper Cook Inlet sockeye salmon stocks, and that mixed stock analysis, algorhythms (ph) can be used with a high degree of accuracy to estimate Kenai River contribution to Cook Inlet fisheries. The 1995 proposed research objectives are to include drift and set net fisheries, which will be sampled throughout July and in-season analysis over a 48-eight hour period will actually be used to make opening and closing of commercial fisheries. Genetic information will be considered in any restoration or supplementation proposal. This is an indication of the extent of the program that has gone on over the last several years in developing inventory. All those numbers include samples that have been taken for genetic analysis to provide a reference base for the different kinds of sockeye systems that we have. This gives you an indication of how these systems classify out through statistical analysis of the genetic database that has been accumulated -- 1992 and '93 database. Some of the things that are most interesting to me, since I primarily spend most of my efforts working on the Skilak-Kenai systems, is how well -- how nicely this group of systems drops out from the rest, indicating a significant degree of reproductive isolation, and that although the Russian River system, which also includes the Kenai drainage, is essentially totally independent of the main stem spawners that we have been most concerned about. The other things are, as you'll see such nice match up of '93 and '92 data, which occurs very often through that data set. This is a real good indicator that, in fact, these stocks are truly well-sampled and represented by the genetic information.

I'm going to talk now about probably what most of the concern has been, and this has been the overescapement in the Kenai River. As I started out saying, there has been a lot of press releases and information concerning this stock, primarily because of the allocation concerns between either the Susitna fishermen and the Kenai fishermen or between sport fishing interests and commercial fishing interests, and the fact that this past summer the forecast we had for very dire returns turned out to be over pessimistic, and we had substantially higher returns than forecast. This is essentially the relationship of spawners, the main stem of the Kenai River, to the subsequent fall fry we've measured in the system. This regression has actually gotten poorer this past year -- I've used it before -- but we find that the '92 is -- fall fry density were substantially higher than we would have forecast. This is a good indication of the excellent rearing conditions which we had in this system, and we also had in the nearby Tustumena Lake, which we've collected some of the late fall fry we've had in the 13 year history of sampling the system. To show you one of the things that we use for comparison, this is different data than I've presented previously on this subject, is we examined relationship of fat content going into the winter with length. This is the data for Tustumena. I'll show you, it takes individual data points out here, I've presented those last ones so you could see the kind of variability around it. But, in essence this is where we start in August. Keep in mind in Tustumena Lake, the fry size and the percent living has no relationship either -- insignificant. There is no slope to these lines, and we also note that the values, 25 percent dry weight to lipid content, which is probably what we would consider normal for sockeye of this size, we can see in September a slight drop, and essentially the same value in November going into the winter. However, we went to April of '94 -- this past spring -- this is essentially what we see, the kind of weight reserve that is lost of these fish. We also see a shift in this line, the mean of the population shifted to the right, indicating there has been some mortality associated with the population, and it appears to be, at length based and not density based. When we get to Skilak, which is what we're using Tustumena as a control for, we have been trying to understand the mechanism by which we get decreased smolt production in the system. This is the fat content as a function of length for Skilak, and essentially it's paralleled the other set of data. First of all, we see this is a very significant length relationship, both in August and in September. The smaller the fish, the lower the fat content is, and when we get down from experimental work that has been done in other places, when you get down at numbers below five percent, mortality starts occurring in sockeye. What we see is even by November this line start flattening indicating some early fall mortality, and by the time we're into April this line is very flat. One of the things you might notice, it hasn't dropped any compared to the other graph, however, all of these data essentially started out with the same percentage of fat content that Tustumena fish were at in May of this past spring. That indicates, at least to us, that these fish are going into winter at much poorer condition, and consequently most recent this has not dropped because it can't drop. These fish are all dying, all those that are falling out, so the remnant population is there. The bottom line here is a different population. In May, when we sampled these, these are all hold-overs of fish that are going to hold over to age two, and stay a different year in the system, and their fat content is really marginally, but they are much tighter clustered than the current ones. Again, this systems are 20 miles apart, they are both glacial, they both have very similar climatic regimes, that we see a very differences in response. The one that has the poorest rearing conditions from the zooplankton perspective, either by density or by numbers, biomass per fry, is Tustumena, yet it seems to grows fatter fish at a heavier weight. This kind of compares both systems together. Essentially the bottom axis is the number of zooplankton biomass per fry, and the Y axis is the mean fall fry weight. The upper line here is the relationship of fall fry weight to this lower ratio of Tustumena Lake, and the data of the squares you see here, or the data since 1987 to '93. Those are split out from 1981 to 1986 because we believe the fall fry weight was driven by hatchery plants from the Crooked Creek Hatchery at that time. These are pre-feed prior to release into lakes and made up anywhere from 30 to 50 percent of the age one smolt that came out of the system, and consequently the weight relationship was lost. Since that time, since about 1987 through 1993, we've seen the relationship holds quite strongly and the contribution from hatchery fish during this period has never been above ten percent, because the reduction went from 18 million plants to 6 million. It was a real major reduction and the effects on fall fry and the number of fall fry that were contributed by the hatchery. In Skilak Lake we see a significant relationship here as well, but the line is very different. First of all, fall fry are much poorer as I indicated with (indiscernible) graph on fat content, but in fact is, this is the response to the increased zooplankton biomass to fry ratios is much shallower. We're aren't getting this effect. We think -- we don't have data that goes back into the era when zooplankton -- or when we had the major production years from Skilak Lake that produce the middle to late '80s strong year classes, but that whole -- we believe this curve has shifted down for some reason inherent within the zooplankton community in the lake -- and consequently that relationship is what has caused the decline in productivity between the system. This is another piece of data that we have that indicates the density dependence in the Kenai River sockeye salmon. The number of spawners is on the X axis and the overall weight of fall fry. As you can see, the densities, how they drop off, have been fairly consistent over time. We can see kind of in '93 this data point has just been drawn on, fits in this overall relationship. Clearly, there is a fair bit of variability at certain conditions which probably reflect chances in summer rearing conditions. In fact, this relationship is quite obvious and very much indicates a density kind of relationship.

Now we get into some of the more thornier issues. This if forecasting what is going to happen in the future, and the liability of our smolt program, which lot of the work and a lot of the hypotheses were developed upon. It reads -- it's very confusing sometimes to the public when we fail to forecast correctly. Well, we almost always fail the forecast correctly, and for a lot of reasons. The forecasting has a lot of elements built into it that are based on marine survival and unsure estimates of such parameters such as smolt and sibling relationships that don't hold too far. The other thing is stock composition. We talk about one system failing and another one is doing real well. The balance that you'll show up in a commercial fishery sometimes is much better than an individual system will be doing. This kind of shows one of the points is that you see the dark part here is the Kenai, which is what we talk about as a major contribution to the fishery. In 1994, the forecast of component was approximately 45 percent, it came in at 61.2 percent, which is quite a bit stronger and was a major contribution. But, the other systems were quite high too, which gave rise to a higher number. This kind of shows the kind of relative contribution of the different problems in '94. The Kenai system was much off the line from what was forecasted. This told us a couple of things. First of all, the numbers of smolt that we forecasted coming out of the system were clearly in error. That would have to have over 100 percent survival from fall fry or from smolt to adults for those numbers to be correct. We counted approximately 2.5 million smolt out, and we got slightly over that number of adults back. So, it means rather than an absolute count, it may be an index, or frankly it may not be measuring anything. We've created a lot of uncertainty in the smolt program.

This is, however, what would have happened if we based it totally on fall fry, and totally ignored the smolt data. There were a lot more fry than actually came back. The forecasted return of age 1.3 was certainly above our line, but not nearly as many that we would have said we would have normal fall fry densities, just based on a total fall fry model.

Probably more indicative is the comparison amongst the major sockeye systems in the Inlet, this is the Kasiloff River, this is the Susitna River, and here is Kenai. The Y axis is in the return for spawner. The last two years, which are very heavy escapement years, this is '87, '88 and '89, all of which have high numbers, of the two lowest in the time series, we have the Kenai River. So per spawner put in, they didn't do real well, which is a good indicative in our data that we have in terms of low weights, low fat content and relatively low survival. However, these were much better than we forecast. We felt our smolt data would indicate these would have been about a third. Consequently these other systems, by comparison, have done fairly well during the same time period. This is the relationship between the '94 to what '95 forecast is, and it's created some of the difficulty. The document that was produced, which produces the forecast, gave a forecast range between 1.3 million and 11.9 million. That gives you some idea of the degree of uncertainty -- this is for catch in Cook Inlet. There is a lot of reasons for that, but the scatter in that data is probably one of the reasons. If you'd pulled that ratio down to here, for the 1-3 animals, you can have a data point here which is essentially that we won't make escapement rules in the Kenai River. Another way of looking at the thing, if we were -- if we took the other data and looked very optimistically at this, we could have a huge surplus returning. We have discontinued using the smolt data this past year, and whether that was a mistake or not I guess we'll find out this coming year. This is a problem with forecasting. The smolt data if it was a good index last year, not an absolute term, but an index, would indicate we would get about one-seventh of last year's return, which would be a number less than 500,000 returning to Kenai to slightly below its escape goal, with no commercial fisheries. If we took an absolute number, which it seemed to count real well the two years before this past year, we get a very dismal number, I mean less than 100,000-150,000. Very few of us believe that number is at all reliable, but we do believe that there is a lot of uncertainty with what's going to happen this coming year, and although we may have a surplus fishery, we could easily have a very disastrous one.

I'll just sum this up real quick. First of all, there's going to be other people around here, like I said, Lisa Seeb will be able to describe the genetic program and stock separation activities with much more detail. I completely haven't got any of the sonar work that was done in the Inlet, which is essentially a technique to assess in-season fishery return strengths to assist the managers in making openings and closures. Mark Willette, who just spoke, has been the principal investigator of the sockeye program on Coghill, and although I probably did some disservice in describing it, he will be around and he'll be able to provide much more detail on the current activities and the status of that component. The other component, my principal investigator -- co-principal investigator, Kent Tarbucks (ph) can't be here this week. I'll try to fill in, but I also -- Linda Branning (ph) is in the audience, and she probably could help answer a lot of the questions that I can't. Anyway, we'll be around, and thank you.

DR. SPIES: Thank you, Dana. The next talk is on marine mammals and is to be given by Kathy Frost from the Alaska Department of Fish & Game, Fairbanks.

MS. KATHY FROST: Well, I learned one thing, if you volunteer or get volunteered to give the first summary for your group, the first time one of these workshops every happened, you get to do it forever more, because everyone says, oh, you have all the information, you've got all that stuff from last year, and you just begin. I'm listed as a presenter today, I'm going to talk about sea otters, killer whales and harbor seals. Work on sea otters that we'll be discussing was conducted primarily by the U.S. Fish & Wildlife Service, but the investigators now work for National Biological Service, so if I flip-flop the agency that they work for, it's because it's the same people. Killer whale work has been done by Marilyn Dahlheim and her helps was at National Marine Lab, and Craig Matkin and his associates with the North Gulf Oceanic Society, and harbor seal work by Fish & Game in combination with help from National Marine Mammal Lab and NOAA.

The damage assessment studies conducted following the spill indicated that three species of marine mammals were damaged by the spill, and as I said they were sea otters, killer whales, and harbor seals. Other species, just as Dall porpoises may have been damaged, but historical data just weren't adequate at that time for this -- for any kind of a meaningful study, to determine whether, in fact damage had occurred. Since completion of those damage assessment studies to document injury, studies have now focused on monitoring those injured species to determine if they've recovered, and if not for those that weren't recovering, designing restoration activities and/or studies to provide information about these species that could be later used for ultimate recovery. I'm going to try to give you a brief summary of the damage to both species, the most updated information on their status, and then where we hope to go from here.

Sea otters were one of the most visibly damaged of these marine mammal species or, in fact, any of the species. Mortality began very early, almost as soon as the otters contacted the oil, and pretty much continued as long as surface oil was present. As in all species, it was a lot easier to document, the mortality itself, than the mechanism that actually caused it. In total, Fish & Wildlife Service biologists estimate that up to 4,000 otters were killed as a result of the spill. Monitoring surveys conducted in 1993 and again in 1994, although not with Trustee Council funding, have failed to indicate recovery by sea otters, particularly in the area hit hardest by the spill. (Aside comments regarding slides omitted) This slide shows the area for the data presented in the next slide. This is an area that was very heavily impacted by the oil following the spill. Here is the survey data for that area, and basically you can see, although all of these data were collected using the same methodology, they are useful for general comparison, and what you see is the numbers before the EVOS were significantly higher than those since 1992 by a factor of almost two, and if anything, this sort of methodology represented in '73 and '84-'85 tends to underestimate the numbers that were probably encountered by those surveys.

This slide presents data on carcasses found in beach walks and presents data also from pre-spill and post-spill, and what you see here is prior to the Exxon Valdez, 1976 to 1984, most of the mortality in sea otters, or most of the beach cast carcasses were either very young animals or animals older than eight years old; that is, non-prime age animals. You look at the numbers in pink, the Exxon Valdez spill year and the year following it, the pictures changes and most of the mortality, over 40 percent occurred in what we call prime age adults, of these two to eight year old animals, the reproductive age. Since 1991, for these 1992, 1993 and 1994 data, the situation appears to have returned to pre-spill conditions. Investigators asked me to caution that the area searched in '89, '90 and '91 was a much bigger area, the '92 to '94 data represents only Green Island, so it's a much smaller area, but we appear to have a return to normal state.

So, where does the otter studies go from here? One piece of information, the carcass recoveries, suggest that things have returned to normal. The other accounts from Naked Island and Knight Island suggest that there has been no recovery at all. During 1995, a proposed interdisciplinary nearshore vertebrate predator study, which includes sea otters as one of the focal predator species, will focus on two area, that northern Knight Island/Naked Island area were sea otter counts remain low, and northwest Montague Island where residual chronic effects are thought to be minimal. You heard a lot more about that study from Leslie Holland-Bartels earlier. Killer whales were also thought to be injured following the spill. Soon after the spill, counts of resident AB pod indicated that seven animals were missing. One year later, another six animals were missing, and by 1990, 13 of 36 members of AB pod were missing and haven't been sighted since then. During 1989 and 1990 -- here these are the numbers representing the animals in those pods and you will see the drop here in the middle from 36 down to 28, with an additional drop in 1990. 1991 and 1992 indicate that increment sighting increased from calves that were born. During 1989 and '90 no calves were born into AB pod. Calf production in this group of animals and in killer whales in general is normally variable, but to have two back-to-back years with no calves born is not considered usual. In 1991 to 1993, reproduction did occur. This just gives you an estimate of the mortality rate, showing that '89 and '90 stick out as being very unusual that six animals out of 36 and then seven the next year wasn't within the realm of normal variability.

Now, no Trustee Council work was actually funded in 1994, but people working with Craig Matkin and his partners working with Northwest Oceanic did spend time in the Sound and made observations, and they wanted to point out that although data had been very encouraging through 1993, there were apparently an additional five animals missing last summer during the 1994 season, and that no calves appeared to have been produced last year. They believe that some of these mortalities may be spill related, one was a calf that had lost its mother following the spill, and another was an adult male whose fin collapsed that summer, about the time of the spill. This pod, which had been observed in earlier years, 1989 through 1991, is traveling in a fractured -- in fractured groups and behaving -- or aggregating -- in a less than normal manner, was again traveling in this fragmented manner last summer. Although no official monitoring was occurring this past summer, it will take place in 1995, and investigators hope to confirm -- well, they hope not to confirm that these five animals are missing, but they'll determine one way or the other whether they're really missing or they were just simply not observed because less intensive effort last year.

The other group for which there have been regular observations is a group called AT pod. It's called a transient pod. It spends less time in Prince William Sound, and because of the different social structure, it's a lot harder to keep tract of whether members of that pod are really missing or not. However, following this spill, a number of individual were noted to be missing in observations in the last few years, including last summer, indicate that those individuals appear to still be missing. Investigators are beginning to wonder if harbor seals may be playing some sort of a lull in the dynamics of these transient pods. Although killer whales are thought to be recovering, at least until we had this recent 1994 data, studies will continue to monitor whales in Prince William Sound. In addition, investigators plan to take biopsies, to collect small pieces of skin and blubber, and this tissue will be analyzed to investigate the genetic relationship of transient and resident animals. Try to figure out if these two groups of animals are indeed genetically distinct. Also, these biopsies will be used to do fatty acid and stable isotope analyses in an effort to evaluate the diet of these transient and resident killer whales, and perhaps to detect differences between the two different types of whales. In the past, it's pretty much been thought that resident animals are fish eaters and transient animals are more mammal eaters, but it's based on observational data, and we're hoping that some of these technique -- analytical techniques like fatty acids will be able to give us a more detailed picture. Questions of primary interests are to what extent killer whales feed on and may be affecting the recovery of other injured species, such as harbor seals, herring or even pink salmon, and the reverse of that, how are the declines in these prey species, harbor seals -- probably in the forefront -- may be affecting the killer whales themselves.

(Aside comments regarding slides omitted.)

Harbor seals were also injured by the spill. Unlike sea otters, where carcasses were highly visible and recovered in large amounts, very few harbor seals carcasses were actually recovered. In lieu of these carcass counts, damage was assessed through a combination of necropsies of oiled seals and aerial surveys to obtain counts of seals in oiled and unoiled areas for comparison to historical counts, and we were lucky that we in fact did have some (indiscernible) historical counts in the area affected by the spill as well as control areas. This gives you an idea what harbor seals look like from the air. You don't want to get up too early, or you don't want to go to bed too late the night before if you have to count these all day. Here's a slide showing counts from the -- the first trend counts in 1984 through the most recent in 1994, and probably the bottom line here is that since 1984, harbor seal counts in the Sound are down over 60 percent. Since the year before the spill, they are down over 30 percent, and since 1989, they are down 16 percent. Counts in the spill area itself are about the same now as they were in 1989, so this continued pattern of decline has actually incurred at the unoiled sites. Here's a graphic presentation of that same data, with a couple of other additional things on it. The trend count route is shown in blue in the middle, and for those of you that can see, the red through it is a regression line, and that is what that actually tells you, that at least through 1993 there was no significant trend in those data. Now, with addition of the decline in '94 that may become significant. The line in corner, people always say how many seals are there really in Prince William Sound, how does that trend count relate to everything else. An additional 26 sites have been counted by other biologists and those added together with the trend count sites come to about 24 to 2600, so the trend count route itself that we're using to evaluate trend represents something less than half of the accountable animals in the Sound. And, again, for perspective, the harvest is shown in yellow at the bottom, harbor seals are as extremely poor subsistence resource in the Sound. There is a significant harvest. Yellow line shows you that harvest decline considerably in the years -- spill year and in the subsequent one or two years, and has now began to increase gradually. One of the things that we're going to be doing as part of this study in 1995 is actually trying to model the effect of factors, such as the subsistence harvest, predation by killer whales or other human impacts on the population dynamics. Trend count surveys have also been conducted during the pupping since 1989. There were no pre-spill pupping data, and so many of our comparisons have had to be based on information we've collected since then. For the five year period since 1989, the number of pups and adults is down about 20 percent, similar to what we see in the molted counts. Again, the oiled area is about the same, it's effectively identical to what it was in 1989, but this decline seems to be occurring outside the oiled area.

One of the problems with all of this count data, is how the heck we interpret it. Whether we're talking about sea otters, killer whales or harbor seals, we know that we aren't counting all of the animals every time we go out there. Some of the animals are under water or they are not in the haul-outs when we count. This can be affected by the weather, the time of day, the tide, those are the obvious things, and then sometimes it can be affected by things we just can't imagine, and so one of the things we're doing as the investigators is trying to figure out the power of our surveys to detect changes: how many surveys must we fly, how many replicates, what must we fly, how many years involvements we fly, and those are the kinds of answers that funding agencies would like to have as well as resource management would like to have. I use this just as an example of the kind of power analyses we're going to be conducting on this data. This was run as if we flew surveys for five years, and basically it tells you that if you have a big change, (aside comments) 10 percent a year, it's very easy to determine to a very highly significant level, you know, that you've got that change, but if you're down here where the harbor seal population is changing at about 5 percent per year, it takes a lot of replicates in a lot of years for a fairly -- well, this is a 70 percent chance of being right, if you fly 20 replicates per year, which is more than the weather window allows. So the bottom line probably is, it's worthwhile flying those surveys at a low level for a long time, rather than skipping too many years in a row, because then otherwise it will take you until 3010 to know what's going on. Although this count data gives us some indication of the trend, as Ted Cooney pointed out earlier, it's only that. It's just a measuring stick. It doesn't really tell you anything about the whys and what-fors of that trend. To do this the harbor seal investigators are going to undertake a variety of other studies to address many of the same questions that Alan Springer talked about this morning for the Bering Sea, and those how, you know, is this decline and lack of recovery due to do food, is it because of disease, it is mortality caused by predators, is it mortality caused by humans? Starting in 1992, we began catching seals and instrumenting them with satellite-linked time-depth recorders. These radios gave us information on the movements of the animal and also their diving behavior. In addition to simply tagging the seals, they've been weighed, measured, blood has been taken, disease swabs collected, small pieces of skin taken for genetic studies, and then more recently in 1994 blubber biopsies taken for fatty acid analyses, and whiskers for stable isotope analyses. And, one of the things the tagging studies has shown us is that the harbor seals are very loyal to their haul-out. In fact, the average marbled murrelet, moves a lot farther to feed than the average harbor seal does. I tell people that harbor seals have little strings tied to their hind flippers that connect to their haul-out, and those strings are about five miles long. It's absolutely remarkable. We've had tags on animals for over ten months now, and from -- our data aren't as good when the animals are at sea, but from what we can tell, some of those animals stay within six, seven, eight miles of the haul-out around -- the complete animal cycle.

This just gives you an idea of what a seal looks like when it's carrying a tag on its back, and what it looks like when it goes to sea. Harbor seals have a nice habit of rolling their back above the water, and so they give a frequent signal.

In addition to those tagging studies which I've talked about at some of these other workshops, we've tried a couple of new things this year. Last year, I came to this -- we all went to the workshop in April, and fatty acids was sort of the big buzz word, and I was one of the people responsible for talking that buzz word around and talking it up, and I went home and went, oh, shoot, is this really going to produce anything, or, you know, did I start something that's going to lead us down a blind alley. And, I think it is safe to say right now, myself and all of the other people involved in this fatty acid analysis are really wired about it, and we're excited. These data -- I got about 48 hours ago, so they're not very thoroughly analyzed, but basically what this slide -- there is about 70 different fatty acids that you analyze for, when you run a blubber sample through, and there are some very complicated statistical procedures, a lot like the genetic work that you saw Dana present, where you sort things out into trees and see what's grouped with what. But, as a preliminary run, we took two fatty acids and sorted the seals into groups, and what we found is that all seals are not eating the same thing, and in fact there is a lot of individual variability, but it doesn't appear to just be random, inexplicable variability. Here, these two pies here represent Chalmers (ph), Stockneal (ph) Harbor, which were very close together in the northwest end of Montague, and this pie is Channel Island, which is a straight line distance of probably only eight miles away, and what you see is whatever this is that these harbor seals are eating is present in 75 percent of the harbor seals of Chalmers and Stockneal and none of those that are only six miles away. This group here in blue and green, we think is some sort of a herbivorous fish, capelin or something like that, and we think that the red group is a pisciverous, be it a fish or squid. We've done approximately 40 harbor seal samples and none of the prey species yet, so that's the next thing to come. The other technique that we've been working with is stable isotope analysis. I think some of you that came to the sea workshop might have seen this slide. Amy Hirons and Don Shell at the university have been using seal whiskers, and basically taking samples at half centimeter increments and looking at this stable isotope composition, and what we find is two really quite distinct patterns, and believe it or not I didn't have to doctor these slides up and pick only the pretty one. They really seem to sort out into these two patterns. As near as we can tell, the one on the left occurs mostly in sub-adult animals, less commonly in adult males. To date, based on a fairly small sample size 15 to 20, we've never seen this in adult female. The pattern on the right occurs in adult females, and in general in adults. What this suggests to us right now is that whatever the juveniles are doing, or eating, their doing the same thing around the year. This looks like the adult females are either moving to a new area to feed at some time in the year and/or they're utilizing an entirely different prey species. Usually when you get a nice pattern like this and a nice set of fatty acid data, you don't put them together because you're afraid what will happen and all your conclusions will go in the wastebasket. So far, and again, very preliminary analysis, it looks like the animals on this right-hand pattern with the stable isotope work -- also fall out into a very different group than the fatty acid work, and I'm sitting here now harassing Don Shell's graduate student almost daily, saying Amy give me more, give me more, give me more, because I want to match up more fatty acid data with more stable isotope data. But, I think, indeed, this may prove to be a useful technique.

And now, where are we going to go from here? There is a group of studies working together as part of a marine mammal ecosystem study, as it were -- killer whale investigators, harbor seals, we're interfacing with forage fish people, and basically plan to try to address in the coming year questions on, is it food through additional work on stable isotope study analysis, incorporating data from the forage fish study, and then each of us have separate side questions we're addressing to pursue genetic identity of stocks, disease questions and that sort of thing. And that's it.

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DR. SPIES: The main talk this afternoon is on subsistence issues, and we will begin with Rita Miraglia, Alaska Department of Fish & Game.

MS. RITA MIRAGLIA: The emphasis of this presentation is slightly different from the others you've heard today. The Memorandum of Agreement, which resulted in settlement of the state and federal governments' claims against Exxon, states restoration funds must be used for restoring, replacing, enhancing or acquiring the equivalent of natural resources injured as a result of the oil spill and the reduced or lost services provided by such resources.

That subsistence is the only service represented as a separate category in this session, speaks to its importance to the communities in the path of the spill. State and federal laws define subsistence as the customary and traditional uses of wild resources for food, clothing, fuel, transportation, construction, art, crafts, sharing and customary trade. Many communities in the spill area depend upon mixed, subsistence-cash economies, where subsistence production is a major economic sector. Within the oil spill area, subsistence harvests are relatively high in diversity. Major resources include seals, sea lions, moose, deer, goats, waterfowl, salmon and other finfish, invertebrates, and plants and berries. Virtually everyone participates in the harvesting and processing of wild resources, especially in the smaller communities. Subsistence harvests make up a large portion of the diet of many families. In a sense, all of the projects which help restore or enhance the natural resources used by subsistence harvesters are subsistence restoration projects. I will be discussing principally three projects funded by the Trustee Council in 1994 specifically targeted at restoring subsistence uses, and a fourth project conducted through another funding source. Subsistence in the oil spill impact area is recovering, but is not yet fully recovered. Much of the information we have on the status of subsistence use in the wake of the oil spill comes from a project jointly funded by the U.S. Minerals Management Service and the Alaska Department of Fish and Game. The primary purpose of the research was to investigate the long-term social and cultural consequences of the development of the resources of Alaska's outer continental shelf, especially as these affect the subsistence uses of fish and wildlife. Investigation of the consequences of the Exxon Valdez oil spill was a major focus of the research. Most data were collected through voluntary face-to-face interviews using standardized survey forms. The surveys showed that annual per capita subsistence harvests declined dramatically, ranging from a nine percent to a 77 percent decline as compared to pre-spill averages, in ten of the communities in the path of the spill during the first year after the event. Declines also occurred in the breadth of resources used and participation in subsistence activities. In subsequent years, levels of subsistence harvests, ranges of uses, harvest effort, and the sharing of resources gradually increased in all of the spill area communities. Generally, subsistence uses rebounded first in communities of the Alaska Peninsula, Kodiak Island and the lower Kenai Peninsula, but lagged behind a year or more in the Prince William Sound villages. This graph shows per capita harvests of wild resources for home use, in pounds useable weight for Chenega Bay. The white bars represent the pre-spill years, the black bar is the year of the oil spill, and the grey bars represent the post-spill years. You can see that harvest declined dramatically in Chenega Bay in 1989, and remained depressed in 1990. In 1991 the subsistence harvest doubled compared to the previous year, and increased again in 1992. Harvests in Tatitlek showed a similar pattern. The 1991 estimate for Chenega is about the same as pre-spill averages, and that for

1992 exceed pre-spill estimates, but it is likely that these early estimates, which pertain to the first two years following the reestablishment of the Community of Chenega Bay, underestimate harvest levels immediately preceding the spill. There has also been a significant change in the composition of subsistence harvest in Chenega Bay, with increased fish takes and a much reduced marine mammal harvest, as well as harvesting outside the village's traditional parks areas. In 1993, harvests fell in both Chenega Bay and Tatitlek. This decline seems to be a reflection of the reported scarcity of certain resources in Prince William Sound. In 1989, a majority of households with spill-caused reductions in resource uses cited fear of oil contamination as the reason for the decline. In 1993, the vast majority of households indicating continued spill related impacts to their subsistence uses, cited reduced resource populations as the cause of the decline. However, contamination concerns about specific resources do persist among many households, especially in Chenega Bay, Tatitlek, Port Graham and Nanwalek. This illustrates an important finding that many households in the spill area returned to using subsistence foods, despite lingering contamination fears. There are several factors preventing the complete recovery of subsistence harvests and uses to pre-spill levels. Many subsistence users remain concerned over the possible long-term health effects of using resources contaminated by oil. There has been a loss of confidence on the part of subsistence hunters and fishermen in their own abilities to determine if their traditional foods are safe to eat. Residents of a number of impacted communities have expressed the fear that animals which came into contact with the oil have been altered in some way that cannot be seen or detected in laboratory tests. In addition, people have reported the scarcity of some resources, and have observed abnormalities in resource species. There is a cultural proscription among Alutiiq peoples against the harvesting or eating of animals which appear sick or abnormal. A view persisted in the Prince William Sound communities, and to a lesser extent in the other communities in the oil spill impact area, that the natural environment has changed in ways that still pose a potential threat to their health and their way of life. This has profound effects on the outlook for the future that people express in a number of communities, and remains an important long-term impact of the spill. In 1994, for the second year, the Trustee Council funded a subsistence food safety testing project. This project continued work begun in 1989 by the Oil Spill Health Task Force. Samples of subsistence resources were collected from harvest areas used by the impacted communities, and tested for hydrocarbon contamination, under the auspices of the Task Force in 1989, 1990 and 1991. The health advice communicated by the Task Force was that most resources tested, including finfish, deer and ducks, had very low to background levels of hydrocarbons and are safe to eat. Marine mammals were also found to be safe to ear, although the blubber of heavily oiled seals were found to have elevated levels of hydrocarbons. These heavily oiled seals were only found in Prince William Sound and only in 1989. Elevated levels of hydrocarbons were also found in some marine invertebrates collected from oiled beaches. Consequently, the Task Force advised subsistence users not to harvest marine invertebrates from obviously contaminated beaches. The Task Force also recommended long-term monitoring of such beaches. The overall goal of the 1994 project was to work with subsistence users to restore confidence in their ability to determine the safety of resources. Specific goals were to: answer lingering questions about oil contamination and subsistence food safety; monitor selected shellfish harvest areas; involve subsistence users in every phase of the project, in hopes of increasing their understanding of and trust in the results and health advice; communicate test results and health advice to residents of communities impacted by the oil spill; and to integrate information from other restoration projects with that already developed through the Task Force studies. The methods used to work towards these goals included community meetings, the collection and testing of samples of subsistence resources, taking community representatives on a tour of the laboratory where the tests are conducted, and issuing informational newsletters to report results back to the communities. In 1994, hydrocarbon tests were conducted on 124 composite samples of edible tissue from shellfish. The tests on the shellfish showed levels of aeromatic contaminants so low as to be within the margin of error for the tests, all below 15 parts per billion. The bile of eight rockfish and six sockeye salmon were screened for the presence of metabolites of fluorescent aeromatic contaminants. The levels of contaminants in the fish bile was so low, one would not expect to find elevated concentrations of hydrocarbons in the edible flesh of the fish. Samples were also taken of the liver, blubber and bile of five seals collected by hunters from Tatitlek for subsistence use, and of the skin, liver and bile of 21 ducks harvested by Chenega Bay subsistence hunters. The tests of the 1994 seal and duck samples are not yet complete. However, in tests on blubber from seals harvested from the Chenega Bay area in 1993, no oil contamination was found and the concentrations of fluorescent aeromatic contaminants in the bile of the five harbor seals tested in 1993 was also found to be very low. There were two tours of the National Marine Fisheries Service laboratory in Seattle, where the tests are conducted. In 1993, a group of representatives from Chenega Bay, Tatitlek, Port Graham, Nanwalek and Old Harbor attended. A second tour was held in 1994, and was attended by representatives from Kodiak City, Akhiok, Larsen Bay, Karluk, Ouzinkie and Port Lions. The tour groups were able to meet the laboratory staff, see samples of subsistence foods being tested, and had the opportunity to ask questions. A number of the community representatives indicated they were coming away with a better sense of how the tests were done, and now had more trust that there was a sincere attempt on the part of the laboratory to get accurate results. 1994 was the last year for hydrocarbon testing under this project. The emphasis will now shift towards helping people understand the abnormalities they are seeing. This will be done by continuing and expanding the dialog between subsistence users and scientists working with the damaged resources. In 1995 we will set up a system where subsistence harvesters can send samples of abnormal resources to be examined by biologists or pathologists. The scientists findings will then be reported back to the communities. As Kathy Frost just told you, the populations of harbor seals and sea otters in Prince William Sound and adjacent waters were injured as a result of the spill. Many subsistence hunters within the spill area, have voluntarily reduced their take of these species in an effort to help their recovery. However, there was no mechanism in place to evaluate the effectiveness of these efforts. In 1994, the Trustee Council provided funding for a project with the unwieldy title "Harbor Seals and Sea Otter Cooperative Harvest Assistance." The goals of the two year project are: to compile the available information on harbor seal and sea otter population status and subsistence harvests; gather additional data as needed; analyze and interpret the data, in cooperation with the appropriate agencies and Native groups; and cooperatively produce a set of recommendations regarding harbor seal and sea otter harvesting to guide subsistence users who want to voluntarily change their harvesting practices to help these two species recover. Some data were available on marine mammal harvests in the spill area. The Division of Subsistence, the Alaska Department of Fish & Game, has collected information on the numbers of harbor seals and sea otters harvested by subsistence users living in several communities in the spill region for both pre- and post-spill years. In 1993, the Division of Subsistence, in cooperation with the National Marine Fisheries Service and Ruralcap, also undertook a project to collect more detailed information on the timing and composition of subsistence harvests of harbor seals and sea lions, including those animals struck and lost. The U.S. Fish and Wildlife Service runs a sea otter tagging program, which gathers information on sea otter harvests, including the location where animals are taken. There was also some information available on harbor seal and sea otter populations in the region. The Division of Wildlife Conservation with the Alaska Department of Fish & Game, working with the National Marine Fisheries Service has conducted a count of harbor seals in both the oiled and unoiled areas of Prince William Sound, along with other research aimed at assessing the health of the harbor seals. The U.S. Fish & Wildlife Service has continued to monitor the recovery of sea otters in oiled areas by determining their abundance, distribution and mortality. In the first year of the current project, the Alaska Sea Otter Commission, under a cooperative agreement with the Division of Subsistence, collected and analyzed the available information on seal and sea otter populations and harvests, and produced a report which is currently under review. A principal finding of the report is that subsistence harvests did not cause the decline of the harbor seal population. However, whether the continued subsistence harvest is retarding the recovery of harbor seals is still open to question. Staff of the Division of Subsistence interviewed subsistence hunters to collect additional information about the location of seal harvests.

Researchers also interviewed elders and other knowledgeable individuals to record their observations regarding changes in the seal and sea otters. Two workshops have been conducted as part of this project. The first brought together scientists, agency staff, and representatives of native organizations. The second workshop included subsistence hunters from Prince William Sound and the lower Kenai Peninsula. Discussion centered around the review of available biological information about sea otters and harbor seals. There was a consensus among the group that a major goal of the second year of the project should be to figure out ways to involve hunters as full partners in subsistence restoration. There is much that scientists need to hear from subsistence users and the mechanism by which they do that still needs to be worked out. The community involvement and use of traditional Knowledge project funded by the Trustee Council in 1995 will assist in this effort. In 1994, the Trustee Council also funded a project to design a coordinated approach to subsistence resource restoration. This project was a joint effort by the Alaska Department of Fish & Game, the Alaska Department of Community and Regional Affairs, the U.S. Department of the Interior, and the U.S. Forest Service, with assistance from the Alaska Department of Law, Trustee Council staff, and representatives of the spill-area communities. Meetings were held in the spill-area communities to solicit ideas and priorities for restoration of subsistence resources and lost or reduced subsistence uses. Following the meetings, staff worked with the communities to develop project proposals. After evaluation of the proposals, recommendations were presented to the Trustee Council. A first round of meetings and project proposals have been completed for Prince William Sound and the lower Kenai Peninsula. As part of this process the Trustee Council funded four projects: a chinook salmon remote release project at Chenega Bay; a coho salmon remote release project at Tatitlek; a project to reseed clam beds in the harvest areas of Port Graham, Nanwalek and Tatitlek; and an elders/youth conference for all of the communities in the spill impact area to be held in Anchorage this coming fall. This process has been a learning experience for both the agency staff and the community representatives involved in it. The restoration plan adopted the following recovery objective for subsistence: "Subsistence will have recovered when injured resources used for subsistence are healthy and productive and exist at pre-spill levels, and when people are confident that the resources are safe to eat. One indication of that recovery has occurred is when the cultural values provided by gathering, preparing and sharing food are reintegrated into community life." The Trustee Council also adopted a policy that projects designed to restore an injured service "must have a sufficient relationship to an injured resource." To qualify for funding from the civil settlement, all projects must directly restore a natural resource damaged by the spill. This meant that a comprehensive approach to all aspects of subsistence restoration was not possible under the civil settlement funding alone. For example, in 1994, a project proposed to restore subsistence uses through the development of a "spirit camp" designed to transmit traditional skills and knowledge disrupted by the spill, could not receive funding from the civil settlement, because it would not directly restore injured natural resources, but rather solely addressed the restoration of human uses and cultural values. Fortunately, proposals developed through this planning process which were not funded under the Civil Settlement could be considered for funding through grants from a five million dollar appropriation of Exxon Valdez criminal settlement funds by the Alaska Legislature. The legislature authorized the Department of Community and Regional Affairs to award grants to unincorporated rural communities in the oil spill area in order to restore, replace or enhance subsistence resources or services damaged or lost as a result of the spill. The legislation required that selection of grant recipients be made after consultation with the state members of the Trustee Council. In 1994, six projects were funded out of the five million dollars available through the DCRA grant program: a spirit camp for all of the Chugach region communities; a fish and game processing facility for Tatitlek; a grant to support Chenega Bay residents harvesting in unoiled areas; a sockeye salmon enhancement project on the English Bay River; and oyster mariculture development projects for both Chenega Bay and Tatitlek. While these projects do not necessarily directly restore an injured natural resource, they are designed to help natural resource restoration by developing alternative resources, and by making the use of existing resources more efficient, thus relieving harvest pressure on recovering natural resources. Meetings have been held with representatives of communities on the Alaska Peninsula and Kodiak Island, and another round of meetings is planned for Prince William Sound and lower Kenai Peninsula communities. Ideas presented at these meetings will be developed into project proposals for consideration for the 1996 work plan. Thank you.

DR. SPIES: Thank you, Rita. (Aside comments omitted).

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P R O C E E D I N G S

(Thursday, January 19, 1995)

MS. JUDY BITTNER: ... summarize the findings for the 1994 archaeology projects, as well as give you the summary of what the archaeology session accomplished yesterday, and we will forego our summary tomorrow because we do not plan to be meeting again. So, we have essentially completed our session for archaeology and tried to put it all together from yesterday. Some of the work is still yet to be written up and done, but we did get through some of the main points.

In 1994, the archaeological project was 94007, and it was project that has three components to it. One component is monitoring, another one is data collection, and the third component of that project is gathering information for community site protection plans. The Office of History and Archaeology in the Department of Natural Resources is the coordinating agency for this project, and the other agencies working with this project are Parks Service, Fish & Wildlife Service, and OHA. The project focuses on the injury from vandalism and from increased knowledge of site locations gained during the clean-up phase of the oil spill. These are sites are vulnerable to unauthorized, artifact collectors. The monitoring phase of a component of this project had most of the action from the State of Action in actively monitoring seven sites in outer Kenai coast of the Shuyak Island. The Department of Interior archaeologists from the National Parks Service and the Fish & Wildlife Service did not directly monitor sites in 1994, although they did track the condition of the sites that were of their interest through other agency personnel who were travelling nearby those sites. The National Park Service checked the general condition of the McArthur Park Site and a site on the Katmai coast through observations of these personnel. Located on a busy route for pleasure boaters, the McArthur Pass site is very vulnerable to unauthorized artifact collection. The exposed intertidal cultural site was thoroughly documented in earlier field seasons and the National Park Service managers felt a year could pass without intense monitoring. The Gull Cove site on the Katmai coast was also of concern and was tracked through this kind of monitoring, and what they are looking for other observations is to see if there is any major obvious damage there, and they could call the archaeologists in, and they did not discover -- they were not disturbed in a major way during 1994.

The Fish & Wildlife's Service focuses on a single Afognak Island site, Kodiak 171, which was a previously vandalized site. They had non-archaeological employees observing and tracking that site for them, and the condition of that site did not change or was not impacted this year in an archaeological way. In 1995, that site will be revisited by archaeologists for a two-year assessment as a 1995 project.

The State of Alaska returned to three sites around Nuka Island , which were mapped and tested in 1991 and 1993, to assess continuation of erosion and possible vandalism. The Seldovia 215 site continues to erode. Monitoring in 1994 revealed extensive additional erosion of the intertidal peat which contains cultural remains. More cultural deposits have disappeared over the last several years, and the filled in trenches from 1991 investigations have once again become exposed. There is no evidence for oiling or vandalism that could be seen at the site. Another site in the vicinity, Seldovia 119, is especially vulnerable to vandalism because it is fronted by one of the few good beach landing sites in the area. That site has suffered from deposition of gravel by winter storms but does not appear to have been recently vandalized. However, the house pits at that site contain modern debris where campers have left gas tins, stove parts and other modern garbage. So, its' being used.

Restoration efforts on Afognak 81, a site on Shuyak Island seems to have been successful as the vandal hole refilled in 1993 is re-vegetating without further disturbance. Continuing damage at the Perevalnie Passage site was documented in 1994. A human burial, partially exposed in 1993, has disappeared in the intervening time and erosion of exposed site sediments continues, and oil is still present at that site. A plan to salvage data before it is lost to erosion and vandals will be needed for very important scientific knowledge about the transition from the Kachemak culture to the early Koniag culture will be lost.

The second aspect for restoration of archaeological resources in the oil spill area was intensive testing and evaluation of damaged archaeological sites by the Forest Service. The Forest Service tested a site on Eleanor Island and the at the Louis Bay

Lamp site (indiscernible). Both sites were identified as impacted through early agency examinations and Exxon reports. Both sites were oiled and one was vandalized before adequate protection procedures could be implemented. The Forest Service testing revealed presence of significant faunal remains in the 1800 year old site which makes it a particularly important site for the study of aboriginal subsistence practices. A sediment sample collected at that site documents continued presence of considerable petroleum hydrocarbons at a depth of 30 cm below the beach surface.

Both historic and prehistoric remains at the Louis Bay Lamp site (indiscernible). The prehistoric remains include information about aboriginal structures, information which is rarely recovered from Prince William Sound site. Analysis of 1994 data is still in process with additional testing and restoration proposed at the Louis Bay Lamp site during 1995.

A third objective of the archaeological site restoration addressed during 1994 was the compilation of information about the need for site protection, with special attention given to adequate curation of collections in the oil spill area. Begun after receipt of numerous requests for support of museums and cultural centers by the Trustees, the Office of History and Archaeology collected information from local communities about what types of facilities exist in the area for housing archaeological collections and what the local people saw as needed. Interviews with local, knowledgeable individuals revealed that spill area museums outside of the Kodiak area will not be able to properly house area collections either because of lack of existing staff or more pervasively lack of appropriate facilities. Most local communities are concerned about impact of the oil spill on their traditional lifestyles, of which artifact collections are only one material representation. Many people interviewed see the need for facilities which house artifact collections and a place to have traditional activities which reinforce traditional value. Most envision multi-use cultural centers which would function well beyond simple housing of collections.

The goal of the archaeological protection planning effort is to recommend to the Trustees measures which might reasonably support in restoring damage to the cultural heritage of the area. The archaeological sites of the area and collections from those sites are the material representation of that heritage. Those recommendations are now being finalized.

That concludes our review of the '94 projects, and what we did yesterday was to (indiscernible) through the questions that we are to review as part of the looking forward to what we can accomplish and (indiscernible -- poor tape quality).

This is a village slide showing oil that is continuing at the archaeological which will be relevant.

We got hooked up by teleconference to our peer reviewer, Dr. Bonn (ph) in Washington, D.C., and I do appreciate -- the teleconference was a little bit difficult, but I was glad to have him by telephone than not have him at all. He is in Washington, D.C., on a Smithsonian fellowship and unable to make it here. We did look at as part of the process yesterday reviewed our objectives in the plan. There was some suggestions and clarification for that plan, and one of the areas that there is some confusion is to which archaeological sites the projects can address -- public lands versus private lands -- and that's something that seems to be (indiscernible) for the archaeology. That's something that needs to be clearly stated or more clearly stated in the plan so we can see what this portion of the oil spill settlement in the overall broader picture of the oil spill settlement as a collective can address and where partnerships need to be forged.

The future projects and the discussion centered around three types of restoration projects, monitoring being one of the primary ones. We looked at the direction the agencies were concerned with archaeological digs aiming to the future and continuing to monitor. Their focus had been monitoring for vandalism. As the prospect and the idea -- the prospecting and the knowledge of the sites had been gained during the cleanup phases and when those people might come back. (Indiscernible) was there going be a return and what was that? A site (indiscernible) pattern might be reasonably expected going from the oil spill forward. Our peer reviewer challenged our thinking and told us to broaden our thinking on that and the question that he brought up was in scheduling of the purpose is that we should also be testing for the effects of long-term hydrocarbon contamination in those sites. A large sections there is some contamination and what are those effects on those sites, and he said that also needs to be factored into our testing and the timing that we schedule for that type of testing. He also questioned that we should re-examine which sites are chosen. He said take another look at that and look at some of the criteria as to which sites are chosen and for what reasons, and so we do have for 1995 what we have called in the index site monitoring systems, in which we've had sites on a two year schedule. We didn't think we needed to visit them every year because we have done some very extensive baseline data from the earlier monitoring visits and have mapped out a restoration plan for each one of those sites. Sometimes it was erosion control, sometimes data collection, sometimes it was just documentation, and he said take a look at those, schedule so instead of every one being on a two year schedule, he said each site should be individualized to see if there were some factors that may want us to visit it more frequently or less frequently, because some are more exposed than others. So, he did get us thinking, and we will be going back and taking another look at those sites and what we're doing at those. And I think the Eleanor Island site might be a good candidate for the hydrocarbon study because of the obvious contamination and finding this year at 30 cm down some very obvious contamination in that particular. We were looking at end points and we came up with 10 years, which seemed to be particularly the vandalism (indiscernible -- poor tape quality) ten years would be a time period (indiscernible) at that point. Looking at each particular site, I think that was the time period that we will be working within.

The other major area was the site protection and public education. We use those very comprehensively because the discussion here covered a very wide area, and it showed a lot of interest. We were very lucky to have representatives from a number of Prince William Sound communities at our discussions, and the oil spill community representatives showed a very high interest in projects dealing with cultural facilities, educational programs and materials, and stewardship programs. The stewardship program was one of our early projects that has not been implemented, and I think the discussions were broad in scope, but also talked about partnershipping, maybe combining what the oil spill projects can do, what is eligible with other broader interests in the communities, and how programs can be structured such so that it addresses some of the broader cultural heritage issues and how the oil spill impact of injuries and the damage fit into to their wider needs, particularly in this partnering and what part that they play (indiscernible). In the site protection plan that we are working on for this current year project, I think will lay out some of the alternatives and options that the Trustees can pick and choose from, and it may or may not apply to the different locations as well. In that one, looking for timelines and what can be reasonably expected, it is too soon to tell because the Trustees have to decide what kind of course of action they are going to take, and then once they take that course of action, then one can look at both the commitment in time and the projects.

For data recovery, some sites are facing loss and destruction from vandalism and erosion. These sites that Forest Service are excavating at this time will finished in '95, and there is currently nothing on the books for '96 and beyond. But some of the milestones for the monitoring is to assess what is going on in tracking the sites, and there may be some corrections or restoration strategies that will include documentation and data recovery if sites are being damaged or impacted. That's really part of the solution. So, I think that's the kind of information, how that will be used just for monitoring. Instead of just monitoring to gather information, that information needs to come back and to be able to make decisions as to what to do, are these restored, are they out of danger.

And that concludes the archaeology session. Are there any questions or things I forgot to bring up because it was kind of a rush between yesterday and today. Any other members that were there that -- major points that I left out? Questions? Okay, that's it. (Applause)

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P R O C E E D I N G S

(Friday, January 20, 1995)

DR. ROBERT SPIES: We have a little bit different plan than is on the agenda. The way we will proceed is to have each of the group leaders to provide a summary of the discussions and any conclusions that their group came to. We'll then ask the core reviewers at the front table to comment, open the discussion then to the audience and move through the groups one by one. The order in which we will be taking them will be Kathy Frost to talk about marine mammals, Dave Irons to then follow that with discussions of seabirds, Ted Cooney going third to discuss the SEA Program and the pink and herring aspect of the studies, and then we move on to subsistence with Menace Riedel and Martha Vlasoff; we'll move then to recreation with Veronica Gilbert; sixth, Jim Bodkin to discuss the nearshore (indiscernible) and nearshore ecosystem; Alex Wertheimer to describe fish stock structure studies, followed by Joe Sullivan with enhancement, and that should take us to the end of this particular session.

MS. KATHY FROST: I guess in the few introductory remarks, I would say some of you may be wondering, if you were at last year's church group meeting, why you have me here talking about marine mammal ecosystems or anything that has a title of marine mammals, as opposed to pelagic predators or some sort of a broader ecosystem titling, and what I tell people in my group and other people is that this is a matter of convenience. In effect, there is a forage fish unifying concept in the middle or prey that is used extensively by seabirds and migrating mammals. In our planning group last spring, we talked about, as an integrated group, the questions that affected marine mammals and seabirds in a similar way and how we would go about investigating those questions. As the planning procedure developed and all of us starting working together trying to write DPB's (ph) and work plans and organize our thinking, many of us felt that this was such a big group it was just logistically unmanageable, and so really we did something that made our work easier, and made it easier to compartmentalize and communicate until we have a forage fish-seabird component and a marine mammal-ecosystem component, but there is, I think it's safe to say, no member of either group that does not clearly see forage fish in the middle as the glue connecting these two sides. So, although I don't spend a lot of time talking about the connections with the seabird projects and the interrelating of these two, that's the underlying premise. We're separate only for logistics reasons.

In some ways the marine mammal group has it easy, for a small number of species, kind of the unifying principle. It had two injured species: killer whales and harbor seals. Killer whales were classified was injured and recovering at the moment; harbor seals have been considered injured and non-recovering, and as such have become the sort of focal species. With that in mind then, we have worked our study design around addressing this injured and non-recovered species. I guess one of the things we were asked to do is talk about how long we're going to do this and what our deliverables are. This is the non-ecosystem part of it. One of the things that came out last April was that although ecosystem studies are important, there is some need to do some amount of monitoring just to track what's happening. Monitoring is kind of this monster that can eat large amounts of dollars and large amounts of time or it can be modest component. Just as an example to let people know what we have in mind for harbor seals, for example, we're looking at monitoring in '95, '96, and '97, we actually plan to produce major products in '95, with power analysis and analysis using the last three years of radio-tagging data to look at correction factors for these surveys, and so at the end of basically this year and really significantly before that, the next three months, have an in-depth, methodological examination of these monitoring protocols that will direct us into the future. The intent is a very low level monitoring into the future, something in the order of $25,000 at the high end and conceivably a lot less than that, with a '98 status re-evaluation scheduled to see where we are and should we do something different. Killer whales we will be monitoring in 1995. They haven't been in the field two years, as you heard from the plenary talk, based on non-funded observations incident to other activities in Prince William Sound there may be some other animals missing in Prince William Sound. That's going to cause everybody to rethink things, and at this point it makes it not near as clear what the future monitoring plan will be for killer whales.

For the injured resource which is harbor seals, basically the question is why aren't they recovering? Harbor seals were declining before the spill, and the additional injury that accompanied the spill and in the five ensuing years there has been no apparent recovery and this declining trend is not statistically significant but the decline appears to be continuing in both the oiled and the unoiled areas.

So, how do we go at it? What I won't talk very much about is -- we came up with basically five questions and/or hypotheses. One, that disease was limiting the recovery and/or causing the decline -- and this is to just quickly to show you that that question really has been addressed. We have been looking at diseased samples for the last five years. The analyses are in. Although we can't completely discount it, there is absolutely no reason to think that disease has anything to do with this. Everybody keeps trying and trying and trying to think of something disease related that could be a problem; it just doesn't seem to be the case. Is it people? Again, harbor seals are an important subsistence resource in Prince William Sound. There is a significant annual take of harbor seals. We have on a casual basis and will in '95 do on a much more formal basis begin looking at just the numbers of population dynamics, the magnitude of the harvest and the magnitude of other things that might be affecting killer whale survival. In a nutshell, it is pretty clear that the harvest had nothing to do with the original decline. It was caused for completely other reasons. The big question now is, would some sort of a change in the harvest level possibly moderate this continuing decline. This is something that an effort that they'll be basically some demographic modeling done in 1995, a report accompanying that model, and then the future activities will be working with the people in the communities. I would like to see a community harvest program developed, so that local people are monitoring their own harvest, getting together with people like me, looking at these numbers on the map and trying to mutually decide where we might want to go in the future.

Then, kind of a third but mentionable non-ecosystem hypothesis is, is it reproduction? I think basically we can discount this also. Near as we can tell, the number of pups being born in Prince William Sound proportionately are as high or higher than anywhere in the West Coast, areas with similar pup production are experiencing 15-20 percent growth. Now, there's some underlying questions about is what we see only the healthy component of the population, but we haven't completely written off, but this is not a prime focus of our future investigations.

So what we get is really the focus of this workshop, and these are what we call our two ecosystem hypotheses, and they are really all part of the food question, they are just food of different critters. One is, is it predation that is limiting the recovery? Again, much like the subsistence harvest, I don't think that anyone in the group thinks that killer whale predation caused harbor seals to decline, but it may be possible that predation is keeping a lid on things. Once they are down, killer whales may be basically taking in or in combination with hunting and incidental take, taking this annual (indiscernible).

Again we were asked to look at some time lines and deliverables, and just roughly, '95 which is our upcoming field season, two sort of products expected to the Trustee Council. One is this demographic model looking at the interaction of harbor seal population dynamics, harvest, killer whale predation, incidental mortality and residual effects of the spill. The second is basically analysis of historical information on killer whale feeding and distribution, feeding it into a GIS system, having a visual representation of where concentration areas and/or hot spots for killer whales may be, and looking at those in light of harbor seal distribution. Following '95 and sort of beginning this year, this how many harbor seals do killer whales eat? What we know is some kinds of killer whales eat some number of harbor seals, but that's about as specific at this point as our information is. Starting in '95, through stable isotope work, fatty acid work, this re-analysis of historical information will begin to work on that question. 1996, starting observational work in the field, when and where are killer whales eating, actually trying to look at cray remains around kills, identify species, and then a final report expected in 1998. If future work is suggested at that point, it would be in the form of a redesigned, newly developed project and, although we show here this continuum of '95 through '97, certainly a reevaluation at the end of the first year's field and laboratory work to see if additional work is warranted.

The final of the second question that is ecosystem related is, is food limiting? But we can actually refine this a little more and say, is food limiting the survival of sub-adults? We are very fortunate there was another megaproject like this almost 20 years. I believe some of us in that room were actually part of that program, and I think at the time a lot of people said it's a money tree -- everybody likes money trees. That's certainly what OBSEF (ph) was, and that's a way a lot of people have accused this of being, but it's been a godsend. That 1975-80 OBSEF data has provided in the case of the harbor seal project the only historical data. It not only provided survey data, but Ken Pitcher went out there in the 1970s and collected harbor seals and had the foresight not only to collect them, measure, and weigh them and look at their stomach contents, but put little pieces of them in the freezer. So, what we are hoping to do starting here in 1995 is take those morphometric measures, those little pieces of 1975 harbor seals, in addition to what we've collected in '89 through '94 and do some comparative work to see if we can look at differences in the energetic content of the blubber. Mike Castellini at the University of Alaska is proposing some pretty innovative, high tech approach to this, and -- I'm not a good person to explain it -- but '95 will be producing basically a final report on the morphometric comparison between the '70s and the early '90s. In a nutshell, if we'd been lucky, we would have seen some great dramatic difference between the '70s and '90s and how fat the seals were and how big the seals were. In fact, the sealions they saw 15 percent difference in size and age in that same time period. The sort of preface to the report, there is no such obvious difference for harbor seals, so our work isn't going to be easy.

Starting the '95 new work, as I said Mike will be continuing the morphometric analysis, looking a little more at annual variability, and I think that without overstating it or being overly optimistic, looking at some of the SEA plan work in this lake-river hypothesis where some years are different than other years and it may certainly affect the productivity or survival of forage fish, based on the '89 to '94 harbor seal work there are apparently some very strong annual differences in the condition of seals. Some of them are actually statistically significant differences. Now, what this means, we don't know. But I think it's sort of a low level background approach. Castellini et al. will also be looking at the health of these animals, blood parameters, any obvious changes in blood parameters that might imply that the animals are compromised, a comparison of adults with sub-adults to see if there is some apparent difference between these age groups.

Feeding -- looked at from a variety of ways. Diet -- through a variety of techniques, stomach contents analysis if we are able to get stomachs from hunters in the Sound, stable isotopes, fatty acids, actual foraging behavior to see over what kind of an area these animals forage, at what depths they forage, and then trying to get at this availability question, and for that question will be interacting and relying heavily on forage fish people, the herring studies, the nearshore fish studies of not only fish but prey such as octopus and actually looking at the energetics of the prey which is some of work that Graham Worthy is going to be doing, variable energy content or caloric content of different seasons across species. And here again with a projected end in 1998, a wrap-up report, hopefully giving us some of these questions about what do harbor seals eat, what kind of seasonal variation is there, are there differences within the Sound, and is there anything there that leads us to believe that our hypothesis that food was problem is right. You know I told our session yesterday, none of us are naively optimistic about this. This is a thorny, difficult question. The EVOS program is not the first to look at this. People have been trying to get at this for at least the last 10 years, and in some cases longer than that, for both harbor seals and stellar sealions. A lot of agencies are spending a very, very lot of money trying to address these questions, and we are all waiting for that breakthrough to come, but so far we haven't had any fireworks go off that says this is it. I think one of the advantages this program has is what you see and hear here is the EVOS-funded component which has a very strong Prince William Sound focus. In addition, there are complimentary studies basically identically designed going on in Southeast Alaska, in the Kodiak area, the Gulf of Alaska, and also very similar studies going in with sealions, and so we are coming at this from more than one direction.

DR. SPIES: Thank you very much, Kathy. I think you appropriately addressed some of the main questions we had in terms of milestones and scoping out the proposed length of these programs over the next several years, which I think is extremely helpful. I would like to now open it up to the core review panel here to ask questions of the group and Kathy based on what you heard this morning.

DR. GEORGE ROSE: I guess I can start with a couple of things. First of all I was very pleased to see these very well laid out hypotheses, which seem to have developed a little bit after our discussions of yesterday. This is a step in the right direction. I have a couple of concerns though of how we will be able to test them, so I made a couple of notes here. One thing that still troubles me about this, it still isn't clear what kind of data we need to actually test those hypotheses, and I am not convinced yet that the links between the various participants in this project, that is whale people and the people who will produce the forage base data, are clear cut enough to resolve those questions. So I'd like to see some action on that front so that the forage people, people who will actually produce the forage data, know exactly what it is that you want or as exactly as you can state it at this time. Obviously, that will be an evolving process, so that, as we discussed yesterday, the survey design and the way the ecosounding would be done, it would be done the right way, because if we don't know that to start there are many different ways to do that, to survey, and we don't want to find out after you've invested a lot of time and money that, oh, my God, we did it the wrong way, and we don't have the appropriate data. That would be disastrous, so I would like to see some action on that front. I'm also not entirely convinced that the statistics of this game, the statistics of the model that would be used to test the hypotheses, are not going to kill us. That is, we are going to have to be able to parameterize whatever model is being used to tests these hypotheses well enough so that we can make a clear cut test whether we can reject or accept that hypothesis. A lot of this work that is done -- you alluded to this, Kathy -- we end up with a wishy-washy solution -- we have this on our own part of the world -- marine mammal work seems to be particularly prone to this, where you can never decide one way or the other whether you should accept or reject the hypothesis. This is a danger in this project, and I would like to see some clearer thought be brought to bear on this, on what we can reasonably expect in terms of the measurements that are going to be made and the variability in those measurements and what needs to be done, so that at the end of the day you will be able to give us a reasonably clear answer to your hypotheses.

MS. KATHY FROST: And I would come back and ask you guys for something, and that is, as you well pointed out, this is a problem in the marine mammal world, and I can't help it. That is the world we are buried in, and we have this problem with sealions and we have it with harbor seals, and we've got to do a lot of things, and one of the potential advantages of an interdisciplinary group like this is maybe you guys can help us get out of our myopic little marine mammal world that has made us less able to ask these questions so that they provide real answers in the past. We've been muddling around with this for 15 years, and it's not for lack of thinking. We don't always state these things more clearly, and so, as I said yesterday, we're looking for help and suggestions as well as --

DR. SPIES: Isn't part of that just a function of marine mammals being very (indiscernible) environment, difficult to study, to take samples only under special conditions, and so on and so forth, so you've got you're own special set of limitations that maybe limits the kind of data available to maybe resolve some of these hypotheses.

MS. FROST: We're stuck with that, almost the one have incredible legal restrictions on doing some of the most basic sampling, and they are very flexible, versatile animals, and they switch around a lot and do a lot of different things in different areas at different times of year. I started out wanting to assume that Prince William Sound harbor seals and the Gulf of Alaska harbor seals were declining for the same reason or were continuing to decline for the same reason. I'm not even sure we can make that assumption. So, it's not a cop-out, but it is hard to get at. It's hard to figure out. It's not always hard to figure out what measures we'd like to take, it is sometimes hard to figure out how we can practically get those measures.

DR. ROSE: I appreciate the difficulties with it, but I still don't believe that it answers my point in that it doesn't really matter about the difficulties, if we set out with a hypothesis and we claim we're going to test it and we know we can't, what are we doing? We can't approach science that way. We have to have some reasonable assurance to start out with a hypothesis that we can actually test it. Now, we don't always know this. In the real world, obviously we don't always know this, but if we know we can't, that's a poor place to start.

MS. FROST: Some practical things that are right now being done that are not stated here, and some of those measurements which really are measurements are differences between sub-adults in Southeast Alaska and the differences in the Gulf of Alaska, differences between blood parameters, stress protein parameters, and those are real measurements compared in areas in the spill and outside of the spill, declining areas and non-declining areas, and those are things that are, indeed, measurable and testable with the sample sizes we hope to obtain. We are lucky in Prince William Sound, but you guys have to pay me a few years to do it, but I've finally figured out how to catch seals, and we are now looking at sample sizes of 70 to 100 per year, which is finally enough to do something.

DR. SPIES: Any other questions from the reviewers?

DR. CHRIS HANEY: Well, I would recommend that the you use the geographic sub-units that you are sampling in Alaska as a basis for measuring the recovery and restoration, whether those various parameters which will have numerous opportunities for statistical analysis, whether they are converging on each other, whether the rates of change are equivalent, and you might be able to compare and contrast Prince William Sound with Southeast Alaska and the Northern Gulf to see whether these differences in harbor seals in PWS persist. When they no longer do or when they begin to converge, then I would think that you may have objective basis for saying that (indiscernible) begin. You're lucky you have such a broad geographic area to compare to.

MS. FROST: Well, in fact, that's the reason we have these sites, and right now we are having to do the same thing with sealions in trying to figure out -- we know one group is, while not termed healthy, is increasing, and the other area is declining, and people are proposing a variety of hypotheses of why that difference occurs, and we are going through trying to systematically eliminate a lot of these possibilities. You're looking for differences in this or differences in pupping rate between Southeast Alaska and Prince William Sound. That one we have been able to eliminate. We have had a number of disease, you know, do we have more healthy less diseased animals in Southeast Alaska than Prince William Sound? No, we don't. The disease incidence is the same. The Southeast component, although it has nothing to do with the oil spill, is absolutely essential to making any progress at all.

DR. HANEY: What parameters in the harbor seals in Prince William Sound are still distinct from all the other regions?

MS. FROST: The fact that they are declining.

DR. HANEY: The rate of decline?

MS. FROST: Uh-huh. We can't separate them. When you start looking at morphometrics, the condition indices, and density and disease, and a lot of that, we can't tell them apart. Now, I think Castellini is hoping with some of this physiological work we have to start looking at blubber content and energy content of the blubber. The other thing that maybe distinct but it's not fully analyzed and it's pretty touchy-feely is the stress protein business, and right now we're not sure how to interpret the results we have, but there is a difference in stress protein levels between harbor seals in Southeast Alaska and California-Washington-Oregon coast in the Gulf of Alaska and Prince William Sound.

DR. SPIES: You are referring to lukens and haptaglobins. Also eschoc proteins are also called stress proteins too.

DR. ROSE: Given the decline of the seals, you have on the board or on the table there's a lot of indirect methods, but what about some more direct observational methods through transmitting tags or something where you could actually bodies. This business of no bodies is hard to ever pinpoint. Have you given any thought to find bodies, know when the mortality is occurring?

MS. FROST: Yeah, the honest answer is, you ain't going to. With the whole Gulf of Alaska out there, harbor seals depending on the time of year sink to the bottom when they die. Prince William Sound is a very deep area. We don't have nice big sandy beaches like Sable Island. They don't crawl up on the beaches and die, and we have such a wide tidal range, even during the oil spill carcasses were washed in 6-12 hours, and so the carcass recovery question is very difficult. The sealion people have been trying to get at this sub-adult juvenile survival business, and their initial approach has been to put satellite transmitters on juvenile animals and then look at the rate of disappearance. There are a lot of problems with that because there are so many reasons that a tag can disappear. Our technology is good and getting better, but it is a far cry, I think, from being reliable enough to compare survival rates of juveniles versus adults, for example, based on how long the (indiscernible) last. There's sort of this leap of faith that says you put a radio on and it goes out sea and disappears and, oops, it died, and we can say that we had a mortality here. It might have died, the battery might have failed and several other things might have happened, a killer whale might have eaten it.

MR. CRAIG MATKIN: ... there were 13 tags of sea lion pups found in one beached killer whale, so I think weakened animals are taken care of real quickly.

MS. FROST: If we can figure out how to catch animals a second time, there would be a whole new sweep of things we could do in terms of particularly evaluating the status of animals, looking at sub-adults over time. At the moment our catch procedure is so random, we really cannot recapture the same animals.

DR. SPIES: So you are educating the harbor seals when you catch them? (Laughter)

MS. FROST: No, actually, they've educated us. We can catch them. When I started this it took us nine days to catch four seals, we now catch every single day we go out there.

DR. SPIES: But once you catch a seal, they are smarter?

MS. FROST: Well, just the probability of -- we are not able to target individual animals. The probability of finding five animals out of how many thousands there are in Prince William Sound, pull them out on a rock with a suitable catching. One of the other things that we need to investigate when I talk to the hunters they say what we look at is seals in Prince William Sound on rocky and intertidal haul-outs that we can go up to in small boats. Fifty percent of the seals in Prince William Sound are on glacial ice that is at the head of these glacial fiords. You may be doing something very different and there have been some suggestions by hunters that pupping rates may be different out there. We can't figure out how to catch them. Now, there again, I was in a meeting in December and one of the guys from Chenega said that maybe they could figure out a way to try something different and get in there and catch those glacier seals, but that would within Prince William Sound comparison, if in fact there was a difference there. We are limited by what we can figure out what to do, and you guys don't want to spend a huge amount of money paying me to do these esoteric sorts of things.

DR. SPIES: It's been a very good investment so far. Pete.

DR. PETE PETERSON: I've got a comment, I guess, more than a question, although some of it may form a question. I would like to make the remark that I think that this project as it has evolved has been very responsive to the call, work plan, science plan in that what has happened here is that we have a serious problem with some animals that we, as a society, have great interest in preserving, and the problem is a long-term one, and it's obviously exacerbated by the oil spill. So, that we have opportunity here to take one of precious resources, perhaps they come in 20 year intervals if we can look at the outset, and then who knows -- to take precious resources to try to make major advances in addressing issues of societal concern in the natural resources that are so wonderful in this system. I think that that interactions between people in different agencies, federal and state and even private organizations that have been brought together to try to focus their expertise on this progenary problem should be encourage and is a very positive, and I hope will be a long-lasting legacy of the unfortunate event of the spill. This particular project shows that continuing level of interaction between the university, private elements, agency -- federal and state -- scientists. I think too that it is terrific that we have world class scientists involved in this project still. I could name them with some embarrassment, but I won't. And new ones have been brought in who are high quality to address particular issues that their expertise can be applied to. I worry a little bit that the budget limitations may force us on this project and on others to take shortcuts and to give up on problems that might be solvable with some modest additional resources. I don't have any explicit particulars on this one because I have not yet seen a final plan and looked at it, but generically through the projects that I have looked at and the discussions over the last couple of days, I'm a little bit worried where we've given up on a couple of things where a bit more in the way of resources might let us actually answer the question better. And there's that bias -- we as researchers always face the problem that we know that resources are limited, and indeed in this case they are, as in all. So, I don't mean to try to open the floodgates for more, but to think very carefully about whether a modest additional investment might yield more explicit and convincing answer to some question seems to me something to think about it. The ability to relate some of the problems here in marine mammals to some of the resources, namely fish, by both the type of fish where distribution in space and time is really exciting. I mean that is the fundamental sort of linkage that we have wanted for a long time, and the legacy of results that come from this can inform management and improve the way that we deal with preserving our natural resources for the benefit of all into the future. I think that that is an important legacy. You know if we just spent our settlement monies from the spill and did nothing more than watch our systems recover or enhance something or augment something in some tangible ways to recover faster, we would still have lost in the end because all of that time during which the resources have been depressed below levels which would have prevailed in the absence of the spill, we would have nothing to show for it. We'd have alternate negative on the balance sheet of the societal value in the system and what we've lost. But when we can, in fact, produce knowledge of the sort that this project promises that can inform management into the future, and therefore provide some dividends year after year as it is applied, I think we have in fact achieved something that we would not have had had the spill not occurred, and that can help make up for those times that during which the resources have been diminished and while they have been recovering. And, as you know, the marine mammals and the issues with marine mammals are very serious ones, serious ones as they affect fisheries and fisheries policy and the way in which we make a living from the ecosystems on which we depend. So these sorts of projects and the understanding that can come from these are truly valuable, so that we can continue to harvest the goods and services from natural ecosystems without the problems that might intervene in the absence of knowledge, such as shut down this fishery or do this or do that, not that we know that that would help, but just because we suspect it might be a problem. So, I think with knowledge we will make better use of this resource and all of the resources, and that this project is the sort of thing that we've been needing for years and the eyes of the world will be upon. I agree with George, and George has looked at this more carefully, but some more clear definition or hypotheses and whether you can distinguish one sort of mechanism from another might be valuable, but incremental gain, if it's substantive in this work, will be of value, and that that's something we need to keep in mind too. I also think, and Kathy started this, that this -- at least in recent times -- the unprecedented declines in herring in this system might provide an unfortunate opportunity to see what impact that may have upon certainly the harbor seals but perhaps the whole marine mammal system in general. So that there is a temporal aspect, as well as the spacial one, that I think makes this project timely, so that we're talking about when this thing might best be done. This is a very interesting time in which we find ourselves, relative to the herring and their role in the ecosystem and how they feed and their interactions that are important to the marine mammals and especially those of harbor seals. But, anyway, that was more or less a speech, but I wanted to balance the cautions that George has been giving, which I'm sure I share when I get to speak to those, with what I feel to be the very positive aspects of this project.

DR. SPIES: Thank you. We'll certainly be relying on you and the other reviewers to make recommendations when you think some sort of incremental funding might achieve some sort of breakthrough here. We're at the point where we need that kind of insight. Phil Mundy, you had a question?

DR. PHIL MUNDY: Got a comment and a question. I think Kathy correctly identified the introversion that people who have worked with different animal groups sometimes have. I think one of the strengths of the restoration studies is the interdisciplinary nature that they possess. So it would be helpful to me as a reviewer if you would identify the extent to which a successful outcome relies on the results of the information of other studies specifically, and the extent to which other studies may rely on the information that you are producing for successful outcome. Specifically, you have the stock structure definition and range definition problems that are common to anybody who works with an animal population. It was my understanding that stable isotope holds out some promise in that area. So that is my question.

MS. FROST: Genetics, I think, is probably more likely, Phil. We're doing a lot of genetic work on harbor seals right now. I mean, it's ancillary very much to this study, but certainly relevant to the Alaska-wide question and all of these animals are being sampled for those pieces of skin and being sent down for analysis. That doesn't entirely, as you know, get at -- it doesn't solve the stock structure question. The animals may be homogenous across the state and still behaving differently, and in effect you've got a management stock for other reasons. We are trying to look at this stock handling question.

In terms of cross-disciplinary, I was just going to make the comment, these meetings sometimes people moan and groan and grumble they're expensive and you've got to take a week out of your life, but they are really valuable because no matter how much we try to call our co-PI's, we all get wrapped up and we don't hear the organized, plenary, big picture. And for me, just like Pete was talking about herring, I've been flying around Prince William Sound for five years, and knowing that Channel Island was the biggest haul-out in Prince William Sound, and you know there's something in your head that says Prince William Sound is the core harbor seal area and these big haul-outs, and you just sort of leave it there because all you do is fly around on airplanes and you don't think why you do it. And then I got talking to Evelyn and John and I heard about the Montague trench, and I didn't have a clue what that was and that there were herring there, and then the herring might be there all year long, and somewhere about two days ago the light went on, and that, wow, there's herring here all year long, and Channel Island is right in the middle, and then Sarah Iverson's fatty acid stuff came in showing that all those Channel Island critters were probably eating herring. I don't have it made into a hypothesis yet, but my thinking has significantly changed in the last two and a half days by having all these other people here and piling this information on top of itself, and I would encourage the Trustee group to spend the money that I know is precious to keep letting us do this because it does pay off.

DR. MUNDY: Yeah, I'd like to reinforce that. I felt that that was -- even though it wasn't a centerpiece in our discussions yesterday, to me it was one of the most interesting outcomes, and I think it actually came from Scott -- about 10 times. (Laughter). I thought that was a series of very fascinating observations that was presented and the very kind of thing that the synergy of a group like this should produce, as you said, Kathy. And I don't think that those are the kind of things that should be just left -- although we talked about this at the meeting, let's all go home and forget it. I think that those are the kind of things that should be built on because you really might be on to something here. The dynamics of the major pelagic species all around the world, every fisheries ecosystem, if we can define it that way, are extremely important to what happens in the whole ecosystem. I think we can just say that as a blanket statement. And it's not this kind of relationship that we've only thought about in very vague terms right now that I believe should be pursued, and possibly to the point of putting down some gut feeling hypotheses, even based on no data -- I don't see anything wrong with that -- about what could be happening here, and try to outline possibly how those kind of ideas could be tested, what data would be required to do it, whether those kind of data are available now or how much it would take to get them. These are the kind of thought processes that don't take much time but could be very, very rewarding scientifically, and also in terms of understanding what is really happening, not only in this ecosystem but in the broader context.

DR. SPIES: I think this workshop provides one forum to do that, and also I think when I was talking about the creative assembly of some sort of a program ultimately limited by resources, we have to think about mechanisms, vertical integration of studies that might achieve the same sort of synergy. That's very difficult to do with 70 studies because you can't just make one huge study out of it, it's too inefficient, but when you're dealing with a smaller horizon perhaps, then put some creative outcomes to it so that more of this vertical integration, we wouldn't have to rely on scattered phone calls or one large meeting every year to achieve the same goals.

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DR. SPIES: (Introductions) ... Stan Senner with the Audubon Society, Boulder, Colorado, a long history in his previous life as head of the restoration effort early in the spill; Pete Peterson, University of North Carolina, has been a reviewer since 1989; Phil Mundy, independent consultant from Oswego, Oregon, a long history and familiarity with Alaska fisheries issues; George Rose from Newfoundland; and on the far left, Chris Haney from Pennsylvania State University, ornithologist. Any other questions of the panel in the marine mammal -- if not, I have one brief question. All the participants in that group, I think, have a high level of professional work but still there are a lot of other efforts going on out in the Gulf of Alaska, and to what extent could we be assured somehow that the results of those other efforts in terms of long term declines of harbor seals and sea lions and to what extent this process could be informed of what is going on elsewhere? Is there some sort of integration, other than in your head?

MS. FROST: I think we are extremely fortunate, and I guess I would brag a little bit on the marine mammal, the pinoped decline problem, in that Fish & Game and National Marine Mammal Lab in Seattle, have worked very very closely together for 18 years now, and I guess Lloyd and I have been the dark horses in Fish & Game -- no one in Fish & Game figures out why you ought to study a seal. I mean, we're in game division; we're not with the fish guys. They could maybe figure it out, but most people don't have a clue, and so we've had to look for (indiscernible) somewhere else, and basically, NMML, the federal agency, are our colleagues and our work partners, and so, I mean, on a daily basis, there is a cross-communication. The harbor seal work is jointly funded. It's all joint. The stellar sea lion stuff is UAF, Mike Castellini and his crew; and then the harbor seal stuff has all the connections. So, it is probably more integrated than any similar program, just because there are so few people involved and such a long, long history of working together. Don Caulkins (ph) is doing the sea lion stuff. He's been doing it since 1971, and Lloyd and I have been working with him since 1975, and this is throughout the whole program. We are very fortunate. One of things a lot of us have talked about is how to more formally, instead of in my head, bring some of that information to this group. I think if Mike Castellini had been able to be here at this meeting, Mike works in both species and all of those areas, and as we proceed into the future a lot of these cross-connections will available, not just through me and my head but through Mike and his cohorts.

DR. SPIES: Thank you very much, Kathy. That's a very useful discussion. Can we open it up now to members of the audience for any further comment, questions. Peter McRoy.

DR. PETER McROY: Kathy, I want to add a suggestion to the list of hypotheses where I think you are missing a major integrated question about this, and probably most of the other component programs are in this, and this is, is it climate? This is a real ecosystem enforcing function that at least the SEA project has a major integrated field that these ocean cycles have -- by climate I mean ocean atmosphere interaction and all the things that evolve from those cycles -- so (indiscernible -- coughing) populations and all that (indiscernible).

MS. FROST: Peter, I guess I would say that it sort of (indiscernible). When I say, is it food, I'm letting the food people say, is it climate? But I agree with you very much. I think the whole food deal may well in fact, may likely be driven by climate, and I just tend to go easy steps, which isn't fish.

DR. McROY: We will have that kind of climate data base through the life of these projects, that is (indiscernible -- out of microphone range) and I suggest that the powers that be consider expanding that access. I haven't asked Vince about that, but I'm sure we could probably do it, but it's not just our oceanographic and related plankton data. (Indiscernible) I mean, how do I get access to say, boy, I wonder what's been going on in the harbor seal population besides calling Kathy.

DR. SPIES: John French brings out the point about the shift in the species composition of fish in the Northern Gulf of Alaska which bears some relationship to these large scale, long-term frenetic changes, and to what extent those kinds of changes can be considered. I'm sure they have. Probably want to discuss with -- maybe related to the juvenile survival seems to be, in the case of harbor seals, as I understand it, the key life stage at which some of these mosses are being found in that population.

Those types of relationships are certainly worth considerable thought.

Yes, Bud Rice.

MR. BUD RICE: I don't know, maybe this has already been addressed, but it seems like we have been working on harbor seals and survival of the sub-adults, and I just wondering if this coordination with subsistence-takers if they are taking sub-adults inside Prince William Sound or outside Prince William Sound. And even if they are taking adults, is somebody working with the subsistence people to get stomach contents so we can found out what they are feeding on. If they are not taking sub-adults in these areas, (indiscernible -- out of microphone range) these are the people who probably could, just by the taking (indiscernible -- out of microphone range).

MS. FROST: To respond to that, probably we would not apply to a program (indiscernible) animals because it looks like a lot of these techniques. I did nothing but analyze stomach contents the first 10 years I was up here, and they are useful but actually in a lot of ways stable isotopes and fatty acid work provide an integration which you don't get from a point in time in the stomach. For example, if I had the same number of samples from Channel Island and Stoffield (ph) in stomachs that I have in fatty acids or stable isotopes, it would probably not tell me as much because they were from one bay, and I might tend to think, oh, a school of herring swam by. But we are trying to integrate various low-level but developing effort to work with the hunters. There is is a real interest in the villages to help survive samples. Kate Winn (ph) in Kodiak has a developing program through SEA Grant. The subsistence division, you guys paid for this food safety program, and actually 7 to 10 harbor seals were collected this fall to look at contaminant analysis, blood from all of those, blubber from all those, DNA plugs from all of those, (indiscernible) and stomach (indiscernible) from all of them. So we are trying to do that.

DR. SPIES: Why don't we move on now to the birds and seabirds. Dave Irons, if you could inform us of the deliberations of your group.

DR. DAVID IRONS: Well, for the bird end of things, 1994 is going to be quite different from what we are proposing to do in 1995. It's a major transition for a lot of the bird work. In 1994, there were several, independent bird studies and there was a pilot forage fish study. In '95 we are proposing a larger seabird-forage fish study with several sub-components. What I'll be talking about today are basically that the seabird-forage fish study that's proposed for '95 and then there's also a marbled murrelet study and then two monitoring studies, one on murres and one on this gentle bird of Prince William Sound. What we did was we went out and came up with some milestones and end points, and so I'll just run through those for you.

This is the seabird-forage fish project, even though it says forage fish -- we're still having a name crisis here. Milestones of this seabird-forage fish project, in Year 1 we'd like to complete a forage fish survey of the entire Prince William Sound, looking at the distribution and abundance of the forage fish. We also want to do the first comparison between seabird diets and productivity with the acoustic and net sampling of fish in the Prince William Sound level. We have determination of whether we need to look beyond the Sound to test models explaining the seabird-fishery or seabird-fish interactions, without a lot input about can you answer your questions by looking just in the Sound or do you need to look in the Gulf of Alaska. So the first year we will be exploring that. Year 2, the potential expansion into the Gulf of Alaska, if that's what the decision is. In Year 3, "A" -- it will be the first picture of the forage fish population patterns, and then "B" we will have an initial idea of the strength and the length between the forage fish productivity measures and the fish population trends, and "C" will be an initial determination of the whether the food hypothesis can really be tested successfully, and we will decide whether it looks like it's a promising hypothesis or whether some other hypothesis is more promising at that point. Obviously, today we feel the food hypothesis is the most important one, and that's why we are pursuing it. Year 4 would be the same as Year 3 for the Gulf of Alaska, and Year 5, which is basically the determination of the study, we have a substantive idea of where fish stocks are going and why, and predictive model linking such trends to seabird population trends, design of monitoring scheme and selection of species and areas to monitor, and initial test of each ecosystem models that best describe our data. I didn't really touch on the hypotheses of the seabird-forage fish study and as we heard in my presentation the first day, basically we're looking to see is food the limiting factor in the recovery of injured bird species. To do that, we are going to look at the bird species themselves and see how they are reproducing. If they are not reproducing, then one question is are the chicks limited by food. If they are limited by food, then you look and see are the chicks actually getting food and the composition that's important, or, if they are not getting the food, then you say why are they not getting the food? Is it unavailable or is just non-existent? Seabirds cannot forage 700 meters deep along the Sound. Some are only surface foragers, some can forage down 50 meters, and some 100. You might have large food reserves in the Sound that are unavailable to the birds. Then the temporal and spacial variation of the food might be the key role here as far as food limitations of seabirds. It may be limiting only in some areas or at some portion of the breeding cycle. As far as end points, as far as the seabird-forage fish project we are proposing to go 5 years, and these are the subcomponents. I'll run through these real quickly as far as the end point and what the projected cost is, whether we expect it to go up, down or stay the same.

The first subcomponent is forage fish assessment project, a five year end point, and the cost will probably remain the same for three years and then maybe decline.

The second project was the forage fish assessment project bird end of things, that again five years, and it may increase if we -- we've been working with SEA to try and get more information by having bird observers on their hydracoustic boats, and if we do that we may need a little increase in cost to that component.

Forage fish diets: That's a three year end point, and that cost could stay the same or go up if we needed a larger sample size. This is a puffin project that uses puffins as indicators of food composition. Puffins go out and sample the environment and bring food back and will give you an ideas of what's representative out there, and that is a pilot project this year. It seems feasible in Prince William Sound, so it might go up in cost.

Kittiwakes: We use those as indicators of food availability. That's a five year project. It will stay the same or go up or down depending on what parameters to key in on and maintain throughout this project.

Pigeon guillemot: Again, that looks at their reproductive success and population changes. It's a five year project.

Seabird energetics: As I said, seabirds might be getting food but they might still be doing poorly because the food is of low quality, and seabird energetics will look at that aspect of the study. It's a five year project, and the cost will probably remain the same.

The energy of fish: This again is tied in with the question of are the getting enough energy, just the composition of the fish, and that's a five year project to do the same, cost.

Then we have an existing data modeling component in which we are going to try to look at existing data on fish availability or fish abundance, composition for the Sound, and if we expand to the Gulf it will increase that for the Gulf too, and that's a five year project.

Then the program management five year project will be the same for quantities and costs.

The other projects outside the seabird-forage fish project, the marbled murrelet project is a three year project to develop an inexpensive method to get a handle on the index and reproductive success of marbled murrelets, and that would probably remain the same for cost for the next three years.

Then we have two monitoring projects, as I mentioned: the common murre project and the bird survey of Prince William Sound. The common murre project: I just want to say a little bit about that. We support the common murre project to go forward in '95 for two reasons. One is that it is in the monitoring plan as outlined last year to get reproductive success on common murres in '95, and also there's been this large abundance of capelin seen at Barren Islands, which, as we referred to earlier, capelin kind of disappeared from the Gulf of Alaska since 1978, well, they might be coming back. Now, if we are going to do an ecosystem study of the Sound, we expand to the Gulf, this observation of large amounts of capelin might be key in that they might be moving into the Sound two years from now, and so we want to get an idea of what is going on at the Barren Islands for capelin.

The bird survey is currently set to be done every three years.

Once every three years there is a monitoring project. We had some discussion about that as to whether or not it should be done more often when we doing this large seabird-forage fish project, and some of you thought we should and some thought we shouldn't. We haven't come to a final decision on that.

As far as how we view interacting with other projects, this overhead shows where some of the interactions come. Basically, our aim is population recovery. To get that the birds have to reproduce. To reproduce, they have to have energy, and the modifying factors that modify the energy available to them, such as predation, hydrography, and zooplankton abundance, and interactions and data needs between the seabird-forage fish project and other projects, SEA is the major one. It comes mostly in at the modifying factors level, where we need to interact with the salmon predation project, the zooplankton project, the (indiscernible) radio isotope project, the nearshore fish project, and physical oceanography components, and the information system and modeling development component.

Marine mammal projects: Both the harbor seal and killer whale, as Kathy mentioned, we recognize as food. Forage fish are a common link there and would be interacting with both of those components.

Other bird projects: At this point the murre project and the murrelet project both need information that we have on forage fish availability and they would be providing information on any productivity. Over here on the nearshore bird predator project, there's a guillemot component of that which will help tell us if other factors such as oil are important.

So, as far as summary on what we did, that's all I had to say.

DR. SPIES: Thanks, Dave. I'm sure we have some questions from our reviewers. George.

DR. ROSE: Yes. I have one overriding concern with this group of projects, and I think it comes out of the fact that it is a group of projects which is relying potentially essentially on all these forage fish questions on a database to be produced by an acoustic survey. It's a little analogous to the oceanographic work going on in SEA, which you can look at it on its own right there. You can also look at it as a sort of a service data for all the other projects. The acoustic survey work is similar to that, and I think with all of the demands or potential demands, although I haven't heard them clearly stated -- I would repeat the same comment I made to Kathy about the marine mammal demand for data -- I haven't heard it clearly stated just what data you want from a survey, but in thinking that through it's going to be very difficult for the survey people to supply the large suite of data that may be required by this project. In normal terms what you would do with a survey like this is you would optimize it to the goal to whatever it was. But if you've got here five or six goals which are going to be conflicting almost necessarily, because you're dealing with so many different species, it's going to put a heavy demand on this survey design. So, that's an overriding concern I have with the whole structure of this, whether it's really do-able. It looks good on paper, but trying to fit these things together may be very difficult, especially given that we're trying to track mobile predators and mobile prey, which is not a trivial task in the oceans, as everyone who has done this kind of work knows, and when you are trying to impose many different species onto the same sampling regime, I think we may be getting into a problem. We discussed this yesterday, but didn't come to any firm resolution of it, but I would like to see some further thought given to this and have it specified much more clearly as to what is expected of the forage fish component and can they reasonably, without driving themselves right around the bend, deliver on these expectations. That's my point.

DR. SPIES: Dave, would you like to comment on that? I know that was discussed pretty extensively in the group.

DR. IRONS: Yeah, I appreciate your comments, and we will consider that.

DR. SPIES: One thing that strikes me is that with respect to the hydroacoustics both the SEA Program and the proposed forage fish program have a lot in common in terms of relying on hydroacoustics and there's a lot of questions about what species can be separated and their abundance accounted for in Prince William Sound. I think that's an area that we have to pay close attention to and work on.

DR. ROSE: Yes, that's one of the fundamental questions that has to be raised. Just what level of discrimination is required by the biologists. In an acoustics survey, any (indiscernible) is possible. From no discrimination to discrimination by taxonomic groups or by aggregation types, by target strength -- there's many possible combinations. But if the people who are responsible for producing this data don't know ahead of time what is really required, how can they possibly produce it.

DR. SPIES: And the biologists, in turn, need to look to the oceanographers to see how well they can discriminate so they are better informed.

DR. ROSE: The acoustics people have to know what level of discrimination will be required, then they can assess and do the research required to see whether they can deliver or not.

MS. FROST: I ought to know after sitting there all day yesterday the kinds of questions on the forage fish studies. Is there a plan -- there's this overview, widespread sampling. But I wonder if there's not some merit in picking a small area and just really doing it intensively because -- at least, we were trying to look at seal diets and sort of these trophic dynamics of seals in the Bering Sea. The National Marine Fisheries Service spent millions and million and millions of dollars going out there doing fishery assessment, and it gave you the big picture on what was going on in the Bering Sea, but it really was not very useful at all when you started talking about what seals were doing. We needed to be in an area where we were collecting seals and looking at diets and getting samples, all right at the same time and the same place, and you could either sort of have this background information of what is going in Prince William Sound, and, unfortunately, it would have been nice if National Marine Fisheries Service had done that 10 years ago, and we could start from there. Somehow it seems that if we are really going to piece this out, we have got to get into a few areas and just beat them to death. We talked about that a little in past in important areas, but --

DR. BRUCE WRIGHT: You are right. The sampling design has been a question going back and forth a number of times. We do not understand what is out there for forage fish. Obviously, it's going to be patchy, but how patchy it is we don't really have a clue for the whole area. If we had that background information, the database, we could establish and it would be easier to set the sampling design to key in on the key areas that you are talking about. We are trying to mix our sampling design at this next year's, Year 4 survey plan, and all four of those will be broad surveys. Two of them in the four time period (indiscernible) will look at July and August (indiscernible -- out of microphone range) key areas, so when we're trying to mix our surveys, there's an awful lot of questions that we answered before we can modify the designs that will fit everybody's needs.

DR. SPIES: So, the fundamental question, I guess, is to whether you attack this whole thing from the standpoint of predators -- marine mammals and birds -- and I heard from Dave Scheel yesterday that if you're going to deal with patchy populations that you probably are better spending your time more efficiently dealing with the predators than the prey, but there also is this aspect of what is the source base and are you answering that question as well and how important it is, and I'm not sure we've gotten the answers or the approach that we need in the end here to successfully carry out their programs.

Are there any other questions from the reviewers? Pete.

DR. PETERSON: I just had one, which is I'm -- let me phrase it this way -- to what extent could some hydracoustical data from the SEA Program be used, given that that is quite intensive in a particular to integrate into this project, and then provide some of what Kathy is talking about in the sense of very intense time and space information relative to foraging of some of the birds that you are involved in?

DR. IRONS: That could be used in some sense to look at that to see how much availability there is. A large portion of the SEA data system was collected before a critical period for us. By July and August they are almost gone. I think they wanted to run through July, and the areas where they are looking are not necessarily the critical areas that we focus in on, but to see how much variation there is, at least during July that could be used for (indiscernible).

DR. WRIGHT: Again, there is the conflict with the key species that SEA people are interested in, and the SEA group is interested in a different corridor or time frame. Although their data may be useful, more likely our data might be useful to their projects, and so the hydroacoustics people have been sure working together so that the data is comparable (indiscernible -- out of microphone range).

DR. SPIES: To what extent have discussions taken place with the SEA Program as to actually using the same database? Gary Thomas.

DR. GARY THOMAS: Actually, I'm working pretty closely with Dick Dorian (ph), Ken Coil (ph), and of course we're using new technology (indiscernible), new software, out-rhythms (ph) for processing data, (indiscernible -- out of microphone range) -- talking and work together on this. I'm (indiscernible) . . . have talked about doing this. (Indiscernible) classification routines were -- they may not be applicable by transferring it over from one season to the next because of the changes in the school signals and (indiscernible) behavior. Some of them probably will. We're certainly working with the same assembly, so we'll run into the same (indiscernible) and we're very much (indiscernible) . . . George Rose said the other day when he had this enormous problem facing him, especially the new technology, and the software has to be developed, and its a massive (indiscernible) management problem. The only way you can do it, the only way it makes sense is to combine all our reports and (indiscernible).

DR. STAN SENNER: Bob, I'd just like to add on that point that it has not been immediately evident to me how much discussion between the forage fish and SEA plan groups there's been to get to where they are now, and we have a SEA plan project with -- I don't know the dollar amount but significant -- in place and a forage fish project which will be in the order of a million plus possibly getting underway, and I'd at least like to have some greater confidence that the respective means of the two groups are actively taken into account in the planning and execution. Maybe that's there, and I'm just not aware of it, but I'd like to have a greater comfort level on that point.

DR. SPIES: I assume we have a key point here in the development of our ecosystem programs, and the key point in development of the funding picture, and the integration here, and I think we're at an extremely crucial point that we should really be paying attention to. Ted.

DR. TED COONEY: Yeah, I would just like to speak to that briefly. SEA has, this past year, gone through an evolution itself where we make sure that we're wired up properly and communicating internally. It wasn't clear exactly what was going to happen with forage fishes and birds, and so while that was sort of settling out we began to make some overtures about how we might be able to interact. Hopefully, to make you feel more comfortable, now that the Dave Duffy/Dave Irons have been essentially capped as leadership in this group, there will be a point of contact, and I certainly will be interacting. I just want to encourage that.

DR. SENNER: I've got some other comments too on this before we're done.

DR. SPIES: Perhaps we'll go to Dave Salmon in the back.

MR. DAVE SALMON: Yeah, I have a couple of comments. First, with regard to the forage fish study, I notice one of Dave's goals was to do an entire Prince William Sound-wide survey of forage fish, so within that context the data we've collected would certainly seem to be extremely useful. But beyond -- what's gone on thus far is we always talk about coordination and integration, and those are great words, between the various projects, and meetings like this are invaluable towards achieving that actual coordination and integration. It gets the people talking and that really is a great experience. It's very easy to just share data, but I think what we're talking about here is going beyond just FTP-ing files back and forth between the various projects. That's easily achievable, we're all set up to do that. So, I guess what I'm getting at is I'd like to see is some kind of internal support from the Trustee Council to foster actual integration between these groups where we say, we have data, is it adequate, how can we change some of our collection (indiscernible), and then actually do some work with the people with the expertise, working together, rather than just handing data files back and forth to achieve the goals of answering the important questions.

DR. SPIES: That's an excellent point, and certainly we would welcome your suggestions and those of others of practical and mechanistic ways that can be achieved by managing these in a different manner, just separately funded studies, because I'm not assured from my experience in this whole process that that will necessarily come about just be funding them and having both of them write a detailed project description that we'll coordinate.

DR. IRONS: I'd just like to make the point that I think the oceanography by Stan's project is key to our project and several others. In some ways I don't mind it being in SEA, but in other ways it doesn't belong to SEA more than anyone else. We all need that data to survive, and I agree, we've got to have the coordination and everything else going, but it's key (indiscernible) climatic changes, well that's information we need to build on, especially the oceanography data which is key to this success of this project.

DR. SPIES: There's the data aspect, and there's some coordination of some intellectual concepts and various things like working together in hydroacoustics, they've got the same problems. You got what are those targets that you're seeing really represent. That's totally almost the same problem in both.

DR. IRONS: In hydroacoustics we have our own component; in oceanography we don't have our own component. We're completely dependent on (indiscernible).

DR. HANEY: I hate to burst the bubble here, but I really feel that the people who are studying apex predators need to have a certain amount of flexibility and adaptability that is not necessarily coincident or concordant with the other projects, and it would be very simplistic to believe that all the results from the SEA project are just going to work themselves up the trophic chain, be immediately beneficial to the people studying marine mammals and birds. In fact, the history of marine invertebrate research is fraught with opportunistic sorts of things that are necessarily designed to answer the questions they are most interested in. In the case of acoustics, the acoustic sampling design as it was related to us yesterday for the SEA program was, in the spring, focusing on salmon. Well, that might be of interest to the seabirds and marine mammal folks, but they really want to know what's going on at the peak of the breeding season during incubation and chick rearing when seabirds' energetic demands are at their peak. So they need ship time then, not three months earlier. They also need to have the flexibility to go after the forage fish species that they don't yet know are critical, and it may in fact vary. It may be capelin at some times or pollock or herring another time. So, I wanted to sort of speak on their behalf and say that they really need their (indiscernible). It's not a trivial or redundant sort of project in that sense. I was impressed with Bruce Wright's comments yesterday about how they have essentially built in some -- is it 10 days per sampling window of flexibility for small scale?

UNIDENTIFIED VOICE: In the core time period.

DR. HANEY: Yeah, in their core time frame, they basically have already a period in which they can focus on a hot spot they may happen to find. So I really want to underscore that this is an unprecedented opportunity to look at the ecosystem interactions from the top down, and that's certainly a trendy area in other kinds of ecosystem research, and I think it has some relevance here. The thing to keep in mind is that many of these apex consumers do share similar prey base and they have all suffered some kind of damage. So I believe that the justification, the rationale behind this is quite strong, and we need to give them this flexibility.

DR. SPIES: Excellent comment. Kathy was raising her hand, and then George.

MS. FROST: Just a quick historical comment, and, again, I'll say I support what Chris says, and this forage fish connection gets to be a hard one to sell sometimes. It's expensive and it's nebulous. We didn't sell it in '75; we're sitting here in '95 wishing we had the data. It was easy to convince people to go out and do a murre study, a kittiwake study, and a harbor seal study, and there were a lot of people back then that were trying to get some money spent on these non-commercial, sort of nebulous species, and maybe our thinking wasn't advanced then, but we failed miserably, not because we didn't raise our hands and say you need this information, but because it was hard to get and fuzzy. It's still hard to get and still fuzzy and it's still expensive, but let's not in 20 more years be here again saying we wish we had forage fish data.

DR. SPIES: I think, just to comment very briefly on Chris's point, it is going to be very obvious that separate platforms are needed out there at the height of the -- at the chief provisioning time for these populations of seabirds, and the question is to what extent does that offer opportunities for the SEA program.

Let's see, George was next.

DR. ROSE: Before this point is lost, I'd just like to make a couple of additional comments on the last little dialogue or whatever it was. I don't think anyone would ever suggest that the sampling would be the same for these two programs. I don't think that was really on. As I mentioned in my opening remarks, I think that what is on the table in terms of sampling already is too much, and the idea that you could then do more with that is ludicrous. But I think that where the cross-fertilization has to take place is in terms of, first of all, in the equipment that is used, that there's room for sharing there, the technologies that are going to be developed -- because these aren't just going to be used, these are going to be developed technologies in terms of the storage of data and analysis of data -- and also in terms of some fundamental acoustic research that needs to be done here, and that goes through right through the process of calibrations, the identifications of the targets being seen, confirmation of the target strengths of your targets, and so on, all of these fundamental acoustic properties. This is where the collaboration has to take place, and of course because it's all within the same system, it's a natural, and I just couldn't see this being done any other way, and you're not going to have two projects doing this independently. That would be ridiculous. So that's where I see the cross links being very strong between these two things. Certainly not in the (indiscernible) -- not at all. You might use the other data and there may be opportunities, but that would not be the fundamental focus of it. Of course, the forage people, as I understand, have a suitable vessel available and they have to have that.

DR. SPIES: I think Bob Loeffler had his hand up.

MR. BOB LOEFFLER: I have the advantage of a little bit of ignorance with respect to this project. But one of the things I keep hearing is the forage fish-seabird project, I'm kind of curious specifically what seabirds you are looking at, just because I've heard murres, murrelets, pigeon guillemots, puffins, kittiwakes, and I've heard mumblings about three or four other species. So the question is, and I'm sure there's an obvious answer and I'm just ignorant of this, specifically which seabirds have been in decline that you're answering the question about, and if the assemblage is nine or ten different seabirds, is that too many? I mean, if you are going to jump into a project this size flaws, so to speak, is it better to start off slightly more limited?

DR. IRONS: There were just two seabirds, guillemots and kittiwakes, and puffins are used as an alternative in the future perhaps, cheaper method of gaining information on available fish out there. All the other ones aren't up as part of the '95 seabird-forage fish project.

DR. SPIES: Dave Salmon, do you still have a question or comment?

MR. SALMON: Actually, I think George Rose pretty much said it, but actually I just wanted to say again that meetings like this foster opportunities to communicate where, for example, the SEA program is not only targeting the status and movement, there are components of that project that will be able to deal (indiscernible), so I wasn't advocating at all that these guys (indiscernible) . . . .

DR. SPIES: Let me ask the SEA people right now, are they satisfied with the plans for late season cruises that you fully integrated to the extent that you need to, maybe meet some additional needs that you have or utilize the platforms that are there?

DR. COONEY: Well, I guess, I could maybe kick it off by saying, no, I'm not totally satisfied because we haven't seen the hard details of what's going to happen. We've heard plans, and that's all well and good, and the intentions are there to do the integration. I think we have some work to do to assure ourselves.

DR. SPIES: When you're doing your DPD's, maybe this would be the time to direct . . .

DR. COONEY: Well, I realize that. Just getting the thing wired up internally in a project as complex as ours in this single, integrated DPD is and will continue to be a very large project. I suspect that some inclinations about how it might work between the projects might emerge this year, but I wouldn't want to promise that my huge DPD would show detailed integration with forage fish beyond some general level this year.

DR. SPIES: There may be some flexibility for integrating a little bit later in your DPD's (indiscernible) and so forth.

DR. WRIGHT: Well, Bob, some of that happened in there. I know that (indiscernible) some SEA boats/cruise have collected some of the data that they needed. That all happened right before -- about six weeks before the cruise (indiscernible) field, and we had space available and we made space available for SEA research. So some of that has happened, and I think all the researchers are proposing more of that (indiscernible).

DR. SPIES: Why don't we move on then to the . . .

DR. SENNER: Excuse me, Bob, I'm not done. I've just got three quick items, Bob, that maybe I can make as comments and we can move on. One is just to give some recognition to the people who have been putting this together in that we, as Kathy mentioned, that we have been waving our hands about forage fish going back at least to restoration in 1990, and we always ran up against very quickly a barrier that the expenditures involved simply weren't justifiable under the litigation scenario that we were in, and that was -- I understand those decisions -- and we have an opportunity now to redress some of that, and I think they've made good progress to putting together a package that is worthwhile. That's one comment.

Two is, I continue to have some cautions about expansions into the Gulf of Alaska, not on scientific or ecological grounds because I think everyone pretty quickly agrees that ultimately that is the context in which a lot of what is happening is played out, but I do have some concern on it on budgetary grounds, in that we are looking at a project on the order of a million dollars or a little bit more. That may or may not be adequate to even properly address some of the questions now laid out with respect to Prince William

Sound, and then to take that same pot of money, which isn't going to get any bigger, and to then add to it or dilute it by expanding out into the Gulf, to me at least poses the possibility of spreading an inadequate amount of money even more thinly. So, that's a concern.

The last one is on the common murres, which are not a part of the forage fish package, and I guess they are not slated for any money to monitor productivity in 1995, and my understanding is or perception is that we may only be one season away from having the data to declare that, at least in terms of productivity, this is a bird that has met the restoration objective or returning to pre-spill productivity. I think that this process needs victories, if you will, when they are properly justified, and I just would encourage the Chief Scientist and Trustee staff to look at how much additional money is required is to get '95 productivity monitoring on the murre. Bearing in mind that I believe that there is some independent funding available as seed money for that effort.

DR. SPIES: Thank you. We'll take a ten minute break.

(Break)

DR. SPIES: Let's then continue. Hopefully, we can go through the SEA Program, the rating programs on pink salmon and herring in perhaps an hour, and then Menace Riedel, one of the presenters on the subsistence part of the workshop has to be somewhere by 12:30 so we are going to try to get her in before lunch. So, Ted Cooney . . .

DR. COONEY: The Prince William Sound system investigation of pink salmon and herring production failures is comprised of really two major groups of studies, the ecosystem studies that have come to be known as the Sound Ecosystem Assessment -- SEA -- and then a variety of single specie inquiries that look genetics, diseases, heritable damage and some specific tools to deal with questions about pink salmon and herring. So, I'll break my remarks about these species down, sort of that way, as I talk about this program, and I think I can get through it fairly quickly because I'm going to take a more general than specific tack to date. Eric was at our sessions and he compiled more pages of notes than the SEA plan was, so I mean it's very detailed and a lot of the answers will be there.

We were asked to deal with a series of questions. We tried to do that, and let's tick those off as quickly as possible. The status of the resources -- I think we can safely say that herring is unrecovered and the predictions are dismal. Kind of an ugly picture for herring. For pink salmon generically or generally the specie is unrecovered, but we have some optimism now, particularly on the even-year brood line. The returns this year were relatively strong, and the natal habitat work seems to suggest that the controls and the experimentals are closely rapidly. So, there may be some hope for pink salmon recovery much faster than herring recovery. I don't want to oversimplify this, however, and that sort of brings us to the next category of trying to understand what the criteria might be for recovery, and the notion that healthy pre-spill levels, the peer reviewers were kindly enough to point out several problems that we get into. The data this year from the pink salmon work suggests that you've got to be a little bit careful about just tracking a single line, a point estimate as Chris mentioned. The information within region variability could be as important or more so with regard to the recovery as a single lumped estimate of this is where pink salmon in Prince William Sound are now in terms of this year, and this is where herring are. We need to pay a little more attention to what is going on within the region. The question about what is healthy pre-spill. Here are pink salmon wild stocks in about 8,000,000 fish, and then they are coming up to something on the order of 20,000,000 fish and then there is, not a precipitous decline but a sort of start-stop decline, some of which is associated with the spill and post spill. How do we interpret healthy pre-spill? (Pointing to illustrations) Is it here? Is it here? I don't have those kinds of answers, but I guess I challenge the resource people in those areas to be prudent in terms of looking at this now, revisiting the question of criteria just to make sure that we're not focused entirely on, say, standing stock. It's possible that health has recovered, but for reasons of caring capacity in the system, the standing stocks are not going to be as high, and so we ought to know about that. So, I guess, enough said about that part of it.

Past accomplishments: That's essentially implications. It's with some trepidation that I show the cone diagram again here, as it's been affectionately called. I do this because it has a misspelling out here. This is the worst nightmare for college professors is to have this happen at a big meeting or even with their students. It's kind of like going back and finding that your thesis has been in the library for ten years, but you never noticed that the results section was bound upside down or backwards! (Laughter) And let me tell you, that happens. Yeah -- so nobody's read it! The other point of trepidation here is that this kind of stuff goes from a "Cooney-gram" at a meeting to something cast in stone down the line. So, Cooney got up and talked and looks like SEA is over in 1998 -- need more money. So, with those as preface statements, the (indiscernible) ecosystem approach has tried to think about how it will phase through time, and we tried to think about it in terms of the deliverables of the milestones, if you were, that we hope to produce. I guess we're looking at, for at least the pink salmon part of the story, of sort of a phase 1 that includes a real blitzkrieg on SEA process related studies: how is the system functioning, and what are the critical wiring up and what are the relationships that are most dominant in terms of forcing predation on or away from early life stages of pink salmon, herring, and other zero-class fishes. And we know that we've just started emphasis on herring, so herring information is lagging a little bit, so the first couple of years that are emphasizing process studies, that envelope may have to shift another year down into the future as well if we are able to capture the essence of what is going on with herring. But overall, the idea here is that once one gets after and explains to some extent what the major process and mechanisms are regulating the mortality of this fish early in their life history, then some simulations, some numerical models can be produced, and as long as we start at some of the real simple, one-dimensional things, modeling goes from conceptualizations in your head to formal numerical statements, and a lot of us do the thinking-about-it stage but don't turn it into the DFQ's that we all had problems with in calculus, well, we're lucky that we've got some people here that handle that with no problem at all. We're going to be talking to those people. Everybody in the SEA program is a modeler in the sense that you get dealing with Vince and the others who will actually be developing the numerical model. So, in a way, one of the milestones is to get the major processes, regulating pink salmon and herring mortality early in their life history, established through a blitzkrieg -- it really is a massive effort of looking at a lot of things through a couple or two or three years and enough authenticity so we can pull this story together. That then, should phase out slowly with time. Whether 1998 is the end point or not remains problematic. If we catch this in the reality of the day, see what was in the field last April, we don't have all of those results yet; we're staring down the barrel of 1995, and soon around the corner will come "please respond to 1996." So in the reality of that, these can only be best educated guesses, I think, about how to phase this thing. We're recommending that any EVOS restoration really follows the guidelines of what may happen to the harbored species and other things in the ecosystem as a result of manipulations mostly at the top down, and in order to do that we need to interact between the data sets to see modeling and then a long-term program of perhaps monitoring. So, we see SEA going, generally speaking, from process oriented to more modeling and monitoring as it phases through this period of time 1994-98. The five years is not just sort of picked out of the air as kind of listening to the grapevine, well, what will the process stand; we are looking at oceanographic cycles that occurred at roughly those lengths of time, and we know, for instance, that the latest El Nino is going to prolong and progress this event. As I understand it, they are still having strong atmospheric forcing associated with El Nino events as we speak today, and the fall-back of five to seven years has been delayed in the last few years of more continuous El Nino event. We are hoping that we may be able to capture some of the essence of the system if a return to more normal El Nino conditions occur. So, five years is part of that as well. These huge programs that look bottom-up, middle-out, and top-down require that kind of time as well to capture again the essence of what Mother Nature gives us every year has been an experiment, and we hope that we have conceived of and are delivering of a field program that captures the outcomes of each year's experiment by Nature. So, with regard to the ecosystem research, this is a tentative, at best, guess about how we're phasing through time related with, with a shift in the emphasis of the studies.

The other studies, single species inquiries that we talked about in our sessions, involved genetics, questions of discrete populations and examining the effects of the environment on these discrete populations, or the effects of diseases or heritable damage, etc. There is a knowledge gap about how many populations or sub-populations that we are dealing with in this region, and that is being addressed. The whole business of disease, particularly with herring, was started last year. The big buzz word that's come out is ichthyophonis. The whole business of heritable damage, aleo time series, experimental approach to determining whether heritable damage is exactly heritable and can be proven experimentally. These kinds of programs that are in place now or soon will be in place all have sort of a time history of their own. Eric has captured that in his voluminous notes. I won't do any more than say that I didn't hear any of the investigators coming forward saying, well, you can have all my money, I'm through. On the other hand, (laughter) I didn't hear anybody say that we're going to retire on this project. So, there certainly is sense that that milestones and phasing of these projects is of concern and people are dealing with that.

A couple of projects that fall into the category of tools: coded wire tagging and otolith marking. For reasons essentially of cross-calibration and understanding the new program of marking hatchery, and maybe in some cases wild stock, fry populations -- the ear bone -- marking that -- provides a tremendous tool for maybe in the near future opening, with the hope of some success, the whole campaign on wild and hatchery interactions. There has been a lot of handwaving and talking about that, and until all the hatchery fish can be marked and identified from wild fish, you can't do that. Now, the possibility is coming on line. For a long time, we've had the coded wire tags in the system, and so the relationship between what information we are getting from coded wire tags in terms of stock separation of adults, management of the fishery, that has to be checked against what we can get with otoliths. So, there will be some overlap there. I think maybe one or two years of coded wire tagging, and then the coded wire tags may drop out of the picture. The otolith thing has a big capital cost up front, got to get heaters at the hatcheries to warm the water, and so there's problems there that are initially high, but essentially should go away if this equipment is at all reasonable and hangs in there. There will be programs, of course, that the tag lab is going to have to turn itself into a otolith lab, and that may not be a trivial matter. There's going to be some of that, I suspect, but, again, these are projects that have a defined life period, and nobody is looking essentially at going on forever with that, at least EVOS studies. I guess that raises another question that -- the work that we're doing at the ecosystem level and with the related studies all provide opportunities for matching funds if we're clever enough to do that and have enough time to investigate that opportunity, and I think we need to think about that. One fo the things that I found at this meeting that was interesting was this sort of hungry nature of everybody swarming around in a feeding frenzy whenever new information that didn't have anything to do with the agenda that we were here trying to develop. But a cool idea came down, and, wow, everybody is excited about that, and it's the nature of these kinds of gatherings, and those ideas generate some thoughts that may not be in the center of the target for EVOS, but could be followed by augmenting the projects with some additional funding that would allow the investigator to look at peripheral matters that might be as important.

So, at that level of detail and just a final word about reality and the notion that scientist (indiscernible) of our understanding of these systems comes through a comprehensive and a careful and thoughtful implementation of studies, and that this takes time, and we beg the Council's forbearance in terms of -- at times we refuse to get put in a box, it's because we don't really know at this stage of the game how far down the line it will take to get some of these results. We can only put in place the machinery that hopefully will provide the information, and we hope that we are tracking -- and the peer reviewers that have come and contributed in the meetings -- important questions have challenged us and put us on the spot is a good reflection of the scientific method. This is how it works in real science. We are applying it here. It's difficult not to get defensive at times when some questions come down the line, but we expect them to come, and that's how science works, and so we welcome the peer reviewers and their comments. There was a question about integration, I hope that we will integrate more than just at the level of let's get the acoustics working properly between forage fish and the SEA program. There is a level of integration that occurs that's an exchanging of ideas that's very important, and there has been kind of a problem in the past with wondering if Cooney gave his data to Irons, would maybe Irons publish that and maybe Cooney would show up in the acknowledgement section and T. Coney (laughter) or something like that. (Laughter) Well, we're going to solve that kind of thing, I think, by up front discussing publications, joint authored sorts of things, and hopefully that kind of level of integration, that science exchange level will continue to keep the fires burning at that conceptualization. It's easy to get locked in on lake, river, prey switching kind of thing. At some levels we've still got to keep the feelers out for making sure we don't miss a big signal when we are focused in this area something is going on there. And I think the way that happens best is to mind the interdisciplinary aspects of the whole thing and talking with David and this group, talking with the people at the molecular level of these populations, and getting involved with the oceanographers is a way to do that. So, I'll conclude there, Bob, and stand for questions.

DR. SPIES: Thank you, Ted. Phil Mundy.

DR. MUNDY: I'll leave the caning of the SEA program to the other reviewers here. (Laughter) That's too easy. (Laughter) I want to focus my comments towards the salmon and herring people and associated studies that are chasing the causes for pink and herring failures particularly, but there is something in these comments, I think, for other people who are working with animal assemblages.

It's time to get serious about measuring recovery. It's time to get serious about how we deal with these recovery issues because they are not straightforward. We can't just go out there and count the number of pink salmon that show up and try to divine in that whether or not the resource is recovered. If we learned anything from our studies in the past six years, it has been that it is complex, there are a lot of interacting factors, and we need to understand how these things work and what the relative importance of these factors might be. So, in measuring recovery of the pink salmon, for example, we've got probably the most significant effects are sub-lethal, and looking we're learning more and more, again as a result of the restoration studies and the damage studies about how these sub-lethal effects may work. One thing that hit me right between the eyes was the finding that short-term exposure of salmon eggs can lead to reduced growth in juveniles without further exposure. Now, that's an incredible piece of information, and that comes from a laboratory study. Now, when we went out into the environment, our measurement tools weren't good enough to show us this in the environment. When we looked at adult salmon abundance in the oiled pink salmon streams, again our measurement tools were too crude or too imprecise, however accurate they were, to permit us to see these things. So, we need to really hone in on how we are going to measure recovery in pink salmon populations, and we are going to have to string together a number of different disciplines to do this. One other point that is really important here is the extent to which pink salmon populations in Prince

William Sound are density dependent or density independent. The model that we have used in the past is that the abundances in the various life history stages are relatively independent on their impact of the survivals of subsequent life history stages, and that's the model that is out there. Now, this involves the oceanographers, as well as the single species people. We need to really focus on that because if you cannot measure recovery, then people are going to ask how you ever knew it was damaged in the first place -- and that's not over. So, I think that homing in on the recovery, how we measure the recovery of the animals is really very important.

That brings me to the herring, and where I'm less familiar with the specifics and where the damages are that I have studied are less clear to me, but I do think that you have catastrophic decline in population sizes, coupled with what we have learned recently about the pathology in the animals, could be put together in a very interesting concept of how the herring populations may have been damaged. Perhaps not in the ways we may have thought coming into this, because my ideas about oil spills and what oil does to wild animal populations has been totally changed by my involvement with this process. I've been told a lot of things coming into this that haven't panned out. So, my mind is totally open, and I think that the herring is going to be one of the toughest nuts to crack, and that's where everybody is going to have to get together in developing that concept of herring recovery.

DR. SPIES: George, do you have comments?

DR. ROSE: I've got three points here that I'd like to talk about. One is the one thing I think is missing from the work plan, this is sort of a reference to SEA, but maybe a little bit broader in overall context, is that we don't seem to have anybody looking at the ecosystem as a combination of all of its parts. I think that it's hard to define what an ecosystem is, but I think it's pretty clear to most people who thought about this that it isn't just a simple summing up of the parts that comprise it. Even when we study things, for example, the SEA pink salmon and herring components, we are only looking at a couple of things. But if we look across the broad spectrum of work that is being done under the EVOS program, there is a lot of different things being done, but there isn't a project, and to my knowledge there isn't a scientist whose job it is to try to integrate those results to look at the broader picture. It may be possible, for example, that through some sort of integrated approach -- now, I don't have one here to offer, I don't have the magic bullet -- but it may be possible through some sort of integrative approach to come up with an index on the state of the ecosystem that may actually mean something. It may also be possible with reference to the herring that you have some sort of keystone species whose state in the system will indicate something about not just about the health or whatever of that species, but about the whole ecosystem. So, I think that there's a whole here that maybe we should think about plugging. So, that's one point.

The other point, as Ted mentioned, was talking about integration. We talked about this already, some sort of vertical integration between the SEA program and the forage fish and birds and mammals people. But I'm not yet convinced that anything has been done about this. So far only smoke, no fire. So, we're looking for some fire here to actually really integrate these things. I think that this meeting, in my opinion anyway, has been really good for that, and we've seen a lot of talk and a lot of discussion between these groups, and I hope that that will keep on, and I hope that a mechanism can be found, with the help of the Trustees possibly, to formalize this, so that those types of interactions won't just become irregular, well, let's talk about this and then I've forgotten about it five minutes later. We don't want that to happen. So that's the second point in terms of integration of this work.

And the last one, I guess, is on the lasting legacy concept. The way I look at this anyway is that this project now sis looking at various processes, various ecosystem processes, which we hope will lead to models which will describe those processes, which then will lead to the definition of what is required in terms of long-term monitoring, if those models are worth a damn, assuming that they are for the time being -- we'll give you the benefit of the doubt. What type of monitoring which would be cost effective could be used to drive those models to give us indices of the state or the health of the system or the health of particular species or whatever the question was, and whether we are really doing that, because I think that that should be one of the key focuses of this whole program.

DR. SPIES: George, let me quickly ask a couple of comments here. First, in relation to your first comment, do you think we're close to compilating together some sort of a conceptual ecosystem model of how things work, and particularly in the state that it's in now with the reduced herring population and from our discussion of how marine mammals, particularly humpback whales, may be interacting with that, and maybe there is a brief kind of a shadow, things emerging from the shadows here in terms of a conceptual model anyhow?

DR. ROSE: I think so. From the discussions I've listened to and participated in over the last few days, I think there's an awful lot of really good, creative, new ideas sitting right out here among these people that are just waiting to be put together in some sort of concept package like that. Not to say that they are going to be right, most of them are probably wrong. But that doesn't matter. At this point, what should be done is to bring those ideas together, even in an alternative way. I mean, somebody might feel very strongly about one particular concept or idea, and that's wonderful; somebody else may feel very strongly about another one. These two could be working together, and that's the way science progresses. So, I think, yes, you are very close to being able to do that. I'd like to see it happen.

DR. SPIES: In relation to your second comment about mechanisms for integrating programs such as SEA and forage fish, I think maybe Peter McRoy might (indiscernible) box of matches here at least that he might talk about. Would you prefer to comment, Peter?

DR. MCROY: Well, sure, I made a certain list of items for what I called the "integration mechanisms" that have been talked about this morning that came up through various discussions and points. (Indiscernible -- coughing) field study sites, that is the type of work on scene, we have a number of locations and sampling points that we work out, but I don't exactly know where the forage fish are working or where the bird people are, per se, and I'm not sure that everybody knows that where our study sites are. That one integration because of (indiscernible -- out of microphone range) reflecting data sets in the same location. That's of course one additional breadth to it. Platforms is another one of course, because we have several ships that we're going to be using, so are some other people, but who has space for additional observers or additional means to be taken from platforms there. Sampling times, as Chris brought out this morning, the sampling times may not overlap, although they do in some cases. We don't have a good schedule yet, although the components of that are here to put together a sampling program. Data is one thing, and everyone, even in the coffee conversations, in that they are looking oceanographic data and so on that's available (indiscernible -- coughing) that's available now, but the rest of it, where or when or what plans you have available to do some of the things that George has been suggesting. Common species, there's a lot of overlap in forage fish and in the other, like in

SEA some of the fish that we're looking at, there's some common species overlap, there's a methodology-technology overlap, (indiscernible -- out of microphone range) acoustics which is a really important technology that's being developed, and, of course, publications. And so, those are all integration points and mechanisms, and there's probably a few more that.

DR. SPIES: Maybe it's time that EVOS has a electronic bulletin board that people could get common information, study plans, and so forth. I'm not an expert in this area, but something like that seems -- we do have a information management component to our '95 work plan that perhaps Molly might comment on later in the day when we talk about the entire program. Further comments -- Stan?

DR. SENNER: Just a quick question now in regard to data management. Is the goal for SEA plan to have its own database that is fully accessible to everyone else or will the goal be to actively incorporate data sets, such as those coming out of forage fish, so that it is really more of a Prince William Sound data set as opposed to a SEA program data set.

DR. COONEY: I think the latter. We've described our databases open and distributed. I think it could be comprehensive among the projects, and hopefully if there's any rats' nests about how that gets done, that can be sorted out. That's the intent.

DR. SENNER: That would seem for Trustees supported projects that would be a good alternate goal, an end point.

DR. SPIES: Pete.

DR. PETERSON: I'll try this and my usual optimism and enter into the spirit of looking for issues to discuss that might need attention. (Laughter) I'll start a small argument with Phil, I agree that the work on the toxicology with pink salmon and the herring is really exciting. It seems to me though that Alex Wertheimer's project did show reduced growth of pinks in the early life phase at sites near oiling, and the growth was less than at sites outside of areas that were oiled, so that maybe there is some field validation that matches the laboratory result, but Alex can inform us on that as we go further. More to the point of the SEA project, I see a couple of problems that loom. One relates to the disappearance of Sam Sharr from Fish & Game and the, it seems to me the very real potential that as a consequence Mark Willette will be asked to do more of the standard management work that has to do with pink salmon in the system. That would be absolutely devastating to this project because of the critical role that Mark plays in this project. I think it would be in the long-term bad interests of Fish & Game and of the State, and the fact that there isn't yet a new Fish & Game representative on the Trustee Council means that there is not someone we can turn to now, and there is not a set of ears listening here and now to all of what we have been talking about here so that he or she may be aware of the significance of this, concerns me, and so I would put that as a very high priority issue here for the staff and the Trustees to be concerned with to guaranty that Mark's time and talent can continue to be devoted where it is needed in this project.

The other thing that I have concern about is the cutting edge nature of the hydracoustical data set and time frames over which that information can be available to the biological people who need. We clearly though have been seeing Mark, for example, using net data as means of assessing population sizes for various prey and predator fishes in the system, and, of course, part of the goal of hydracoustics is to provide more synoptic and more extensive coverage to use that information to assess some of these. I'm not meaning to be critical in this, in the progress of this. I'm trying to be realistic in recognizing that there are substantial issues in data interpretation that need to be solved. But I am saying that the speed at which that is moving forward is not providing quickly enough the information to modelers, to Vince and his group, and to the biologists in the process. I don't necessarily know how to solve that, I don't come her with a solution, but I do, in honesty, identify that as what appears to be a problem that needs to be addressed by the SEA group.

Then third in this mode, the SEA program has, from its inception, more or less defined the ecosystem as being topped by fish, and two fish in particular. The point is, the focus has been more of what I would have called a fisheries oceanography study, rather than as an ecosystem study. And that's fine, except that more is going to be asked of the SEA data set as these other programs come on line, and we've heard a great deal of comment about that today, so I am really just emphasizing it again, but it means that some of the designs are going to have to be reconsidered in the context of what other data needs will come from other programs. A point obviously the physical oceanography program is going to serve, and I would argue probably the phytoplankton and zooplankton programs will tend to serve any broad reconstruction of the Prince William Sound ecosystem, if we can define that as an ecosystem, and so that those elements need to recognize that they are going to play a broader service function, and they may dictate a little bit of reconsideration about the designs and the programs that are being conducted. I think probably West for the physics, because that is already being done at a variety of scale, including the most broad and including fine, but it is something I think that needs to be addressed. We already spoke here about trying to put, George mentioned it, to put the entire ecosystem together, and I welcome Harold Springer on the scene, for example, as one who has experience with trying to do that. Tom Fine (ph) has the tools to help those reconstructions, and if you view all the projects that we discussed here and take the assumption that they are going to go forward in some form, that's not necessarily true I realize, but if we take that assumption, we have a fantastic amount of data across all the main levels of this ecosystem to put something integrative together, and I agree, it's probably worth thinking about how actually to do that and to try to solicit some sort of project from someone who has that as the goal. But a lot of that is going to be based upon the SEA program, and if such a project exists, that person is going to need to move across programs and make certain that all the projects, especially those in SEA, are going to serve that integration. Those are sort of the main criticisms that jump to mind.

DR. SPIES: Thank you, Pete. Do we have any other comments. Jeep, are you stretching or --?

DR. RICE: (Indiscernible -- out of range of microphone). . . . The second comment I have is that the Auke Bay Lab and (indiscernible) and the one we're dealing with here (remainder indiscernible -- out of microphone range) . . .

DR. SPIES: Thanks, Jeep. I think you brought up a key concept that we might discuss a little bit later from a process, a strategic point of view of role that cost-sharing might play in this process. I think that all of the factors being equal, it would be a positive viewed by the Trustee, but at the same time I think that it would provide justification for those agencies that really want ecosystem research integrated in a serious way -- justification for the life of those programs after the life of this process is completed or is narrowed down.

DR. RICE: Yeah, you're right in the sense that that program is more valuable than the SEA program, and the SEA program is more valuable than (indiscernible -- out of microphone range.)

DR. SPIES: Exactly. Jeff Short.

DR. JEFF SHORT: I just have one quick comment on the peer reviewers. (Remainder indiscernible -- out of microphone range)

DR. MUNDY: Well, this is great. I think that's a very good tactic actually, Jeff, because Pete has already set upon me, so you've got the peer reviewers. It's my job, coming from the fisheries background, to never let -- I think that to the extent that we can afford it, yes, Jeff, we should look at the processes. I mean there is no question in my mind about that. But coming from a fisheries background, I can't let you forget who is paying for all of this and why, and that is, what you call apex predators, other people call fisheries resources, and you can't forget the end users. You can't forget the end users; you can't forget the people who use these resources. They depend on them, not just for commercial purposes, but for subsistence and other purposes. They are culturally extremely important resources, so I think that is sort of the dichotomy that I see. It's our need to understand the oceanographic processes, it's also our need to serve constituencies of people of our fellow human beings. He also pointed out to me the need for me to clarify my comments a little bit about the pink salmon. I didn't mean to tell you that we haven't production in growth of juvenile pink salmon in the environment, because we did. The National Marine Fisheries Services was successful in doing that. However, we saw embryo mortalities in 1989, 1990, 1991, 1992, and 1993, and that's what really turned my thinking about the effects of the oil spill on wild animal populations. It's persistent. We were not successful in measuring production in growth of wild pink salmon in the marine environment beyond the year after the oil spill. So, we couldn't match -- we knew it in the laboratory, we've seen a correspondence between embryo mortality and production and growth of juveniles, and we know the production and growth of juveniles is significant in terms of survival in the marine environment. So, the fact that we had early in history the oil spill investigation, the fact that we had negative results was interpreted to mean something, to be definitive. Of course, the science is not getting the result that it is not indicative of anything. The fact that we didn't see the reduction in growth in the marine environment -- we couldn't measure it, that is -- does not mean that it was not occurring. Now, we have some evidence. If you put the laboratory studies together with the field studies, then I think you begin to get to where you need to be in terms of measuring recovery, and I think that's key here. So we have to put the laboratory studies with the field studies and not let the negative results we've got in the past in terms of measuring abundance at various life history stages detract from the purpose of trying to measure recovery.

DR. SPIES: And growth reductions measured in '89 were on hatchery-raised fish so they were unexposed to the oil period.

DR. MUNDY: No, they were also measured. There were two projects. One was a Fish & Game project which looked at hatchery fish, tagged fish. But there was also a project which was headed by (indiscernible) Hortheimer (ph) that measured production and growth on wild --

DR. SPIES: That's correct. Right.

DR. MUNDY: . . . wild populations, but as I recall that was just the first year of the investigation. The second year you got a negative result.

DR. SPIES: The studies that Auke Bay are showing is that the exposure during the egg development is carrying over to production and juvenile growth and not exposed by the hatchery stage.

DR. MUNDY. Right.

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DR. COONEY: Those of us who are toiling in the trenches would like to think that we have carefully considered long-term programs moving ahead and describe some of these things, and I guess a little bit nervous when the people at the front table are maybe suggesting major course changes or sexy things that could be done. Yes, we can be building on our research, and I hope that that will happen and it certainly will. I'm just saying a word of caution about how careful and comprehensive research proceeds. That we don't want to be jerked about when we haven't been able to establish what we tried to do. So, it's a little bit of a dilemma. I'm enthusiastic about, for instance, the state of the system -- George's idea. I don't know that there's anything in the literature that supports a notion of that at all. I mean, it sounds good and maybe there is something, and perhaps we can mine some of that information out, but I'm just wondering if this is exactly the time to start worrying about that.

DR. HANEY: Well, I have some more comments about peer review schizophrenia. We're certainly concerned about upping the ante or change. I mean, that's something that I try to keep in mind constantly. It might be useful to conceptualize this as this stock portfolio. You want to invest wisely initially, but conditions change. You get new knowledge, and sometimes you want to redistribute your resources. So, at least in my mind, I guess I perceive it that way as being more of a subtle readjustment rather than a mid-course correction.

DR. SPIES: I think Jeff's point is well taken though. We don't want to necessarily get blown hither and yon intellectually and find out when we look back if we'd just stuck with that, with this particular part of it for four or five years we'd be a lot better off now, because I've seen programs in which things have moved, in contamination studies for instance, and all of a sudden you don't have the long-term record that you might have benefited from. One more quick comment, I think there was also Jim Seeb, then I'd like to move on to subsistence before the lunch break.

DR. HANEY: Just a quick come-back to Ted's comment. Ted, I don't think anyone, certainly not myself, is suggesting any major changes in the research strategy at all. So, don't misunderstand my intent there. It's just that it seems that there could be something else on top of all of this. I mean, the comment was not directed particularly either at you or at SEA (indiscernible) all those projects. But there is no intent in which you have expressed as far as massive redirection at all -- because it wouldn't come from me.

DR. SPIES: As long as the hydracoustics works. (Laughter) Let's move on to the subsistence presentations, and we have Menace Reidel from Cordova, followed by Martha Vlasoff to talk about the results in that section.

MS. MENACE RIEDEL: I'd like to pass out some historical photographs of my family, my heritage, to scientists, and also some of my tangible products from my culture and cultural activities. My name is Menace Riedel, and I'm one of the 485 members of the Native Village of Eyak tribal council located in Cordova. I was raised by my maternal grandparents in a total subsistence lifestyle. I am a subsistence user, the mother of two children, and I earn a living making traditional garments. My outline today includes covering those questions from the work session yesterday, my personal comments and recommendations, my village leaders' comments, and the summary of the first three questions. We are going to do this presentation in two parts. Martha will take care of the last three questions.

The comments from my Village leader are as Native people we always relied on being able to survive off the resources of the land and sea. Now we have a fear that we have lost everything because of the oil spill damage. Also, we are more at risk to losing those resources we have depended upon for thousands of years due to the impact of the vast awareness of our subsistence grounds from the media and the oil spill workers. I'd like to share some of my personal views on subsistence, and first of all I'd like to say that I consider indigenous peoples at the top of the food chain, inseparable from the ecosystem. Among the Native communities subsistence has been interpreted as traditional and customary use of all resources. When I'm involved in customary activities, I am pulled into a large network of related events. For example, when I buy seal skin from a hunter -- actually, my cousin's husband -- that seal meat is delivered to his lifelong friend to feed his family, the precious and most delicate part of that animal is delivered to my elderly uncle whom I visit. When I deliver that portion of the meat, I visit with him and he relates stories to my son and I about his experiences hunting, which include geographical history of Prince William Sound. I include my children in the processing of the pelt that I acquired, including the teaching of spiritual connection and respect for the animal. I also teach my daughter the traditional techniques of turning the seal skin into a useful, warm, and beautiful product -- the mittens -- which we all could use right now. It's really cold out. I have been challenged in working with the group to strengthen the link between the researchers and the Native community. We need to be part of the process as equals, to say what we want when talking about our lives and our children. We have more to lose than any other group, and given that it should be standard procedure from here on out that principal investigators budget for and include traditional knowledge when making decisions which will impact customary use of resources in the ecosystem.

Moving on to the questions that came up yesterday, a group attempted to answer No. 1, what is subsistence? I mean, excuse me, is subsistence recovered, recovering, not recovered or is the recovery unknown? And it was the consensus of the group that subsistence resources are somewhere between recovery unknown and not recovered. No. 2, the second question, was, how will we know when it has recovered? And in dealing with question two, the group reviewed the recovery objectives listed in the Restoration Plan, page 55, which states that "subsistence will have recovered when injured resources used for subsistence are healthy and productive and exist at pre-spill levels and when people are confident that the resources are safe to eat. We are all concerned about that -- being confident. We went round and around with that one. The indication that recovery has occurred is when the cultural values provided by gathering, preparing and sharing food are re-integrated into community life. While the group agreed these were good objectives, they also felt two others needed to added. The first one deals with the concern over what people see as a lost generation. The goal here would be to see when that the younger generation has had the opportunity to learn subsistence skills first hand. We agree we cannot separate subsistence practices from cultural heritage. This is why projects like the Nuchuk (ph) spirit camp and development of community cultural centers is so important to us, because it will give us the opportunity to teach and pass on these skills.

The second item to be added to that list was when people no longer have to put in more effort to harvest the same amount of food. For example, due to the decline of the harbor seals, the hunters have to travel further and expend more and energy to collect a pre-spill amount of resources. After I get through with the questions, I'd like to add a couple more comments on that, but we'll move on to No. 3, and that was, what did we learn in 1994? There is now a greater dependency on a cash economy because of the decline and availability of subsistence resources. At the same time, jobs have become harder to find because of uncertainty in the fishing industry. The cycle of yearly activities has now been disrupted for more than five years. Subsistence is not just food on the table. The activities are what binds the community together. Their loss is felt throughout community life. One of the phrases from an elder was "March month is a smorgasbord or potlatch." When the herring came, everything else follows. The loss of herring in the cycle affects everything else in the food chain, and that affects the attitude of the whole community. The loss of subsistence sharing affects relationships throughout the region and self image becomes a casualty. One of the recommendations to address the loss issue is to hold some healing seminars in the villages.

I'd just like to talk a little bit about integration that you've been talking about, the way that all the information should overlap. I was at the marine mammal meeting with Kathy earlier in December, and I see a lot of opportunity for the Native community to work with Kathy's group, gathering and supplying biologicals samples in the future, and I'd like to restate that there is a lot of value to traditional knowledge, and we can work with her group in that area. The archeological group, I see there's a lot of opportunity for traditional knowledge there. But it is so broad, those are just a few of the things I'd like to touch on. At this time, I'd like to thank you for the opportunity to listen to our summary, and I'd like to introduce Martha Vlasoff, who will be going over the last three questions. Martha.

MS. MARTHA VLASOFF: Last year there was no separate work group for subsistence, and I was the only Native person invited from any of the communities to participate in the meeting last year of the church meeting that was held last year, and I tried to bring the local concerns to the resource category of archaeology, and I wrote a little sign on my notes last year "woman from Tatitlek has to use artifacts to club scientists over the head to stress the importance of subsistence use" in this whole scheme of subsistence restoration. I am so glad to see so many Native representatives in our work session yesterday. I really makes me feel a lot better, and I'm glad we had so many comments. I'd like to share with you some of them. The things that our group saw as important changes that need to take place to help restoration of subsistence resources are that we need to see subsistence users full partners in the restoration and research efforts. There should be a direct communication link between scientists, villages or community members, and this can include more hiring of subsistence users in monitoring and research projects. I know most of you heard me say the same thing last year at the church meeting when I was representing Native communities, but in that process I tried to fight for and track the proposals submitted through DNR to achieve more involvement of local people who have used the natural resources in Prince William Sound for generations and have more understanding of their ecosystem, just as a matter of survival, than any high tech research group can hope to attain. That was project number 95052. As the proposal sifted through the review process, I made the recommendation that the coordinator be a Native person from the region. It should be noted here that Menace and I are not hired by an agency who pays our salaries for working on these projects. We are subsistence users concerned about the preservation of our customary and traditional natural resources in Prince William Sound. Every time we spend six months developing a project idea, it is absorbed into another agency budget. This is not involvement of communities; this is support for agencies. If this trend is not reversed and these direct lines of communication are not developed with communities, their participation and contribution to research may be stymied. We need to see a change in the way subsistence service is seen as a separate research conducted through the EVOS process. We use the whole ecosystem. We want to know what is being learned about the effects of the oil spill from each species being studied, whether it is deemed recovering or not recovering. Like John Christianson, chairman of Chugach Alaska Corporation said yesterday, we are the top of the food chain. We realize that because of litigation there was separation between the effects on the resources and the effects on the compensation to subsistence resource users last year, but we believe without considering the interaction between the human use and research being conducted to analyze species-specific data, you will never be able to produce an accurate conclusion to the questions being asked by the EVOS Trustee Council about restoration of injured resources. Although there were projects funded last year through money set aside to help restore subsistence in the villages, we feel that there was an inadequate amount of time to prepare project proposals. One leader made a comment that he thought that sometimes the process seems set up to fail. In an effort to change that we would recommend that the project funds set aside last year to help villages and communities to prepare proposals for the '95 work plan, that's Project No. 95428, be used to bring subsistence representatives to Anchorage as a regionwide, consensus-building work session before the April deadline for proposals. That way we can consolidate our priorities for restoration of natural resources for the oil spill affected communities instead of competing with each other for project funds. We need to have a cultural anthropologist to do peer review of subsistence projects. Yesterday, we didn't have any peer review person assigned to our work group, and everyone else did, as far as I know, so we could really use some help from a cultural anthropologist on that. We feel there is a need to collect hard numbers which more accurately reflect the time and effort people spend per unit harvested. There were many comments yesterday that the Fish & Game surveys may show that the resources gathered are back to pre-spill levels for some species, but they don't reflect how much more effort it takes to obtain these resources now. There are still fears that the resources are unsafe to eat, but I believe that through a more direct partnership with research scientists, the people in the communities could have a clear understanding of the environment and hopefully that will help dispel at least some of those fears. We need to have subsistence representatives on each research group, such as the marine mammal group or nearshore group, who will be an equal partner in those exchanges, who can make recommendations to the scientists and to relate to them our concerns as subsistence users, and there should be a Native subsistence representative who sits on the EVOS Science Review Board as there is now on the Public Advisory Group. There is a need to educate the public, especially the lawyers -- this is a comment one of the village council presidents made -- the Trustee Councils themselves and their staff, about the importance of subsistence to our way of life through village site visits. So, come out to the villages more. I've suggested that before but everyone has always said how hard it is to get out to the villages. And I know Molly and her staff have made those efforts, but we need to see more of that exchange.

One recommendation is to quit buying real estate. Don't spend so much money on habitat acquisition. And the reason that was stated was some of the subsistence users are fearful as far as what habitat acquisition will have on their use of subsistence resources, and that is a major concern. I'm not saying that everyone in the regions are saying that, but this is one of the concerns.

As far as how long it will take to see subsistence recover, we don't think our generation will live to see it. That is why it is so important to have projects like the spirit camp at Nuchuk to teach the new generations subsistence skills. As far as the cost of restoration, we believe that the cost of not restoring subsistence resources will be the loss of our way of life. This is just the beginning though. Establishment of equal partnership and an increased appreciation for and incorporation of traditional knowledge into the scientific research will benefit everyone. We appreciate efforts accomplished so far to include subsistence users in the EVOS process, especially the task force that was sent to develop project proposals last spring in the villages. We recognize the concern and sincere attempt to integrate subsistence issues by Molly McCammon and Rita Miraglia, but we need an even greater participation of our own Native organizations in the process of developing partnerships and collaborations. Perhaps we can try to address how these partnerships will be formed when we conduct the '96 work plan proposal writing work session in March. I would just like to encourage you to be thinking about how we can form those partnerships and collaborations with the scientists and subsistence users. Thank you.

DR. SPIES: Thank you very much, Menace, Martha. I think some very good points were made. Are there any comments from the review board? George.

DR. ROSE: Yes. I would just like to thank Menace and Martha for covering some excellent ground there. I agree with almost everything you say, and I think there are some very compelling reasons why these types of interactions should be done. Some of them are for moral and ethical reasons than legal reasons, and things like that, but it goes far beyond that. A lot of it has to do with scientist reasons and management reasons. I personally don't believe that science or management of any natural resources can be successful without the cooperative involvement of users, and I would apply that to almost any system. We have terrible problems in our fisheries all over the world and -- whichever you want to talk about -- because that doesn't exist, and the trend should be, and it isn't always the case, but the trend should be towards resolution of those problems, and it will almost inevitably result in a much better management of the natural resources, and also much more interesting science that can be done because you have a much broader sampling base there, or the possibility of a much broader sampling base. The other point that I would like to emphasize is the potential importance of all the traditional knowledge. In my experience with the fisheries, traditional knowledge is usually right, and it's about time that science got off its high horse and started listening to them more.

DR. SPIES: Thank you, George. Phil.

DR. MUNDY: I also echo what George said, and thank my good Lord for your comments. We don't have any cultural anthropologists up here, and that is shortcoming. Early in my career, I worked in parts of Alaska where the subsistence uses of the resource are at least as important as the cache uses of the resource, and I'm currently working for Native Americans who are trying to maintain their cultures, putting their resources on the endangered species list, so I'm certainly sensitive to those concerns. Also, I would like to echo what George said about traditional knowledge because in working in the Yukon and Kuskokwim area, typically checking in with the local people is one of the most useful tools that I had in learning how to manage the fisheries in those areas, so traditional knowledge is definitely something that I take into account, and I think ought to be taken into account when doing fisheries management type activities.

DR. SPIES: There are different ways of looking at the oil spill process, and one that I have heard is that there are a lot of white, middle class people in Anchorage making good livings off this, and often the money goes to them first and then it dribbles out from there into the region, and I think we have to address that problem. We have to do better in that area.

MS. FROST: Bob, this is something that the Trustee Council staff may have to take an active role in though. I'm an adviser for an Alaska Native science commission that is being created, and this came up as an issue. There is a real desire and an interest in local people being hired. The fact of life is many of us here work for federal or state agencies that have extremely rigid hiring procedures. I couldn't hire a Tatitlek resident if I wanted to under the State hiring system, without probably a six to nine month process of the registers, college degrees and detailed applications. It's an inflexible system. Something that the Trustee Council could perhaps work on at the staff is some sort of a more flexible way to hire people and then loan them out for different projects, or somehow get around the bureaucratic red tape, because no matter how well intentioned people are, it is very, very difficult to do, and some people just can't do it. Some people don't try to do it, but some well intentioned can't do it, and we've got to work with our agencies, collectively, to get over these barriers.

DR. IRONS: U.S. Fish & Wildlife Service are developing local hire out in the villages (indiscernible). I'm not sure how it works (indiscernible) Anchorage, but that could be explored.

DR. SPIES: (Indiscernible) priority for (indiscernible) adjusting the policy too much or there might be a priority for . . .

MS. FROST: Everybody.

DR. SPIES: For the EVOS staff to maybe push this to front burner. It's been talked about a lot, and we want to be in the business of doing good service for this thing that you're doing, actually doing something about it. Your point is well taken. Martha?

MS. VLASOFF: I was just thinking about couldn't we make proposals to supplement the local hire issues (out of microphone range -- indiscernible) the Native organizations (indiscernible).

DR. SPIES: I think that's one way. We might find a way to fund a separate labor pool, aside from separate projects, maybe through an RP process. I'm just talking off the top of my head here. I'm not an administrator, and I don't know the ins and outs of these issues, but something like that might be -- yes, sir, can I get your first name?

DR. A. SATHY NAIDU: Yes. My name is Sathy Naidu, and I've been working in the Arctic for several years, for five years, and I have quite strong associations with the Natives up there, and I serve on the (indiscernible) Science Council (indiscernible -- extraneous noise). One thing that we have (indiscernible) that we are concerned about and the most important thing is to educate the Natives, and we realize that. It is very important to educate the Natives and young people, and they can go back to their community and let them know what is going on. One way that we are meeting this problem is that NSF has funded a Native internship at the University of Alaska Fairbanks at the Institute of Marine Science. The idea is to take interns into the (indiscernible) and the scientists serve as mentors, and these students are given a stipend and (indiscernible) and various communities within the state, and that is what I would like to see augmented in this Trustees -- if that program -- the Institute of Marine Science -- it would be supplemented to accommodate local interns from Cordova and other places (indiscernible).

DR. SPIES: Thank you. Joe.

DR. JOE SULLIVAN: I just like to comment that that education cuts both ways in that on the one hand -- I heard what you say -- but I also think -- we discussed this a little bit at the meeting yesterday is to have more PI's visit the villages and talk with them about their results because it not only educates the people in the villages as to what the PI's are finding, it also educates the PI's relative to not only what they are particularly interested in but it also gives them an idea of what subsistence is about anyway. I think a lot of us don't really have a clear idea of what that is. I appreciate the fact that we do have a subsistence section at this meeting, and I appreciate the comments made here yesterday and the day before that. Really, we need as much education about subsistence as (indiscernible) does.

DR. SPIES: Thank you. Are there any other comments from the audience. Yes. I'm sorry.

MS. JOEY SEITZ: (Indiscernible -- out of microphone range)

DR. SPIES: I have David Scheel here, and he has developed a proposal for looking at the abundance of octopi and critons as addressing a concern that was raised on last year's workshop with those resources. They slipped through the cracks in some sense in that they weren't covered by the intertidal/subtidal studies per se. They are a little more difficult to count (indiscernible), but Dave has developed a proposal in this area, and I think he has planned in fact to utilize Native people and local people in the area. Perhaps you could comment.

MR. DAVID SCHEEL: Yes, just briefly, I made an opportunistic visit to Tatitlek and Chenega about that project last year. As soon as the paperwork is taken care of for the funding, My plan is to travel to Tatitlek and (indiscernible) offices in

Cordova (indiscernible) to talk to the residents about their use of octopus and to tell them what I am doing and to ask them what areas in particular they might recommend that I locate that work and hopefully to (indiscernible) opportunities there (indiscernible) that project. So, that is what is in the plans right now, and the paperwork is to get the funding going (indiscernible).

DR. SPIES: So administratively, the fact that you are with the science center, perhaps, presents less of a constriction on that.

MR. SCHEEL: (Mr. Scheel is out of range of the microphone for the most part) I (indiscernible) speaking a little bit more about that (indiscernible) Forest Service. The science center doesn't have a lot of difficulty to process to hiring personnel from one of the villages or communities of the Sound. They have -- the proposal now is going to peer review and (indiscernible) status of the (indiscernible).

DR. SPIES: I have been informed that we must adhere to the original schedule because the Sheraton Hotel has other groups coming in later, so we do have to be out of these meeting rooms by three o'clock. I still think we have sufficient time to reach logical conclusions of the workshop, so I suggest we take come back at 1:00 and that will give us two hours to carry things on.

* * *

DR. SPIES: . . . as the review session leader for our recreational, presenting a summary of recreation.

MS. VERONICA GILBERT: We had a very short review session on recreation. It may not be on the top of your list, but it certainly falls into the area of human use that we just discussed before lunch, and the group did revisit the status of injury, and because had been no damage assessment studies of recreation and no ongoing monitoring, we had no hard data about how recreation has changed. It was strictly anecdotal. However, the sense from the people at the meeting, which consisted of a number of people from the local communities and members of the Public Advisory Group, was that there is a marked increase in recreation use, sport fishing, tourism, in the spill area. However, that is not documented, and furthermore, given that we are almost six years since the spill, it is difficult to know the extent to which the increase resulted from simply increased publicity about the spill area or maybe the value of the dollar affected it or it may be a number of things that just in general affected recreation and attracted people to the area, but the sense of the recreation users get and certainly the people from the communities in the Sound is that there is a marked increase, although we can't document the reason. With them we revisited the recovery objectives. The objectives that we have are threefold: recreation will recover when the natural resources on which it depends have recovered. In other words, when wildlife sighting and sport fishing returns to the way it was before the spill. No disagreement on that. The other is when use of oiled beaches is no longer impaired by the presence of oil. Everyone agreed with that. The area of disagreement and where we would want a small change has to do with the part of the recovery objective that states that facilities and management capabilities accommodate changes in human use. The concern here was that the changes in human use that we would be looking at in terms of expanding capabilities to any extent or constructing facilities be very, very narrowly tied to the major impacts from the spill. The needs are almost insatiable under the best of circumstances, and we really have a desire to narrow those down. We then turned to projects, and because no recreation projects were funded for either '94 or '95, although certain projects like shoreline assessment and shoreline cleanup, various efforts of that sort, do affect recreation, the major project that has been going on in '94 that I think you should be aware of that we did discuss was the Alaska marine recreational project, which is funded by the State restitution funds. It is not funded by the joint trust funds. And this is an amount of money from the State restitutioin fund of slightly more than 10 million dollars that was set aside for restoration, and that fund can be used for recreation facilities, as well as acquisition of the lands for those facilities. That project has developed to the point that soon there should be decisions on that project, and 107 proposals were submitted out of which 65 were eligible. The goal is to complete these projects, so these would be recreation facilities -- largely these are facilities -- funded by State restitution funds and that should be completed -- most of them should be completed over the next three years. If nothing else, keep that in mind in terms of how that might affect the rest of your projects. The idea is to reserve about one million dollars to anticipate future needs. Having discussed all those, we then turned to any changes in strategies or suggestions for the future. One suggestion that I think can be easily accommodated is that in the invitation it would be useful to have better defined legal parameters for potential proposals and reviewers. The pattern, when it comes to any proposal for recreation, most of which are generated from local communities, is that the proposal comes out and says we are interested in doing this, we feel we have a need. We then begin dissecting it: well, it is for recreation? No, it's not exactly recreation; it's a little subsistence thrown in there. The fact is, this is something from the local community that deals with human use. Furthermore, our typical way of handling this is to put it in the category where there are legal and policy concerns, and there it stays. The suggestion was to really have much better parameters, so people can develop thoughtful proposals and so that reviewers have some idea of what they are looking for.

The final thing I have to say was actually the most important recommendation that came out of this meeting. Once we had allocated nine million dollars to facilities, then we were able to move onto other things, and the other things that we moved onto was a sense that the Trustee Council needs to be involved, engaged in some way in management of human use in the spill area -- recreation management, I'll call it that -- that addresses increases in use, conflicts among users, trespass, and also protection of injured resources, with a particular interest in directing people away from archeological sites as an injured resource in which communities are particularly interested. The reason this is difficult even to articulate is that, as you know, the joint trust funds can only be used for activities on public land, dealing with public resources, and these increased uses, trespass etc., span public as well as private land. Furthermore, the Native corporations which own lands, as well as state and federal agencies, Trustee agencies by and large, already manage lands. They have their own system for land use planning that should actually address all of these issues. However, I do believe, after considering this recommendation from the people who attended this session, I do feel that their is a role for the Trustee Council on this, and it mainly has to do with sharing of knowledge with the land managers, both private and public, and sharing much as the scientists have shared at this conference, but sharing with the poor schmo who has to issue a tideman's lease somewhere and may not really be in tune with some of the restoration concerns we may have or some of the conclusions from the scientific studies that may say to them, you know, we've really got a problem here, this is an area we need to protect. We may not establish a sanctuary, but there is a lot of stuff you can do to protect this area. And I do think in the arena of sharing knowledge or sharing concerns with agencies, private as well as public, in cooperation with communities, we can at least make some contribution to improving the knowledge that is used that is reflected in the existing management plans, and I think some that can be handled through the Project '089, some of it through Project '052, but I think between now and March we need to give a little more thought to how to deal with this issue, how we can constructively engage in resource management and management that affects human use of the area. Now, that is difficult to express. I'm finished, and Jim Richardson is in the audience.

DR. SPIES: Thank you, Veronica, for that able comment. And we've had another reviewer join us -- Jim Richardson, who has helped us in the recreational area. So I'll introduce Jim, who is now standing. He's going to make a few comments, I take it.

MR. JIM RICHARDSON: Thanks very much. Phil Mundy made a comment earlier this morning that there's some risk of credibility if you can't measure recovery, and people might ask how you measured damage in the first place, and in recreation's case neither one of those occurred, so everybody keeps running into the same block. But with that said, I think there's a number of general themes and data needs that we can mention that the group talked about and that have been ongoing for some time. There is a common theme of concern over secondary human-induced impacts. We have information from a number of different sources that recreational use has increased, but it's not hard data. It's anecdotal, and in the group session on Wednesday there were comments about the Katmai and the Kenai Fiords areas that have increased. It's extremely difficult and may not be worth the effort to try and go through the research job of trying to assess that increase to that caused by general tourism and population growth and that caused by publicity associated with the oil spill, and a recent project I've been working on, taking a look at an EIS for the Whittier Access Tunnel for general tourism, and I believe that that would be a difficult task to try and accomplish that and may not be useful. The fact is that we have increased use, and we're going to have impact from that -- and two types of impact. One is recreational user conflicts, potential conflicts between user groups in oil spill areas, and secondly, the recreational user conflicts potentially with recovery injured resources. And so there is a need for human activity management in the sense of comprehensive recreational planning. The problem with that is that is handled in the spill area by a number of different agencies, some of which are state and some of which are federal, and the Trustee Council really isn't in the business of doing land use planning or management. So, one possible approach might be for the Trustee Council to provide some sort of framework under which a comprehensive recreational planning could occur in some of the spill areas.

Another issue is that there is a need for coordination of projects impacts. In the session on Wednesday I believe they discussed a (indiscernible) project where a number of different restoration projects were proposed. The Trustee Council didn't pick up any of those in terms of funding. They may have felt they were facility-type projects or improvement projects that some representative in that might best be handled under the criminal settlement. But we need to be able to tie recreational management to those because that is sort of exacerbating a user-resource conflict. If through financing a facilities project, for example, you could increase use in a sensitive area, not knowing that that was a sensitive area, you would be causing user conflicts or slowing the restoration or recovery of an injured resource, that would be a problem. One of the things I concur with Doug (indiscernible) about is that there is a need, a very strong need, for the definition of the type of project that would be considered funded. I've sat in on meetings with different community groups or groups that have this feeling that they might like to present a plan, but they are intimidated by the fact that they know some have been thrown out and they are not exactly sure what the types of criteria are. So, getting that information out is pretty important because we're depending on different user groups, Native corporations and communities, to address some of the restoration needs, and that definition may help us move beyond the paper listing of everybody's pet project or facilities or capital improvements on to some things that the Trustees might well fund. Apparently, there are no Trustee agencies that have submitted viable projects for '94-'95 and probably won't for '96. That may indicate that we going to have to get information, if it's going to be used, it's going to have to come from other sources. I would cite an example is the concept of the Nature Conservancy is promoted and utilized in different places, so that the site stewardship where a group will decide that an area, a beach, an archaeological site, a river, is very important and will try to get a cooperative effort among the user groups to prevent increased use levels or other things from happening. For increased use levels, we're seeing where that type of approach might be appropriate. Those are the sorts of things that came to mind in my review. I think that hopefully some discussion will interest some in pursuing some of those.

DR. SPIES: Thank you, Jim, for those excellent comments. I'd like to throw it open to both the audience and the other reviewers. I think Alex Swiderski had his hand up, then we'll go to MaryAnn Bishop.

MR. ALEX SWIDERSKI: I just have one brief comment and that is that both Brock and Jim have the same misconception in that we did not conduct any damage assessment studies of recreation. We did one of sport fishing, (indiscernible) that identified approximately $30 million injury to sport fishing, I believe entirely in 1989. Then we conducted the second study of tourism. That had a number of components to it, and I don't recall for sure if one of those was a recreational tourism, it may well have been . . .

MS. GILBERT: Yes, we did.

MR. SWIDERSKI: So, those studies are public. Yes. So (indiscernible) . . .

MS. GILBERT: I could just add to that. You're absolutely right, Alex. There was some of the information dealing with lost revenues, etc., or dealing with licenses, sport fishing licenses during the year of the spill, and it did concentrate on the '89-90 period of time, and that was useful information. That was fine for as long as it went, and we've not continued to monitor.

DR. SPIES: Is there more economic base, perhaps, than recreational based? Have we got a combination of things?

MS. GILBERT: It's useful for its purpose.

DR. SPIES: MaryAnn Bishop had a question or comment.

MS. MARYANN BISHOP: Yes, I would have a comment too. I would have said that what information (indiscernible). I know the Forest Service station special use permits for cabins. All that information is readily available. I know for two summers they've done some type of survey of recreational use around the Sound, so I think there is a lot of information out there.

DR. SPIES: Yes, Jim.

MR. RICHARDSON: I have reviewed that information, and it is not particularly useful, but there are certainly logs at the cabins now, and in terms of being able to assess the changes and patterns of the typically, fully utilized, you don't see any (indiscernible). The survey provided some information , it was called a customer survey, and it provided some information but certainly not near a baseline or (indiscernible -- out of microphone range and extraneous noises) going on.

DR. SPIES: It would seem to me, and I'm not much of an expert recreationist, but it would seem to me that as much as the Trustees have tried to protect the resources by the purchase of habitat, certainly this pattern of increased use does provide some challenges for protection of natural resources in the spill area during the recovery period. So, I would think that needs to be addressed in some manner, at least some thought given to that potential impact. Bud Rice.

MR. BUD RICE: The National Parks Service does have the data from the commercial uses as well. It gets data from tour boat operators (remainder indiscernible -- out of microphone range).

DR. SPIES: Thank you for those comments and contributions and discussion of the recreation issues. It's something that's still important that we should keep in mind and not be totally focused on the science.

Let's move now on to the summary from the nearshore ecosystem group that will be provided by Jim Bodkin.

MR. JIM BODKIN: I guess I'd like to begin by thanking Molly and her staff for their support and encouragement of the fostering of this process. It's a far cry from what we had several years ago, and it's added quite a measure of reason to something that was fairly unreasonable. I would also like to thank them for their record-keeping during our working group, which will ultimately correct most of the errors that I'll probably make in the next few minutes, and finally, I'd like to thank the participants in the nearshore working group. (Aside comments from audience).

We began with this list of questions for each of these resources that you see highlighted here. As you can see they are fairly varied, going from (indiscernible -- coughing), and it was not a difficult task actually asking most of these questions from merely the status of the resource. I don't believe that we had any concurrence within the group that any of these resources should be changed from their injury status. It probably stems from a lack of information than anything else. We did discuss the need for adding resources to this list, and I think it was mainly based on information provided by Dave Irons and the Fish & Wildlife Service's boat surveys, and because I believe that at least one of these species of birds, the goldeneyes, we might consider a nearshore species, and so we discussed the addition of that, and I think there was general concurrence that in fact the goldeneyes should be supported in their addition to the injured resource lists.

We answered the questions regarding these resources in specific terms in cases and in general terms in others, and in no terms in some others, and those are part of the notes that we have, and those will be provided -- the answers to these questions. What I would like to do today in the few minutes that we have is to discuss the highlights of each of the studies that we reviewed in terms of the results from '94 and end points, but really more in terms of accomplishments. What did we gain from these projects that will be useful, and I guess that's what I chose to emphasis in this review of what we've done over the past few days. I will be begin with the sea otter monitoring project, and I guess I would consider the accomplishment in this project is basically the completion of the development of a tool that will provide us improved precision in estimating sea otter population abundances. This is a tool that has applications well into the future and well beyond the oil spill area, and I think that there is great value in that -- and that there is some positive notes in terms of the sea otters and in what we feel to be a return to the normal H-class distribution of otters that we find in the dying population. On the other hand, we still had elevated levels of juvenile mortality in sea otters in western Prince William Sound compared to eastern Prince William Sound. So, it looks like one possible interpretation is that chronic damage may be subsiding, but that we still haven't seen recovery.

The next project is a harlequin monitoring project. I guess there was actually quite a profound accomplishment within that project and that they developed again another method that can be used in monitoring harlequin duck populations, and that was the ability to discriminate juvenile age classes in male harlequin ducks. I guess that was a positive accomplishment. Kind of a negative accomplishment was that they didn't find any broods in western Prince William Sound during the survey. Then, Bob brought up a finding regarding the use of P450 within the liver samples of harlequin ducks collected in 1993, was it, Bob -- and they were all positive?

DR. SPIES: Those on the western side were significantly greater than those on the eastern side which would be consistent with oil exposure.

MR. BODKIN: Oil exposure. No comment as to effect, but certainly the exposure.

The black oystercatcher study again identified some differences between oiled and unoiled areas in terms of black oystercatcher reproductive biology. More important, I think, that the black oystercatchers were recognized as a potentially valuable experimental animal within the nearshore ecosystem for a lot of different reasons. It was recognized last year that black oystercatchers provided a potential, and no response was basically made to that identification.

The next study is entitled sediment recovery. I guess there is good news here. From what I gathered, this study is at its end point. This is completed, '94 samples that were collected are under analysis, and I guess another accomplishment here, something I found, it was quite remarkable and potentially quite valuable is that the sediment hydrocarbon data is available through Auke Bay Lab -- something I didn't know. I may be the last person on Earth that didn't know that. I thought it was a remarkable finding that this is now available to everybody, and it's a resource that has a potential great use. They do plan on, or at least are reserving the potential for monitoring within Prince William Sound in subsequent years, but for now the project is closed.

The next project we looked at was a Herring Bay experimental and monitoring studies, and some of the highlights of that study were that the demonstrative spill-wide effects on fucus populations and a reduced biomass, algo biomass in the upper and mid intertidal zones, with an enhanced biomass in the lower intertidal zones.

This project will continue into '95, and the end point is identified as 1997.

94266 -- I've already passed the ones I put in the wrong order -- 94266, the shore assessment and oil removal. I saw this again as a great accomplishment. I recall last April we discussed a resident of Chenega Village going out in their skiff and hauling up asphalt off the beaches, and now I see this year that tons of asphalt were removed from beaches in southwestern Prince William Sound, and I am going to assume it was a result of that finding from last April, but to me it represented a tremendous actual step in mitigating some of the damages and responding to some of the input we received less than a year ago. It was generally concluded that most of the asphalt was recovered from that area, and that again that they don't plan on removing any more asphalt, mainly because they've removed it all. So an end point was achieved in some respects. I understand that they are considering the development of some technologies that might allow them to remove residual oil, remaining oil other than the asphalt map that they have recovered here. There was another component to the shore assessment, and that was the oil persistence work, and that was conducted in the Gulf of Alaska, on shorelines along the Gulf of Alaska, where they looked at persistence of oil over time. What they found was subsurface oil and near-subsurface oil was still persistent in relatively large quantities, and it was related to me that it was somewhat similar to -- these were on exposed, rocky shorelines -- and the way that it was described was that the substrate was somewhat like that of a mussel bed only on a different scale, and it consisted of a large (indiscernible) vertical relief of very large boulders and rocks and that they were actually being used to hold out within the interstitial spaces between the rocks.

The mussel restoration project: Again, another accomplishment I saw here, and we heard a little bit about this, about the project where we going out and removing contaminated sediments from beneath mussel beds -- a tremendous accomplishment. These were beds that were providing a source of contamination throughout the nearshore system and that's been remedied to a large extent. Again, there was an end point achieved here in that the restoration was completed. There will be a monitoring phase that will go on to monitor the level of hydrocarbons in those sediments in the beds. There was another component to that study that looked at hydrocarbons in mussel beds in the Gulf of Alaska, Kenai Peninsula area, and they found a high proportion of the oiled shore mussel beds have hydrocarbon levels that exceeded 1000 micrograms per gram -- again, demonstrating persistence of the oil.\

Just briefly, we reviewed some earlier findings from the subtidal system, and a couple of important findings that led to the development of one of the projects that we are going to discuss next was within the subtidal area, a condition in demersal fishes, hemosiderosis was noted in fishes -- nearshore demersal fishes from the oiled areas. These fishes are important prey resources for river otters and pigeon guillemots that forage in the nearshore region. In other areas there was increased numbers of urchins that were observed in the subtidal areas within the oil spill zone in Prince William Sound.

The next step we took after reviewing the past was to look into the future and identify projects and review briefly the projects that are being proposed and considered for future work. You heard the other morning about 95025, the nearshore vertebrate predator project, and I guess at this point I'd like to go back to one of the questions in terms that we were addressing, and that's our pinpoints, and I guess I can describe this project as a means to answer the question, are these injured resources recovered? I would envision a decision-tree, in that we were taking three approaches to asking the same question -- three different ways to ask the same question. Those are to look at the characteristics of the population -- demographics, population level measures, the density of animals -- and we will ask the question, do they differ, do the density of animals differ either before or after the oil spill if we have pre-spill data, or are they similar between oiled and non-oiled areas after the spill? If we answer that question in the positive or if there are no changes, we would consider the answer in a positive light, then we can go onto the next question or next method that we are using to answer that question in terms of the individual health of the animals. So we have similar densities of the animals, and then we look at the health of the animals in these two areas and, if they are the same, that provides us with a measure of recovery. We have generally said that we need to have two years of data that would show up in the same path in order for us to say that a resource has recovered. So, in the case of river otters if you had equivalent densities and equivalent health variables between populations, and if their food resources were equivalent, we would say that recovery has occurred. So, we are looking at a two year end point for the four species that we are addressing within the nearshore vertebrate predator project, and those are river otters and pigeon guillemots and sea otters and harlequin ducks.

Another study that we discussed was the avian predation on herring spawn study with MaryAnn Bishop, and it is currently looking at the effects of avian predators on the amount of herring spawn, and I think that there's a general feeling that it is kind of a more interesting question to look at from a different perspective. In terms of the effects of that herring spawn on primarily the avian predators that are utilizing it. Tens of thousands of seabirds are moving through the areas where this herring spawn is occurring and utilizing it and then going on to nesting areas, and although it didn't appear to fit within the nearshore vertebrate predator project specifically, we thought it was an interesting, compelling story that offered some great opportunities.

There were some coastal habitat studies conducted by Ray Highsmith and Mike Steppon (ph) and I'm sure a large number of others. I don't have a great depth of understanding of those studies, but I understand that they were tremendous, and that there would be a great deal of value in revisiting those sites. So, that work is being proposed and will likely be proposed in the future, and it is primarily sampling within the intertidal regions. I understand they have a four year end point to that study. They'd like to revisit each of the sites twice over that four year period.

Finally, I understand that there is a proposal that would be submitted in the future to expand the harlequin monitoring project outside of Prince Williams Sound. It will be a joint project I assume with the National Park Service and the Fish & Wildlife Service and will include the Kenai Peninsula, the Alaska Peninsula, and Kodiak Island, and, again, I think they were talking about using some of the methods that Dan Rosenberg developed in this past year.

I think I'll stop with that.

DR. SPIES: Thank you, Jim. We've got about an hour left now, so we'll be looking at some advantages in time now, so I'll move on to the -- if any of the reviewers have questions of Jim or the nearshore group or comments to make.

DR. PETERSON: It just depends what you want to do with time management. I mean, like all of them, there are numerous comments that could be made.

DR. SPIES: I think what we were hoping to do at the end of this is to have some sort of general discussion of overarching issues, and in some ways we have touched on some of those issues this morning in various ways. So, I am comfortable we have covered much of that, but we certainly -- I'd like to have a little bit of a discussion of the nearshore thing. I think the intertidal studies are something that you are keenly aware of, as I am too. There's been a long history of those studies and they have been very valuable, there's been a definite impact, and there appears to be lack of recovery and we have to balance the great cost of doing those against the benefit and how that fits in with the whole program. So, that certainly merits some discussion, and we do yet have the fisheries enhancement issues to deal with and Alex Wertheimer and Joe Sullivan, so maybe we could just have a few comments from the audience for a few minutes, and then I'll try and move on.

DR. PETERSON: Apparently, you hit upon one of the problems that I see, and that is that the intertidal zone as a system, as a community, is clearly one that was extremely hard. I mean it's the (indiscernible) king crabs where there were substantial damages, as documented by the coastal habitat study. Those damages occurred in all three geographic areas and that project was one of those that did a better job than anything else of going outside the Sound to assess the impact. Recovery is incomplete, although recovery has been initiated, and in probably all those systems it's possible that the estuarine soft sediment intertidal zone has not begun a serious recovery, but it has certainly been initiated in the others. The program as it was originally conducted was quite expensive, and I think there is a real need to address how to efficiently return to some of those sites so as to gain additional information as to whether recovery has been completed, and if not, where not and why. We want to do that in some way that it can fit within the budget constraints, and that, I think, is a challenge that lies ahead.

I think that this issue of the herring spawn and the lack of it is a real significant one. It has community-wide implications, but it also has implications for the conservation of particular species who may use it very importantly for refueling for the migration that is about to be initiated. The surf birds and the turnstones come to mind, and of course MaryAnn has begun some study under the umbrella of the SEA program to look at the impacts of those birds on herring spawn, which is an important question, but the conservation questions related to this in analogy with the rule that (indiscernible) cram eggs play for red knots, for instance, and their migration on the East Coast, becomes, I think, a significant question, and one that ought to be considered in the future.

The nearshore predator package I think is a very interesting and important one to consider. I had questions myself, I don't think they can necessarily be resolved here, but the questions about "pigus" showing up in two of these different packages and what sort of overlap there is in that and whether that can be adequately justified and reconciled that there are two programs both looking at pigeon guillemots. I suppose that's a question.

MR. BODKIN: I think that can be easily answered by Dave and I.

DR. IRONS: Well, basically, it's one investigator doing two studies, one on each project, and it fits very well -- no overlap by the same investigator. One is spiral indicators and one is energetics. It's very efficient.

DR. SPIES: Sounds perfectly integrated to me. (Laughter). Okay, let's move on then to the fish stock separation and management issues and also the fish, shellfish, and bird enhancement replacement with Alex . . .

* * *

MR. ALEX WERTHEIMER: We paraphrased the questions on the floor over there, and the way we went about addressing these projects were: why are you doing it, what are you doing -- by describing the project -- and how has it worked to affect restoration, and how long does it need to go on? The (indiscernible) for the stock separation and management issues can be more or less generalized that increasing management resolution on heavily exploited fish populations is one of the best techniques to effect restoration of an exploited resource. It allows continued harvest of undamaged populations, while you go about the business of rebuilding and restoring damaged populations. This has been a controversial issue since the damage assessment process started because the problem with the defining where normal agency management responsibility ends and where the Trustee Council obligation for restoration begins, the Council has signed on to this as a very important component of the restoration process, but has always wanted to know how are we going to phase it out and return the level of management resolution over to the management agencies. But through this, as I think a couple of examples will show, these activities have had tremendous effect, a tremendous positive effect, in terms of minimizing the damage and subsequent results of the damage to major fisheries and major resource users within the spill area, and I think sometimes not enough credit is either given or taken by the Trustee Council in acknowledging the successes. I think it was the consensus of the group that that needs to be brought out and publicized that important fisheries have continued because of these activities.

There are really two arenas that we discussed. One is the tagging and tag recovery of salmon in Prince William Sound; the other is the suite of sockeye overescapement issues in Kodiak and Cook Inlet. So, I'll just split them apart.

Basically, the Prince William Sound activities involve two projects and really if we brought in the 320 issue, the thermal marking and tag recovery as well and consider that as part of the suite, one of them is a phase-out of an earlier study that was the recovery of tags that were placed on coho, chinook and sockeye salmon, that essentially that project has come to an end point in '94.

The other issue is the continued tagging of pink salmon in Prince William Sound or to have a management resolution that allows intensive harvest of the resource, and at the same time permits the department to have information to open and close fisheries so that they can effectively give escapement to the wild spawning streams. This project, this management technique, has allowed the department to continue high exploitation rates in the Sound on abundant stocks through a period of unprecedented variation in numbers of fish, from all-time record highs to the collapse in 1993 and bouncing back up to high returns in '94. Without it, there would either have been foregone harvest or, the worst case scenario, a serious over-exploitation of certain wild populations. So, I think that it is hard to overemphasize the importance of that management tool. There is now a time line on the tag program. You heard a little bit about it from Ted Cooney, the concept of phasing out the coded tagging, beginning thermal marking of fish, and switching over to the thermal marking system, the Trustees have committed to carry out that process with the idea that when that time line is reached then the department management agency responsible and the constituent groups will have the responsibility for continuing it. One recommendation that came out of group is that, as it is designed right now, the overlap period between coded wire tagging and otolith marking is one year. That does provide one opportunity to ground truth the otolith marking and compare with historic records exploitation rates, survival rates -- based on coded wire tagging -- and mark recovery ratios. But there was some discussion that certainly two years would be better and give a expectoment (ph), but probably more importantly, there is a risk of adverse rationale to extending that overlap to at least include tagging fish a second year. If you just think for a minute about the life history of pink salmon, if thermal marked fish are initiated with the '95 brood year, coded wire tagging in the '95 brood year, you have and overlap year in '97, then the fish return. If there are problems in terms of switching over, in terms of sampling, mark recognition, switching over manning the system from coded wire tagging to thermal marking, you do not have the opportunity to now go back and tag the '96 brood year. They are already in the water. So, a risk-adverse approach would be to go ahead and bite the bullet and dedicate some additional funding to make sure you can provide that management resolution that has been so necessary in Prince William Sound throughout the (indiscernible), if you will.

Switching over then to the sockeye overescapement, we considered two areas there. One was the development of new management techniques specifically for Cook Inlet, and here, again, it is a success story, albeit a co-methodology one, where stock separation tools have been -- using genetic stock identification -- have been developed that now are available to make in-season management decisions which are critically important if it is necessary to try and maintain escapement levels to the Kenai River, if it should meet some forecast of low productivity and still have mixed stock fisheries in the lower and middle Cook Inlet. Another aspect of that is the ability to get in-season estimates of the abundance of fish so that you can correct or either low forecast or high forecast, and there is an opportunity to do that using hydroacoustics. You can have test fisheries. Test fisheries can provide that type of information, using commercial openings as test fisheries can provide that type of information, but if you are concerned about very low escapement levels, you are not really willing to risk those test fisheries, and this hydracoustic technology provides an alternative. So those, again, there is some phase-out cost to those in '95. Those tools have been developed and now they are available for managing that very important economic, recreational, subsistence resource in Cook Inlet.

The other aspect of sockeye overescapement is the monitoring of the damage done by the overescapement to Kodiak lakes and the Kenai lakes, and the story as it was presented to us is that in the Kodiak lakes it is fairly straightforward. Red Lake is a recovering system and requires perhaps one more year of monitoring, and Lira (ph) Lake is still showing signs of damage from over escapement and density-dependent effects (indiscernible -- extraneous noise) in the lake and probably will require monitoring for an additional four year cycle.

The Kenai River system is a much more confusing story. There are certain evidence of density dependent effects in the lake as a result of overescapement. However, the return in 1994 were much better than forecast. There was not a problem, and now there has been more or less a contingency put on the level of monitoring and also the consideration of doing lake ecosystem studies based on the strength of the '95 return. So, in essence, in contrast to our discussion of pink salmon in Prince William Sound, there have been a cut-off end point criterion established for the Kenai River, which has to do with how many fish return to the system in '95. We had substantial discussion about this, and that does not mean that we understand the Kenai Lake ecosystem. There are very mixed signals and confusing results, and while there's a real good handle by limnologists and sockeye salmon biologists on clear water lake systems, systems like the Kenai Lake are poorly understood and many of the results from the overescapement monitoring research are confusing to say the least, which is one reason why nobody is quite sure how many fish are coming back. But, again, I want to emphasize that this money has not been -- because there is still this lack of understanding of these ecosystem relationships within these lakes, this money has produced some substantially important results. One thing that was pointed out to us by Dana Schmidt was the recent re-evaluation of escapement goals on the Kenai River. I'm sure everyone here is aware of how much attention is paid to sockeye returns to the Kenai River. Without the information generated by the overescapement monitoring program, there is a good chance escapement goals would have been changed considerably, much to the detriment of that resource over the long term. So, I guess I'll leave it there and see if anybody has anything to add. I think Phil Mundy might have some comments. I'll turn it over to the reviewers.

DR. MUNDY: Thanks, Alex, for those comments. I don't have a lot to add. I would point out the many accomplishment in this area, that in addition to having a genetic stock i.d. technique put in place for Cook Inlet sockeye, we also have assembled a genetic baseline in the process of doing this which I think will be of benefit to the management of the resource for many years to come. So, this is, in my view, an historic achievement to determine, so I think some follow-up studies there are definitely indicated so we can judge recovery in this case -- so that the link effects, the effects on the zooplankton, the effects on the growth and survival of the fresh water life history stage of those sockeye on Kodiak, that was the indicator there that we rely upon, and subsequently those indicators were confirmed in terms of adult returns. So, we've got the whole story, the whole picture on Kodiak came together very well. Over in Cook Inlet, the freshwater story did not come together as well. It's a much more difficult situation to study. It's glacial in Cook Inlet versus clear water in Kodiak. There were some difficulties in demonstrating freshwater lake effects. I'm not saying that there were not effects, I'm just saying that in trying to demonstrate those effects we have not come very far. We have done a lot of good science over there and made progress in understanding these lakes systems and the dynamics of productivity, but nonetheless in terms of restoration and demonstrated recovery, we have to rely on adult return data for that purpose, and so that's why the adult returns in this year and in the next calendar will be so critical to really determining the extent of recovery and extent of damages all together (indiscernible). So, in view of the hour, I'll just stop it there.

DR. SPIES: Thank you, Phil. Are there any other comments by the reviewers? If not, we can open it up to the audience comments. Okay. Thanks for your cooperation in moving the schedule along here.

and certainly something that is a major benefit of the oil spill studies. Also on the line of the stock i.d., I think -- the switching over to Prince William Sound -- the mass marking program that the Trustees have helped get going, over in Prince William Sound, is also another historic achievement. This will provide a lot of benefits for resource management in terms of the basic conservation of the resource, but also in terms of allowing harvesters access to the fish in Prince William Sound. I certainly do share the concern about the degree of overlap in terms coded wire tagging and mass marking programs. Mass marking is being developed as a production tool, it's been demonstrated in principal and demonstrated in application in other places, but when you're putting in a program like this, it is a very, very large undertaking and lots of things can go wrong. It would be unfortunate if we ran into some unanticipated difficulties in the mass marking program and we wound up not having this tool available for a year because of cropping the coded wire tagging program. So, I think that at least the application of coded wire tags should proceed a case and hold the recovery during the second year as an option.

Now, the area of sockeye overescapement, this is an area where things have gone well and not gone well from a restoration and recovery perspective. The sockeye overescapement damages on Kodiak, these were clear water systems, and because the studies were put in place, I think these were clear cut demonstration of damages. The extent of the damages is a little bit more difficult

One of the things that not all of you may be aware of, there was a series of workshops held in development of the '95 work plan, and there was quite a bit of effort devoted in October-November, maybe even September. A lot of them were fishery-based issues and our workshops were based pink salmon, sockeye salmon and herring. So, if you don't hear a lot about those important resources in terms of people getting terribly excited, a lot of the reviewers here and the people participating are those who have gone over this in some detail already.

Well, that brings us to the end of the reports from the group leaders, and I what I would like to do is (aside comments) -- my apologies, Joe, we still have your contributions here.

DR. SULLIVAN: I'll try to make them brief. Well, basically, our section looked at essentially general restoration projects of enhancement and supplementation of Nature, and it included a multitude of sins for 1994, and it will include a multitude of somewhat similar and slightly different sins for 1995. We, like the rest of you, had a couple of sessions these last couple of days on that specifically associated with this workshop, and we also had some discussions over particular projects off and on during this period, and we had, I think, a very good workshop last week in addition to the ones we're talking about in September that covered a number of the different issues. I'd like to kind of give you an idea in general of what sort of things were common among these different projects and how they may really separate out from the rest of the projects we'll be talking about later.

There are basically about three or four different kinds of restoration projects that are restoration projects that we're attempting to address -- particular injured specie or species that has no consumptive value, and that takes one sort of form and has a number of different conditions that surround that. There were restoration of stocks that do have consumptive value; there were supplementation or enhancement of stocks that were not directly injured but because they provided a service, some other member or some other portion of that population was restored or attempted to be restored and we went along with that track, and then finally there was replacement of a lost or injured service. One of the things that separates these things out from some of the rest of the projects here is that most of them require an environmental assessment. There was one project in particular that we looked at yesterday on Otter Creek cutthroat trout that demonstrated some of the aspects of environment assessment that were involved with that particular project, but I think that it is a good example of the rest of them as well in that a number of the issues that have come up, how may this bigger project impact adversely the environment or how will it help the environment or the different alternatives that need to be examined. These are the kinds of things that go into an environmental assessment and help really get to the bottom line on this. I think a number of the brief project descriptions over the past few years have raised some of these kinds of issues with the peer reviewers and Chief Scientist and the Trustees, and yet before we are able to put any of these things actually into implementation we are required to write some kind of NEPA document, like we are saying in this type of example, at least an environmental assessment is typically necessary. And I'd like to point out the fact that these things typically address the issues that are critical that the Trustee Council and the peer reviewers and Chief Scientist would have a problem with.

Now, the kinds of restoration projects, we have one that was essentially a change in predator-prey relationship. This was shooting foxes on some of the islands towards the south end or just around the edge of the oil spill. It has been shown in the past that this sort of project has increased the numbers of nesting birds by, I think, thirteenfold -- 15 to 29 times for black oystercatchers and 8 times for pigeon guillemots in certain areas -- that shooting foxes is rather an easy technique, typically can be accomplished within a two year span, doesn't take a lot of follow-up in the way of monitoring to find out whether it has been successful or not, and that monitoring actually is a key element of all of the restoration projects, of all the general restoration projects, in that while in the past many of these sorts of projects that various agencies have put on have perhaps not had that elements, these elements all do have that element. I think it's an important element, it's obviously a part of these projects, and it was a major part of one of the criteria that we set up last week that we listed off in our stock supplementation workshop. My point is, that's an element, that is a cost -- a number of projects the monitoring aspects will be borne by the agency, a number of the coho restoration projects apparently will be borne by the agencies, but in any event someone has to pay for that. The project is not complete unless you have the monitoring element that tells you whether it has succeeded or not.

Another type of project was habitat manipulation of one sort or another. We listened to a number of projects yesterday and the day before that talking about repairing state passes, changing gates on the state pass to allow only one different kind of fish through or another; we looked at adding debris, woody debris, to various streams. Most of these projects were in Prince William Sound, there were a large number of Forest Service projects that simply minor repairs to fish passes that are already in existence and are expected to have a pretty decent impact. Some of the cutthroat trout projects, for example, would limit coho perhaps from entering an area that is particularly sensitive to cutthroat trout. Looking through their EA, they were able to demonstrate a number of different alternatives that could be used to accomplish their goals, and therefore lay it out, I thought very well, for different people who would have to review the project.

Another project would increase the spawning area available to fish by, again, it was a pass manipulation, and that particular project -- this was one on Kodiak now -- Little Waterfall Creek -- but essentially increase the spawning habitat available to pink salmon and coho that would increase perhaps the number -- or estimated at least -- increase the number of coho by about eight times and about four times for pink salmon. So, if those stocks were impacted in that area, that was supposed to have an area-wide restoration value. There was also a project, Chenega chinook release, that was a hatchery project that was replacing lost subsistence foods. Because it was a terminal release there because there were no other chinook stocks in that area, the environmental impact was considered to be small enough and acceptable enough to allow this to be a replacement tool. On the other hand, we have been discussing for the past week or so another project which would go into impact restraints in Prince William Sound to look at whether or not they were in fact impacted, do some genetic studies on these things to look at fish strain and so forth, and use hatchery involvement, take egg from the streams, incubate them at the hatchery, take them back to the streams. This sort of thing would be considered supplementation and was really a large part of the workshop that we had last week. We went through this workshop and came up with a long list of criteria that they are about half way (indiscernible -- coughing) which we will then pass on to Phil Mundy and Bob Spies, allow them to digest them for awhile and make a better set of evaluation criteria for these sorts of projects that will be coming up in the future.

Coghill Lake -- lake fertilization was one other technique that was used. Make Willette and Dana Schmidt have been involved in that one. Sometimes lake fertilization works, sometimes it doesn't, but the effort that we put into it, I think, makes a great deal of difference in the amount of success you are likely to have, and they have put in a great deal of effort on that. Lake fertilization in particular, I think, we really like the idea of monitoring for several years and several more years down the road. That is not as cheap as some things because you are looking at water quality parameters, you're looking at zooplankton, phytoplankton, the number of smolts in and out, the number of adults in and out, and that particular project has some complicating features also with a stocking program in addition to lake fertilization. Nevertheless, it appears that efforts over the last few years have increased the number of smolts out to something like 35 to 36 smolts per spawning, which is quite an improvement over where that lake had been in previous years. That again is a replacement fisheries. In addition to the things that I've noted thus far for restoration projects in general, there is also the set of environmental safeguards that the Department of Fish & Game and other agencies have set up that make these types of projects for anadromous fish very difficult sometimes. It means that we have a large number of regulatory hoops that have to have been gone through, as well as NEPA documents, in order to ensure that the Trustee Council is not funding something that will be environmentally disastrous. One of the criteria that we looked at last week and a little bit this week as well was trying to get that sort of department review really before we get too far along in attempting to get Trustee Council funding for these projects. I think I'll bring it to a close at that.

DR. SPIES: Thank you, Joe. Are there comments from the reviewers? Phil.

DR. MUNDY: Just a couple of brief comments. First of all, in the area of dolly varden and cutthroat trout, both of these resources were clearly demonstrated have been damaged by the oil spill. In fact, I believe that the statistical detective work that was done in these studies was some of the finest that we had in all the damage assessment studies. However, they are no economically important species; however, they may be quite important biologically -- cutthroat trout is on the northern limit of its range in Prince William Sound -- and the habitat restoration proposals that we looked at in the session I think were good examples of how to do this kind of a restoration project, particularly on the monitoring evaluation study. The limiting factors were identified and addressed, and the follow-up work that would be needed to be done in order to determine the effects of the restoration project were clearly identified, and so I find out to be very promising and want to compliment Dan Gilligan (ph) and the Forest Service for their work in that area. I think it is important work. Then the other thing I just want to comment on is the exotic predator removal. That's the foxes, which is not my area, but also there is a situation in terms of cutthroat trout where exotic predator removal may be necessary. That's a fairly common technique in recovery and restoration of damaged species nationwide. In fact, that is usually the top of the list is getting rid of the exotic predators to give the damaged species a chance at recovery.

DR. SPIES: I might say also that the Pacific Seabird (indiscernible) has endorsed that as a primary and most effective way to restore seabird populations in the northern Gulf of Alaska. Alex.

MR. WERTHEIMER: I'd like to follow up on Phil's comments on dolly varden, cutthroat. Both of those are on our list of injured species, but the recovery is unknown at this time. There is absolutely no effort to turn recovery, and I think what Stan Senner was saying earlier, you know, it's good to come to closure on some of these issues and show that something is being accomplished. In some of this restoration work, there is some good opportunities, I think, at least in cutthroat trout of examining the question of recovery as well -- a very cost effective way, and perhaps the Council might consider a small RFP-type approach to soliciting some effort to try and move the status of those species from the unknown list to some degree -- shed some degree of light on recovery now.

DR. SPIES: That's an excellent point. I mean, from the sense of damage assessment, cutthroat trout are also a big puzzle to me because we had persistent effects on growth and no other markers of oil exposure in -- I think it was -- the second year of study. So, we noticed the pathological; it doesn't mean they weren't damaged, but it did raise questions, and we have done some restoration activities for cutthroat trout.

Okay. Are there any other comments from reviewers or the audience in this area. As an introduction to kind of wrapping this session up, I think I might ask Molly McCammon -- I believe she's got some comments to make of a general nature -- and we might take it from there after Molly's comments, in terms of overarching issues to be addressed by the workshop.

MS. MCCAMMON: Bob, we have until 3:30 here. We've been given an extension. Did the core reviewers want to do a little summation first before I kind of get into some more technical things?

DR. SPIES: Okay, so you're comments are more of a wrap-up.

MS. MCCAMMON: Yes.

DR. SPIES: I didn't have the chance to really -- okay. Okay, they're the ribbon on the package here. (Aside comments). So, I don't know if the reviewers have -- are we prepared to make any general comments -- we are springing this on you a little quickly here. (Aside comments) Why don't you go ahead, Phil.

DR. MUNDY: I didn't know whether we were going to get a chance for wrap-up or not, but there are a number of things that I want to point out. First of all, I want to emphasize the positive accomplishments of the restoration work. There are quite a few, and I couldn't begin to sit down and list all of them in the time that's available, and if I don't list your contribution, please, it doesn't mean I don't think it's important, but I think it's important for us to reflect on some of the top accomplishments that we have had, particularly in fisheries area, and so I wanted to list those. First of all, as we briefly mentioned, the genetic baseline information we've got on Cook Inlet sockeye, I think, is an incredible piece of resource management information. It's the sort of thing that probably the state government would never have been able to pay for. It's certainly a valuable piece of information, but it's not the kind of thing that routine fisheries management operation would normally be able to collect. Secondly, I would identify the wild stock tagging data on pink salmon that was done under damage assessment and under restoration. The ability to go out and study the wild stocks is extremely important, and again, we typically emphasize hatchery fish because they are easy for us to handle and cheap for us to handle, and we usually wind up with very little information on wild stock fish. So the wild stock tagging did change some of our ideas about homing and stock identification for pink salmon. It will give us some ideas to work on for some time to come. And thirdly, I'd say the oceanography of Prince William Sound, the way that that's coming together I think is going to make management in the future a lot easier. It's going to make the kind of information that we can afford to collect in fisheries management program a lot easier to understand and we'll get a lot more mileage out of it. So, I think the oceanography, particularly the physical oceanographers, always have it easier than anybody else because they deal with things that don't move around and aren't perverse like salmon. But they are quite far ahead in giving us the kind of information that we need to understand -- water masses, water movements -- in relation to fisheries management. Again, this is not the kind of thing that a fisheries management operation can normally afford to collect, so I think we're most fortunate to have these.

Now, after the message of hope, there's a warning for all the studies, and that is, take care to partition the biological and environmental variability. That is, take care to try to distinguish between the variability that you measure with regard to its biological or its environmental source. The reason that I bring this up is that this is a study that is focused on commercially important apex species, stock structures critical to understanding salmon and herring and also pollack too, although pollack is not one we normally focus on. I would like to look back at the North Sea herring work that has now been going on for over 100 years, and note that that was very important when they were trying to tie oceanographic factors and fisheries production factors together in a coherent story.

Lastly, I'd like to echo Bob Spies' call for vertical integration -- vertical and horizontal integration -- that is, putting the hypotheses together and coordinating the work. I don't think it's unreasonable to ask the studies whether they are able to use the same sampling platforms or not, whether they are able to use the same hydracoustic surveys or not, but it's not unreasonable to ask them to use the same hypotheses, and that is what I'm saying. They should be able to link your work in some way to the central ruling hypotheses that are being developed, principally in the SEA program but also elsewhere. So, that is I think the key to making progress in these things.

DR. SPIES: Thank you very much, Phil. (Aside comments) George.

DR. ROSE: Yes, I'll take a little different view from Phil, not that I disagree with what he says, I don't. I do agree with just about everything he says, but I'd like to just emphasize the collaborative nature of this project and the excitement that that should bring to the research. I really think that the best things that will come out of this, we don't even know yet, and they are going to come from the collaboration, some that are taking place right now and have taken place, and some that are yet to happen, and I'm sure will happen if we can keep the momentum of this project going. In order to do that, I would hope that a more formal mechanism could be put in place to keep this kind of synergy going. It won't happen from my experience of these kind of projects; it won't happen unless it's forced. People will normally tend to drift apart unless there is some sort of cohesive force to keep them glued together. So, I don't want to mention any particular projects, although as Phil has mentioned there have been some notable accomplishments and some very exciting accomplishments by anybody's standards already in this project, but I think the best is yet to come, and it's going to come through an increase in the collaboration and greater ties between the projects, and that kind of synergy is really, I think, where excitement of the future is, and I hope that between you as the PI's and the researchers and the Trustee Council and so on, we can somehow formalize that and keep that going, so you don't all go back -- keep these workshops as a regular part of what you do, because I believe very strongly that that is where the best will come.

DR. SPIES: Thank you, I think you're right on. Stan.

DR. SENNER: I want to make just a couple of comments about the process and money, and that is that the process now that is no longer driven by the litigation is certainly a breath of fresh air, and several of you have commented on that, and it's actually fun to be part of it. You know the rooms aren't filled with quite the tension and frustration that there once was, but I think it's important to also remind ourselves that, although we are absent the litigation, that we still have a legal framework laid down by the settlement, there has to be the tie to restoration objectives, and that we can't forget that. In fact, we forget it at our own peril because, one, there are those legal requirements; two, the Trustees have very hard decisions to make about how much money to allocate for research and monitoring, and if they don't see a program that is responsive to those restoration objectives, that will come back and haunt this effort. I don't say that because I see big problems there, but from time to time I detected some things have stronger ties than other things to those restoration objectives, and we just need to remember that.

Lastly, to put this into a bigger context, we are in a period or we're starting a period at the national level and in many state governments where there is going to be less money for science, and some of the people who will be fostering that are not only interested in reducing the size of the federal government's budget, but they are also not interested in science. There's active hostility to science, and it really -- we are in a situation where a year from now the National Biological Service, recently renamed, may not exist as it is today, but also the functions that it now carries out may simply be zeroed out of the federal budget. So that's a very real context sort of swirling around in the bigger picture, and it makes it to me all the more important to make sure that this program remains firmly grounded in the restoration objectives, which surely do mean good science and the kind of ecosystem approaches that we're taking, but we have to be able to explain it to our mother, to our congress person, to Newt Gingrich, whoever it might be, and that becomes the ultimate test, and if we can't do it, everything else will be jeopardized.

DR. SPIES: I think quite appropriate comments, Stan. Do you have any comments to make, Chris?

DR. HANEY: I would challenge all of the researchers to use a process described some 30 years ago now by an (indiscernible) whose name I believe was Platt or Pratt called "Strong Inference in Science." And basically the idea is to use (tape malfunction) case yesterday. But if herring are important, then such and such will happen. And then you go from that statement to another one, and if that's true then the next state is true, and you keep going through that process until finally something is falsified, and believe me it will be falsified eventually. I would also encourage you to look for common patterns across widely divergent groups in trophic conditions in the animals' communities that you study. Look for areas where damage is persisting among groups and look at places where damage is becoming less and less -- where there is evidence of restoration. I think if that kind of broad scope is maintained, the science will be better, and you really will have a much stronger likelihood of contributing to an ecosystem level answer.

DR. SPIES: Thank you, Chris. Pete, do you have anything further to say about this?

DR. PETERSON: One wise old owl among us pointed out that by my earlier abuses I lost my right to make summary comments now, and if that's the case, the comments I made earlier about the bridge project that I was talking about really do apply to all of these, and I harken back to those at this point and simply won't repeat them. I think we are at an exciting time here. Stan reflected properly by saying that the tensions are off, the opportunities are here, and I think the groups are working well together towards very exciting and what will be very useful ends, and more specific comments I'll enter later when appropriate.

DR. SPIES: Thank you, Pete. I'm not sure I can add much to that except to make a general observation that we required the principal investigators to come to this and we have established that this is part of the process, and we all knew we had to go through this workshop, but I got a sense -- a real basic and genuine sense that people thought this was a worthwhile process, and it's very reassuring, and I think we're really on an evolutionary path here that's extremely useful and productive.

This sense of genuine excitement in a number of different areas, and there's a real sense of (indiscernible) to flesh out some real basic knowledge about the natural resources in the spill. I'm excited about it and I think I sense that excitement among many of you. I want to thank you for your participation. I have really enjoyed this workshop. I think it's been very productive and useful. We do have until 3:30 -- this room -- but it's an absolute imperative that there's no trace of us or our belongings is here by 4 o'clock. (Aside comments omitted). So, we do have a little bit more time than we thought, so I might ask Molly McCammon to make closing comments.

MS. McCAMMON: I think the last item on the agenda was developing a long-term restoration program in the 1996 work plan, and what you see on the overhead here is our draft timeline for the FY 96 work plan. Eric reminded me that the ghost of Jim Ayers still lives because I didn't put a draft on this timeline, so I'm penalized a dollar, and I'll have to find Jim to pay him my dollar. Jim's view is that if you have "draft" on anything, you can never really get in trouble as long as it's still a draft. If you notice "draft" floating around a lot in all of our documents, that's why. If you look at the timeline though, starting Monday the staff goes to work really hard. We basically have about 5-6 weeks to develop the invitation for the 96 work plan, and as part of that invitation in the same document, we will be putting together the long-range view, the long-term plan for the restoration program. As part of that, we will be working with all of these various work groups that developed reports and documents over the last four days, we will be working further with individuals who have been kind of coordinating these various efforts -- we will be laying all of this down so that when we go out to the public with the invitation approximately March 15th, they will see the context in which we are proposing work to be done in '96. Since we kind of compressing two schedules here in one, we're doing some long-range planning as well as doing the invitations for specific projects, when we go out with the invitation for project proposals on March 15th, we are also going to go out for public review of the long-range direction. So, March and April will be a number of workshops, public meetings, public review of what we're kind of putting out as the trial balloon for the long-range plan. As part of the Restoration Plan, the Trustee Council made a commitment to report to the public on an annual basis about the status of the resources, what they are spending the money on, what's happening, how they're involving the public. That was done last year in the form of an annual report that came out at about the time of the spill anniversary. We will also be using that forum this year also, so we'll also have an annual report coming out about the third week of March.

So, we kind of go down the timeline, approximately May 1st project proposals are due. So, be aware. This is it. And the reason you asked for it is that we are really committed this year to avoid interim funding, splitting up the fiscal year. In order to that, the Trustee Council has to take action on the work plan by the last week of August. In order to get this done, in order to allow enough time for peer review and public comment, proposals have to be in by May 1st. So, this is a really compressed time. It gives you approximately three months to think about this. After peer review, the project proposals, in consultation with the Chief Scientist, the core reviewers, the restoration work force will be developing draft funding recommendations for FY 96. Those will go out to the public for comment and review. This is a very compressed timeline here in that it doesn't include a lot of steps. It doesn't include the involvement of the public advisory committee, it doesn't include the Trustee Council review of these things. So, this is kind of at a gross level of what we're predicting for this coming year.

Now, the science program often talks about this adaptive management process and how every year you come in and review what you've learned this year, modify based on the results, and then figure out what you should do next. The management process of the whole EVOS process is also a form of adaptive management. Probably a lot of you think we're just kind of wishy-washy and can't figure out what the heck we're doing, and to some extent I think that's probably true. When the spill happened and in the early damage phase went through and then the settlement came about, there was manual, there was no quick book that you could go to and get the recipe for how you were going to put this process together. It is an evolutionary process, and we are constantly learning as we go. I hope we are modifying the process to respond to public input, the input of all the people involved like yourselves, and to reflect as we go along a better process. But there are a few things that I think have come up over the course of the four days that I wanted to mention specifically.

First of all, I think the Trustees, with the adoption of the Restoration Plan as I mentioned on Day 1, with the Restoration Plan behind them they are committed to looking at multi-year funding, and I think they've been doing, kind of ad hoc, in the past few years just by the fact of funding certain projects. This time they want to see it much more up front, they want to see the contexts the projects have to each other, they want to see what the end point is, they want to see what they are buying for three years worth of work or two years worth of work. Let me emphasize though that they also know the uncertainties in the scientific world. They know the uncertainties of the natural world. They are not inflexible. They know that you cannot predict completely what you're going to do or what you're going to find out within that three year process.

Secondly, this year we are committed to funding for the entire fiscal year. I think this will be a benefit to all of you doing budgets in that you won't have to do two budgets, an interim budget and a remaining budget. You'll just have to do one budget for that fiscal year. As part of this commitment to multi-year funding, that's the good news. The bad news for some I think is that there probably will be a little bit more oversight. I think for most people -- I don't think this will be a problem. I think the budget instructions -- we're going to work with people this year to try to make sure that our budget instructions are clear, more detailed, up front in the interests of getting economic efficiencies here. It's really hard after a budget has been developed for us to come back and start nitpicking it and just saying, oh, do you really need a computer again, and just kind of doing that whole scene. We're going to do a better job of giving instructions up front, and we are really going to insist that you look for ways for cost-sharing in order to get the most out of our research dollars. In addition, the peer reviewers here -- we've gone back and forth over this idea of whether to require a brief project description up front or a detailed project description, and in some cases a brief project description makes sense, and a lot of cases it doesn't. We will be working with the reviewers to figure out what the appropriate level of detail is. But if you are coming in with a request for three years of funding, I think you can expect that you're going to have to provide a far greater level of detail in order to get that kind of approval than perhaps there's been in the past. I think though that all of us have evolved sufficiently that we have a much better understanding of the kind of information that the reviewers have required and need in order to make a really worthwhile, productive review.

As part of management of this process, the Trustees have asked for greater oversight in the sense of a greater comfort level on their part that projects are tracking, that they're actually on schedule, that there is a schedule, that somebody knows what that schedule is, that if they want to find it out they know where to go. A year ago when I first started at the Restoration Office in Anchorage, there wasn't even one single list of all the projects that the Trustees had funded. Over the past year, we have developed that list, and in addition we have developed a quarterly reporting process that actually is tracking where those projects are, not only as reports, but also the peer review process, and as we go we're going to be adding more detailed tracking in terms of are you reaching your quarterly or bi-annually -- or whatever every six months is -- milestones and objectives, so that there is some way of figuring out that these projects are moving along. In some ways, I've heard complaints from people in the past about this kind of oversight, the fact that we ask questions and want to know how is the money being spent, is the project on schedule. And I think I need to emphasize here that the EVOS Trustee Council is not the NSF granting agency, we're not a private entity, we are a public trust, and the Trustees take their trust responsibilities very seriously. They are very conscientious about the fact that they need to respond to the public about how that money is being spent. They want to be assured that it is being spent wisely, they want to be assured that someone knows what's happening with all of those projects, and as such I think we're not asking for the kind of information that we're been asking just for fun, and we're not asking it to make your life difficult, although you probably think that, but we really are trying to ensure that there is proper management and fiscal controls. We are getting ready this year to have an audit of all of the expenditures for the last three years.

This will be done on both the federal and state sides. We've been spending the last six months getting all of the books in order to get that audit ready. We are trying to be responsive to the GAO audit of a year and a half ago in terms of better management practices. The only thing that I think you should know though is that we're flexible and receptive to your comments and your suggestions on how to streamline the process and on how to make it better too. So, it's not that when we kind of figure out what direction we want to go in terms of reporting requirements, we try to include as many people as possible so that it's going to be responsive to your needs, and if it's not, you need to tell us so that we can figure out how best to accommodate that.

One of the issues that I heard a lot of people say today is the need for more integration, more coordination. By going to a four-staff that the Trustees have done in the last year, the talents of our staff are primarily in the realm of planning and communications. We have relied on the Chief Scientist, on the peer reviewers, and of all of you to give us the advice on the science part of it. One of the things though that we're doing in the next month is to hire a science coordinator who will be on staff in the Anchorage office. This Alaska presence will be working closely with the Chief Scientist to implement the science program and the restoration program and, of course, he will also be tasked with providing that kind of staff support that so many of you have requested in terms of trying to integrate the various projects, not only within the project, between projects, but also the kind of integration that we need within the whole program. I think having that presence on the staff will be of great benefit in this next year.

Another issue that I feel compelled to talk about is the issue of competition. When the Trustees adopted the Restoration Plan in November, they adopted a policy that to the greatest maximum extent possible we would try to make the process an open, competitive process, and this has been, in a large degree, to respond to concern that this was kind of a closed, in-house -- you know -- agencies just kind of at the trough, kind of feeding their own budgets, padding-their-own-budgets kind of perception that's been out there. And we have experimented in the past year with a couple of different processes to open that up. We've used the state's two-step RFQ-RFP process on an experimental basis, we've used the federal government's broad agency announcement process through NOAA, which actually worked quite successfully this year, and we will be looking at other ways to do that. What makes it difficult is that often the Trustees intend when they support a project there is kind of this inherent intent that funding is to go to the proposing entity, and then it goes to an agency actually to figure out what to do with the project and how to get that money out. We are tied to complying with federal and state procurement regulations, and these are all very limiting. For the most part, they encourage competition too. They believe everything should go RFP, and if you want to do sole source or go some other route, you have to have a really good justification for it, and they don't approve many of those, and they have informed us this year that we're not going to get many of those in the future. So, I just want to put this out to you that when you put a lot of effort into a -- especially the private sector -- that when you put a lot of effort into a proposal and then it seems like why can't you just give us the check and give us the money, we don't have granting authority. We can't just give a check to a person like that, and it's quite possible that even after working with you really closely and knowing that you should be the people doing the work, it's quite possible we're going to have to go out to competitive bid on it. And I just want you to be aware that this is a reality of the situation. We are still trying to figure out different ways to get to improve it, but it is part of the process that we are dealt with.

Fiscal reality -- I think, you know, as I started out on Day 1, showing you that the Trustees had committed to spending half of the remaining funds on habitat acquisition, and then about half of the other half on establishment of a long-term restoration reserve, funding obviously is getting more limited. How we can leverage these funds for other purposes and from other sources, I think is very important. So, I think in terms of reaching out and trying to use a lot of the EVOS funding as seed money, and then using it to attract other sources of funding, I think the more we can do that the better off we are in the future.

One effort that I think has been made significantly in the last year is in the area of community and public information and involvement. And I know Martha, when she was giving her presentation mentioned that she was the only Native person at the church group meeting last April, well, it's not that we didn't invite more, Martha was the only one who came. And, we've worked a lot in the last year to try to get more people from the villages involved in the process and to have that kind of interaction that all of us feel is so valuable, and that's one of the reasons why we were supportive of the project 95052, which is our community involvement project, which will hire on a pilot basis people in Tatitlek, Chenega, and Port Graham to serve as community liaisons for the program that we have, to be the contact there in terms of getting information from local people back to us, and information about what we're doing back to the people in the community also, and we are really looking forward to seeing that part of the project implemented. In addition, as part of our public information outreach, when we have public meetings in the communities this spring, we will be taking key researchers into those communities to talk about the issues that the community is most interested in. This may be herring, it may be harbor seals, it may be pink salmon. We will try to focus on that and have that kind of interaction and exchange, and we will be looking very closely at the ideas that the subsistence work group here have put forth in terms of trying to see how we can implement those into our program.

Last year we sent out a letter to all of the PI's before the field season, encouraging them to hire locally when possible, to go to the community if they were close by to kind of exchange some of the information of their research. I don't know if any of that was successful, I don't know if anyone ever read the letter or did anything about it. We had no feedback mechanism for that. And I think one of the suggestions that the subsistence group put into their report today is really good and one we're going to look at really closely. As part of our budget instructions last year, we were really up front about how you had to budget and show that you would attend this meeting. That was very clearly stated up front, and I think we will be looking at very clearly stating up front, show us how you're going to get your information to the nearby communities or show us how you are going to hire locally or involve the community in your project. I think a lot of times, you know, you just have to force these kinds of issues in order to really get some things accomplished. I don't think it's lack of good intentions on anyone's part, it's just getting everybody to really do it. So, I think we have gone a ways in addressing a lot of your concerns, but by no means are we there yet and I really appreciate Martha's involvement and a lot of other people in the communities who have brought these concerns to our attention.

Lastly, I did want to talk just briefly about information and data management. We do have a project in the '95 budget to look at what to do with the information that we've been collecting through the EVOS projects, and I don't think we really want to develop some huge data base like Vince Patrick is doing, and certainly we don't want to duplicate what other efforts are, but somehow we have to be able -- whether it's in the form of an electronic bibliography, whether it's in the form of a point and click for school systems and libraries -- we have to somehow get the information or a way to show people where they can the get the data, not only the general public but also other EVOS researchers and the general scientific community at large. We do have a project and we do have funding, and we will be starting planning on that in the next month. We will be working with a lot of various people on it. It's not going to be done in isolation. It certainly will involve the other efforts going on, but I did want to make mention of that.

I did want to comment also on something that Martha had brought up earlier about habitat acquisition, and I feel compelled to do this because, obviously, other than kind of the science research part of the program, habitat acquisition is obviously the most major part, restoration tool, that the Trustee have chosen for use of the settlement funds. The fundamental rationale for buying habitat is to make sure that it isn't adversely affected, that it provides sanctuaries for the resources that were injured by the spill. There was significant public support for using all of the money on habitat acquisition; there continues to be. I think the Trustees really made a pretty balanced decision in deciding that they would commit half, and although there is no -- they are not held to any specific numbers, I don't think that will change too dramatically in the future either. I think that the Trustees recognize that -- that are committed to that restoration tool, but I think that they are also committed to the kind of work that all of us are doing here and have been talking about for the last four days.

So, I guess, in conclusion I have a number of acknowledgements and thank-you's that I'd like to give, first, to the core reviewers and -- some of you may not know how these guys got up here and who chose them to sit here, and part of this developed from feedback that we got from all of those who participated in this process about having a group of people who could look at everything and kind of give us some big picture feedback on what was going on, and these people were chosen for their knowledge about the process and the various aspects of the systems that are being looked at, and I think they have really provided extremely valuable assistance that I know, Bob, as Chief Scientist, and the Trustees, and myself as Executive Director depended upon greatly. And I really want to thank all five of them for their participation here in this.

(Applause).

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CERTIFICATE

STATE OF ALASKA)
) ss.
THIRD JUDICIAL DISTRICT)

I, Linda J. Durr, a notary public in and for the State of Alaska and a Certified Professional Legal Secretary, do hereby certify:

That the foregoing pages numbered 05 through 351 contain a full, true, and correct transcript of the 1995 Restoration Workshop of Exxon Valdez Oil Spill Settlement Trustees Council taken electronically by OSPIC staff from January 17-20, 1995, Anchorage, Alaska;

That the transcript is a true and correct transcript requested to be transcribed and thereafter transcribed by me and Sandra L. Yates to the best of our knowledge and ability from that electronic recording.

That I am not an employee, attorney or party interested in any way in the proceedings.

DATED at Anchorage, Alaska, this 23rd day of February, 1995.

LINDA J. DURR NOTARY PUBLIC STATE OF ALASKA

Linda J. Durr

Linda J. Durr, Certified PLS
Notary Public for Alaska
My commission expires: 10/19/97