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DAY 1--TUESDAY, FEBRUARY 7, 1989
MORNING SESSION

WELCOMING REMARKS--DR. DONALD AURAND, MR. JAMES LANE

DR. AURAND: Good morning. My name is Don Aurand, I am chief of the Environmental Studies Branch in MMS headquarters. Jim Lane and I are going to do some introductory remarks here this morning to get this thing started.

First of all, I would like to welcome you all and tell you that we have great optimism about the outcome of this, and I hope that it will turn out to be a good meeting for all of you, as well as for MMS.

The role of my branch in the studies program is that of program oversight. In the course of discussions with the Atlantic Region several years ago, we came up with the idea for this workshop.

What I'd like to do this morning is take just a few minutes of your time to explain what the studies program is all about, what this workshop means in terms of the studies program and how it fits into that, and then a little bit about what we hope to accomplish and why we came up with the idea for doing this workshop.

First of all, the main purpose of the workshop is not necessarily--although we'd love it if it did--to change anyone's opinion or to come up with any incredible new insight into how MMS manages this program.

The purpose of the workshop is to provide information on what we consider to be a major issue in the North Atlantic, for use by all the participants in the decision-making process, not just the Department of Interior or the Minerals Management Service, but the States and concerned scientists as well, so that we can continue to have a meaningful dialogue on the entire issue of oil and gas development in the North Atlantic area.

This is a new approach, if you will, from the Studies Program. It came up somewhere between a year and two years ago, when in discussions on planning out our programs, the issue of whether or not we should do additional studies in canyon-head areas and in canyons themselves was brought up.

I'm not going to go into the gory details of all of this discussion, but the bottom line for all of us was, well, certainly there are people who would be interested in additional information on canyon areas, but, in fact, is that really what the problem is, or is there something more fundamental that we have to work on?

Would a new field biological or chemical study really address the issue, would it resolve anything? Our conclusion was, basically, that it was unlikely to change our conclusion, if we had more field data, and that in many cases we didn't think it was going to really change anyone else's.

So, then the question became what could we do that would further the discussions on this topic if we didn't do field research. What we came up with was this workshop.

There's a couple of other things that bear on this, one of them is the recent GAO audit of the Environmental Studies Program, which, quite favorable to the Studies Program, did highlight that one of the things that we had the most difficulty with was information transfer and dissemination, and, in fact, suggested that there were some areas where that could be improved.

Some of the comments on the questionnaire that the GAO sent out were fairly convinced that we had not done as good a job as we could have done.

I know that is probably true, because I'm not even sure we do as good a job as we should internally disseminating the information. It's difficult to read 300- and 400-page technical reports on all of the issues that the Studies Program gets involved in, and then distill that information down to something you could use if you are involved in making decisions about the program.

In addition to wanting a mechanism to further the discussions on canyons, we are also looking for mechanisms to further the exchange and dissemination of information that already exists in the Studies Program.

So, the idea here is to generate and then document through the proceedings open discussions about whatever conclusions you may have concerning our proposed hypotheses, based on Environmental Studies Program data or any other information that you may want to bear on the program.

This is really an expansion of the purpose of the Environmental Studies Program. As stated in the OCS Lands Act, "it is to establish information needed for the prediction, assessment, and management of impacts on the human, marine, and coastal environments."

That has meant that we have usually designed mostly field or literature studies to do these four things. There is no real reason that we can see why we have to continue to focus on field studies or even synthesis reports.

In fact, there is a fairly, to me at least, pervasive argument for us doing more in terms of risk perception and communication than there is is in doing field biology at this point of the program. So, that's what this is all about.

There is a growing trend in the Studies Program towards this kind of effort. There has been a workshop in Washington and Oregon to identify studies needs. That situation is, of course, different than the North Atlantic because there hasn't been as much done up there.

There is a growing trend towards synthesis reports, and in open-forum discussions. So, what we hope to accomplish is a more thorough utilization and evaluation of the available data by bringing together the scientists who are involved in it, establishment of consensus, where possible, if any consensus can be achieved between the participants in this case, the States and the federal government on technical issues, not necessarily managerial ones, and identification of technical issues which could be resolved through scientific investigations and determination of what would be an appropriate approach to take.

This whole workshop contains elements of at least four different activities that we can identify. The first is sort of an informal risk analysis because we will be talking about perceptions of risk associated with canyon heads and drilling, environmental mediation, although we have no mediator, there are some elements of that here.

There are some elements of preparing a technical summary and, last but not least, if we are lucky there will be a little bit of debate and that's not bad either.

So, what's our approach? Our approach basically was to try find a way to stimulate discussion. We thought the most effective way to do that was to propose a conclusion or a hypothesis that probably would not be entirely acceptable to some participants, at least, and throw it out there and let everybody shoot at it.

Hopefully, if you do shoot at it, or for that matter if you do support it, you will try to provide facts to go along with the comments that we can then use either to provide these to decision makers for their consideration, or to plan additional work within the Studies Program, should that be appropriate.

What we hope to do is allow an open discussion between all of the participants, but it is not a public meeting. That isn't because we didn't want the public here, it's because in order to do this effectively, we felt you had to limit the number of participants, otherwise you wouldn't get the kind of exchange that we need to have.

There are plenty other forums for open public discussion in the MMS. So, that's the approach that we have taken. Then, ultimately we will provide a written record which contains the points of agreement, points of disagreement, and hopefully recommendations.

Now, recommendations could be either technical or policy, for that matter, although we expect to focus on only technical ones. We have no control, nor do we wish to edit what anybody else has to say about this entire issue. It will all go into the proceedings.

The key to success, though, is to focus largely on the science and the facts and the mechanisms which are at work to support your conclusions. If we don't get some information on those things, we will be hard pressed to figure out what to do with the results.

We can't resolve all of the issues, but we're certainly interested in doing it, and one of the purposes of this is to separate out value judgments from the Environmental Studies Program.

We can analyze the elements of the risk which are involved here, but we cannot decide if this risk is acceptable to you, nor do we wish to do so. All we wish to do is to provide the forum for the discussion.

Studies is not involved in making value judgments. However, all of us individually and all of us as managers do do that; that's a separate issue.

So, what we hope to see, if the conclusion is that there are areas which truly can be scientifically examined and which need to be examined and a recommendation of a study were to be forthcoming, we would hope to see the following.

First of all, when should it be done? For those of you who have followed the Studies Program, you know we don't have as much money as we used to, so that's important. Secondly, is the study focused on an issue which can be resolved or narrowed?

I'll tell you right up front, I've been here for four years and the Studies Program has been, often, in a trap of studying something where we couldn't resolve it no matter what we found out.

Now, sometimes that's appropriate, if you don't have enough background information. At this stage of the studies program, we really need to focus on things where collecting additional information will do something.

Are there criteria for evaluating the results? We would like to think that we would have a testable hypothesis that everyone agreed to up front, sort of like if you find the following, then . . . and there's some conclusion about what everyone will agree can happen if you find the information.

Are the users of the results clearly identified? What is the relative significance of the study in relation to other efforts? Is the need for the study clearly explained and documented?

Now, as I guess the overall program manager here, I would like to make it clear that I don't necessarily hope that one of the major outcomes of this is a recommendation for an additional study. I think we have a lot of information that we can already use to clarify issues.

I don't rule it out and if that should be the direction that you intend to go, or if that's the direction we all end up going, these are the kinds of questions that we would like to see included.

Just for closing, I would like to emphasize my own personal biases here. This is only going to work if everybody takes the coats off, loosens the ties, sits back, says what you really think, try to defend it as best you can, and participate fully in the next three days.

Otherwise, we won't get much in the way of dialogue to put into this report. Jim and I are in uniform this morning. We both have our "power-pink shirts" on. We didn't know that this morning when we got up.

We don't want this to be a formal meeting. We want it to be an orderly meeting, certainly, but not necessarily formal. I would urge you to approach it in that vein.

DR. COOPER: Most of us, Don, are pretty timid people.

DR. AURAND: I know. It's been my experience that no one will speak up in these kinds of things. Still, that's the key to success. I wanted to make sure everybody understands that.

So, with that I'll introduce Jim and he'll speak a little bit more directly about that the Atlantic region sees in this. It will probably be somewhat redundant, and then if anyone has any questions, either one of us will be happy to answer them.

MR. LANE: Good morning. I'm Jim Lane, chief of the Environmental Studies Unit in the Atlantic OCS Region. My unit has the responsibility for planning the studies that Don alluded to.

Once they've gone through the approval process--for designing them in the form of specifications for contracted research, and administering those contracts after they've gone through the procurement process.

I'd also like to echo some of Don's other comments, which is to thank you for taking time out of your busy schedules to come here and help us with this particular issue.

The Atlantic Region or MMS in general has funded a good deal of research on submarine canyons, their geochemistry, transport of contaminants related to OCS development into canyon areas, and the biological resources in and around canyon communities.

A good deal of that research, funded not only by MMS, but by others, has only recently been published in the scientific literature in the last two to four years. We feel that enough information has been accumulated to start addressing the issue of submarine canyons.

I'd like to also point out that there's some other motivation here in conducting this kind of workshop, for sometime there has been a moratorium of one sort or another on leasing in the Georges Bank, including submarine canyon areas. There have been protective stipulations applied to tracts around submarine canyon areas.

Most recently the moratorium extends to 400 hundred meters, which effectively precludes not only submarine canyons from leasing, but all areas that would be of prospective interest to the oil industry within the operational constraints of present-day technology.

When Congress imposed this moratorium as an attachment to the appropriations bill, even they recognized that this was stop-gap measure. This was something that they have encouraged MMS at public hearings to try and work to resolve.

This workshop is an attempt in that direction, to provide a forum for deliberation and evaluation of at least the scientific information that's available through the studies program and try and focus them on these controversial environmental issues.

I have received a number of phone calls for people who are also interested in the relationship between this workshop on submarine canyons and a parallel effort by the National Research Council on Environmental Issues, surrounding leasing in general on the Georges Bank.

It is also another effort at a deliberative approach to resolving complex and controversial environmental issues. However, we felt that those issues were so broad and an attempt would be so complex, that it will take some time for the National Research Council to sort through the information and reach some kind of conclusion.

We thought we would take the approach of focusing on a narrower subset of environmental issues where a good deal of information has been collected, so we though we'd start with submarine canyons.

As Don has mentioned, this is an experiment of sorts. We hope that it will be--we are very optimistic that it will work out.

We have basically four factions here, State government representatives, oil industry representatives, representatives from the scientific community, and people that are generally interested in resource management issues and conflict resolution between development on the one hand, and environmental conservation on the other.

We thought that, if nothing else, [this workshop] would stimulate debate and dialogue. I don't think we'll be disappointed in that.

What we are hoping to do, though, is to reach a conclusion. We are encouraging some consensus. There are mechanisms to deal with areas where consensus is not achievable, such as minority reports. We are hoping those mechanisms won't be necessary.

I think I'd also like to conclude just by mentioning one other thing. We are coming to this with as few biases as possible. We are trying to maintain an open and objective approach to this meeting.

Basically we are hoping to let the membership of the panels draw their own conclusions without any undue pressure or bias from us.

That's really all I had to say. Thank you again from coming. If you have any questions for either Don or myself, any questions about the approach of the meeting, why we are doing this, please feel free to raise them now, otherwise, we'll get on with the show.

DR. AURAND: I did manage to forget perhaps the most important logistic item that was given to me. If you smoke, you have to smoke in the lobby, not in the room. I don't want the contractors mad at me for forgetting to say that, so there it is.

Anybody have any questions, if not, we'll go ahead. I think it's a pretty straight forward process.

DR. MACIOLEK: Good morning. I'm Nancy Maciolek and I'll be chairing this morning's session.

Before we get started, a couple of items to mention to you. First, we'd like to record attendance at the sessions and I'm going to start this attendance list around the room. If you'd please sign your name and some other information that's requested.

Secondly, after the talks when you'd like to ask a question, if you would please give your name so that the people that are recording the sessions will have a record of who it is that is asking the question.

Each talk will be 25 minutes and 5 minutes for discussion at the end. We should be pretty much on schedule this morning. There is one change, Bob Ayers from Exxon, I understand, has been iced-in in Houston and won't be with us this morning, so his talk will not be given.

I'd like to introduce our first speaker this morning, Dr. Richard Cooper, director of the National Undersea Research Center at the University of Connecticut. Dr. Cooper's presentation will be on "Pre- and Post-Drilling Benchmarks and Monitoring Data of Ocean Floor Fauna, Habitats, and Contaminant Loads in the Georges Bank Submarine Canyons."

Dr. Cooper.

PRESENTATION OF DR. RICHARD A. COOPER

DR. COOPER: Thank you, Nancy. Good morning folks. It's nice to see the faces of some very old friends, old in terms of years gone back, not necessarily age. It's especially enjoyable for me to step out of the role of being a program manager, center director-type and get, at least for a few days, into the role of scientist.

I'm going to zip rather rapidly through some introductory comments and some generic summarizations of 12 years of submarine canyon work, so that I can show you as much of a submarine video tape, 8 mm [?] in color, that time will permit.

I probably can squeeze in about 15 minutes of this. I'm going to take a speakers prerogative and use up all of my 30 minutes and ask you to ask questions during the coffee break or lunch or this evening.

I think what I have to show you on this color video tape here will say much better than I can verbally here on what the nature of these very unique submarine canyon habitats and their associated fauna are.

The biology and geology of some 18 submarine canyons, ranging from Corsair to Norfolk, was studied primarily through the use of submersibles from 1973 through '84. This was a cooperative effort between NMFS and the U.S. Geological Survey, with my good friend Page Valentine here, providing much of the geological expertise.

The lion's share of this effort was directed towards the Georges Bank canyons, and those immediately to the southwest, i.e., Atlantis, Block, and Hudson. The motivation for these canyon studies was fisheries assessment, habitat definition--thus the concentrated efforts on habitat-type definition, and the description and quantification of associated megabenthic fauna.

These studies were directed towards the canyon heads. In anticipation of exploratory drilling for oil and gas on Georges Bank in 1981, a before-, during-, and post-drilling study was conducted at seven site-specific locations, including five canyon sites, to identify impact, if any, on ocean floor habitats and their associated fauna.

The study took place in July of each of 5 consecutive years from 1980 through 1984, a 5-year period. Most of you are somewhat familiar with this study, but just to kind of redescribe the generalities here, we used the Johnson *Sea Link* submersible calibrated for quantitative purposes with three quarter inch video and 35 mm externally mounted cameras for making north, south, east, and west 600-meter traverses from a site-specific station marker, which was a 37 kilohertz pinger.

We also collected surficial sediment samples and animal samples of scallops, lobster, tilefish and jonah crab for a body burden, edible tissue, and organ analysis of trace metals, PCB's, PAH's and dioxins.

The results of these studies have been published in a variety of outlets, scientific journals, and technical reports. I'm not going to get into the specifics of these, because they are all, probably to some extent, available to you. You've seen them before, and time this morning really doesn't permit.

I'm sure that we will be getting into the discussion of some of these specifics through panel sessions later on, especially tomorrow.

At this time, I'm going to just generically summarize the findings of these 12 years of submarine canyon studies, primarily from the point of view of the Georges Bank canyons, Lydonia, Oceanographer, and Veatch, and show you, as I mentioned, the short video tape.

Several summary comments I'd like to make. Submarine canyon heads are unique physical features, that through physical and biological processes present a wide range of low relief, three-dimensional habitats that attract and/or support a megabenthic fauna whose species diversity and abundance greatly exceeds adjacent non-canyon areas of the outer shelf, upper slope environments.

A major reason for these three-dimensional habitats having been maintained as well, relatively speaking, as they have in recent years, certainly is the inaccessibility of canyon-head environments to mobile fishing gear, i.e., trawls and dredges.

I can't imagine an impact any more devastating than trawls and dredges would be to these submarine canyon-head environments, having seen over the years what similar gear does to comparable environments in our inshore areas.

Species abundance and community structure of the megabenthos is very much a function of surficial substrate characteristics. The so-called habitat types, as we've defined them in past years, habitat types 1 through 5, vary considerably in most of the canyons--Welcker and Block are exceptions.

Along a given depth contour and across depth contours, patch size of a given habitat type and the associated fauna, can be as small as a few meters and as large as several kilometers. In other words, these submarine canyon-head environments are very heterogeneous areas in terms of habitat types, and therefore, their associated megabenthic fauna.

Let me just quickly summarize what these habitat types are and tell you what we estimate their percentage area coverage in these canyon head environments.

Habitat type 1 is a fairly flat, featureless, less than 5 percent cover by area overlay of rock and gravel. We estimate that the type 1 environment covers about 60 percent of the canyon heads.

Type 2, same description, but more than a 5 percent overlay of gravel and rock, occupies about 10 percent of the area.

Type 3, a boulder field, highly productive fishery areas, you'll see that in the video tape. It occupies only about 5 percent of the canyon heads.

Type 4 is our so-called "pueblo village" environment, that ranges greatly in intensity and you'll see this, I think, very clearly in the video tape. A very important environment for the canyons, probably the most important, occupies about 20 percent of the canyon heads.

Type 5, the sand dune/sand wave canyon axis environment, less that 5 percent. Type one, 60 percent, type 4, the pueblo village, 20 percent --I'm sorry.

Type 3, the boulder field and type 4, the pueblo village environment, are occupying a total of 25 percent of canyon floor area. The heads are by far the most important commercial fishery nursery ground environments.

These characteristics, the heterogeneity and the strong association of community structure, species, type, species abundance as a function of habitat type, strongly demand a very site-specific in-situ approach to any future benchmark or monitoring efforts of these megabenthic communities.

Trying to do these kinds of studies in any kind of a monitoring mode from surface vessels is nothing short of a complete waste of time, in my opinion.

Canyon heads represent important nursery grounds as regard the boulder field, type 3 and pueblo village, type 4 habitats. Commercial species taking advantage of these nursery grounds are the lobster, jonah crab, red crab, white hake, ocean pout, conger eel, cusk, and tilefish. Again, you'll see this clearly in the video.

Exploratory drilling had no measurable impact on faunal habitats, species abundance, or community structure in Lydonia, Oceanographer, and Veatch Canyons. This was to be expected from our point of view when we began this study.

Likewise sediment and body burdens of heavy metals, PAH's and PCB's were unchanged from 1980 through 1984. This 5-year database is considered a good benchmark against future drilling operations in this general area of the Northwest Atlantic.

Likely candidates for key indicator species status in terms of long-term monitoring to reflect impact from drilling operations have been identified. The criteria used for indicator species designation can be discussed later and occurs in several of our publications.

These species are the rock anemone, two species of those, actinavi and telia [phonetic], the jonah crab, four-spot flounder, squirrel hake, white hake, tilefish, black-bellied rosefish, conger eel, and ocean pout. Most of those species have some degree of commercial importance.

Now, time wise, I started 5 minutes late, do I get that on the other end here?

I would strongly suggest that those of you in the back of the room move up here forward, you'll be able to see this much better.

I'm going to turn the volume up and let the scientists tell you what they see and I'll interject from time to time.

(Video tape presentation.)

This is about a 15-pound lobster at about 700 or 800 feet under a silt outcrop, pueblo-village type community 4. Rock anemones, ocean pout, another extensive pueblo-village community.

These are important nursery grounds. From the submersible you can actually position yourself where you can look into these biologically created tunnels and grottos here and you can see the small juveniles in the inner recesses of these hiding places.

The cusk and ocean pout here. The ocean pout, is probably in biomass the most abundant of the canyon head fauna. These are areas here created almost entirely through bioerosion, not by current activity. There are cusks, jonah crab, black-bellied rosefish, galatea crabs.

A PARTICIPANT: Which canyon is this, Dick?

DR. COOPER: This is in Oceanographer Canyon, northeast corner.

This is the krill, *Meganyctiphanes norvegicus* that appears to be, I can't prove this, we are fairly certain that krill is far more concentrated in these canyon head environments that in the slope environments. A major source of food for a number of the fishes.

These kind of environments here are where you find your pandalid [phonetic] shrimps and your juveniles of several hake species. If you look carefully and you sit on the bottom for a number of minutes, you can see them move ever so slightly and pick up their presence.

These are high-energy, relatively fast-current, depositional-type environments. Here's a cusk, related to the cod. These are, again, very typical pueblo-village type environments. You'll see lobsters in these. Cusk, conger eel, lobsters, jonah crabs, pandalid shrimp, cleaner shrimp [phonetic], silt stone outcrops, again bioeroded.

We don't have a good idea as to what the longevity of a given rock system is, but I'm sure it goes back 30, 40, or 50 years, I would guess, maybe longer.

That's a basket star, large populations of white hake. This is about an 8 to 10 pound fish and the ever-present swarm of krill. That's obviously not the normal concentration of krill. What you're looking at here is they are attracted to the lights of the sub.

This is about a 1/3-pound lobster, 1/2-pound lobster that's probably 2 years old. This is a very typical pueblo-village type of structure. You do not find these environments outside of the canyons, and especially outside the canyon heads.

They are virtually nonexistent in the outer shelf environments and they are very rare to see in the open slope between canyons.

This is a white hake here and an ocean pout over here. Page Valentine is the scientist who is forward steering this dive. He's slowly working his way up from about 800 feet to 490 feet in the northeast corner of Oceanographer Canyon.

What we've done is take short segments, 10-, 20- and 30-second segments of what, in essence, was about a 2 hour tape climbing up this escarpment.

You'll see how effective these animals are in restructuring and moving sediment in these canyon head environments. A trawl or a dredge would do just absolutely havoc to these kinds of environments.

It took many years to train Page to identify a few species of fish, but after about 10 years he got pretty good at it.

(Laughter)

You're at the base of talus slope now, silt blocks. We're starting to climb almost a vertical wall. You see this is very predominantly a white hake/ocean pout community. These dives were made in the summertime in July, consistently, for 5 years.

Just a minute and we'll get into the shrimp and tilefish habitat. I'll show as much of this as I can during my allotted time and I'll be glad to show you the rest of it over coffee break or this evening or over lunch. There's quite a bit of information in this.

This straight area is biological in origin. Again, if we were out here at this time of year, a lot of these holes would be filled with lobsters.

DR. HECKER: Dick, what's that fuzz on there? Is that a hydroid [phonetic], that fuzz?

DR. COOPER: Yes, I believe that's what it is. This is a rock pile, type 3 habitat, glacially deposited in the northwest corner of Oceanographer, one of our site-specific stations that we monitored for 4 years. This is a white hake/ocean pout community.

You might say, why don't you find lobsters there? The white hake is a voracious predator of small lobsters. Lobsters have very distinctive habitat, as you'll see in just a minute or two.

Unfortunately, the most commercially valuable of all the species out there are the lobsters, so highly nomadic and migratory in behavior, and respond so quickly to 0.1 degree change in centigrade in bottom temperature, that it's not a good indicator species for monitoring, as are some of these that are relatively endemic year round.

A lot of temperatures in these areas are 9, 10, 11 degrees centigrade. These animals, some of them, are very responsive to just a 1 or 2 degree or 0.1 degree change. This is a mild pueblo village community in Veatch Canyon.

This is a pueblo village community occupying the top 20 percent of all the canyon head environments. This is a lobster in the lower right hand corner in this very typical excavation, it's about 1-pounder, 1 or 2-pounder. There are primarily lobster occupations out here at these depths.

This is a type 1 bottom, not much to say about it. It's very featureless and three-dimensional, just a few yards and getting to what superficially may look like a type 1, but really is a thin veneer of overlying silt with an underlying silt substrate into which there is extensive erosion, either moderate to extensive erosion.

We're going to zoom in on some of these lobster habitats so you can get a closer, clear resolution and definition of what they are. Type 1, the type 1 and the pueblo village communities interchange rapidly and vary tremendously in overall size.

This is a tilefish coming up here, about 20-pounder, found out in the open foraging for food. It is going to dive into a very shallow depression to try to get away from us here. You can see it's only about a third covered.

This a crab right down here. Again, these are relatively high-energy erosional types of environments. Short of a massive oil spill right now, I doubt that these environments would show any commercial impact. I would not make that statement with regard to a depositional environment.

Here's another type one, quickly leading into some pueblo village. It's about a 30 degree sloping silt substrate bottom, Veatch Canyon, west wall, one of our site-specific stations.

That's a true angle of drop off there. A 50-pound male tilefish, various species attached, anemones, a black-bellied rosefish, lobster in a pueblo village.

These are areas around the sort of three-dimensional--attachments to large boulders like this are where you see a lot of your juvenile hakes. There's another tilefish grotto. We used hook and line techniques, paying the surface crew members $25 a fish, a very effective way to collect fish.

This is a typical tilefish grotto habitat that you see here. If we were down in Hudson Canyon, these grottos would be much larger. These tilefish grottos down in Hudson, the bigger ones, would be about a quarter of the size of this room. We got quite a few of them on tape.

Look carefully, you'll see a number of juvenile fishes living in the side of this grotto. After looking at some of these tapes here, I see something that we didn't do during the heat of the study.

I think we have pretty good evidence to suggest that these grottos and smaller structures make various cleaning stations for shrimp and various fish.

This is what Hydrographer Canyon looks like. I'm going to fast forward here. This is the axis of Hydrographer. We sat down there and a sudden current came from several tenths of a knot to 2 knots going from south to north at depths of 800 and 1,000 feet here.

I'm going to fast forward real quick. Page is in the back compartment of this sub. The current is about 1 1/2 knots. You could sit there on the bottom and actually watch the sand grains blow over the bottom.

We had all four thrusters in the sub going full tilt and all ballast tanks flooded and we can't quite maintain our position. We're facing south, the current is running north.

A PARTICIPANT: Is that up canyon or down canyon?

DR. COOPER: It's flowing up canyon. This is about 1 1/2 knots here. If you're up here closer, you can see the sand grains and shell fragments working their way over.

The floor of Lydonia Canyon, galatea crab, tilefish. Veatch Canyon, pueblo village community, it's about a 40-pound female. I think this is a cleaner fish, there's conger eel in here. You'll see this guy twist 360 degrees, one of two cycles here. He may be scraping off ectoparasites, or that may be his way of enlarging or shaping that tunnel.

The last thing that I want you to see and then I'm going to turn this off so I don't run over. This is the floor of Lydonia Canyon, a large tilefish population there you never would have expected, and we think it's a depositional environment.

This is about a 40-pound fish at depths of 500 to 1,000 feet in the axis of the canyon, one of our site-specific stations. This is typical and very predictable behavior of tilefish. They'll back out until they get eye contact and then dive back in.

I'm going to fast forward to one more scene of a lobster feeding on a jonah crab. Lobsters are very abundant in this area as well. This is 584 feet, Lydonia Canyon, type 1 habitat.

One or two tenths of a knot current. You never observe any currents here of about more than several tenths of a knot. This is a depositional type environment, as I would judge it.

I apologize, I occupied most of my 5 minutes that I lost at the beginning, so if there's no objection, I'll step down.

DR. MACIOLEK: Does anyone have a quick question for Dick? I guess they'll wait until the coffee break.

Our next speaker is Dr. Brad Butman from the U.S. Geological Survey in Woods Hole, and the title of his talk is, "The Lydonia Canyon Experiment: Circulation, Hydrography, and Sediment Transport."

PRESENTATION OF DR. BRADFORD BUTMAN

DR. BUTMAN: Good morning. I'd like to present some results from the Lydonia Canyon Experiment, which was a major experiment which was conducted between 1980 and '82 by the USGS, with support from the Minerals Management Service and also USGS.

I'd like to leave you with several major conclusions. The first is that the canyon environment is extremely complex, and with respect to the hypotheses we've been presented, that they are probably erosional, at least in some canyons there are both erosional and depositional environments within a single canyon.

So, we can't really classify a canyon as being all erosional or all depositional. This base map shows the location of Lydonia Canyon on the southern flank of Georges Bank. I've colored the 200-meter isobath here. It is one of nine major submarine canyons which cut northward into the continental shelf, along the southern flank of Georges Bank on the order of 10 to 20 kilometers.

Lydonia Canyon was selected for this experiment, because it was the canyon closest to the eight exploratory wells which were drilled along the southern flank of Georges Bank.

I've just indicated here schematically the flow regime in that region of the continental shelf. In red I've shown the mean flow, which in this region is westward along the shelf at 5 to 10 cm/sec. I've indicated the rotary tidal currents which are strongest on the crest of the bank and decreased to 20 to 30 cm/sec in the region of Lydonia Canyon.

I've also indicated the storm-generated currents, driven primarily by wind, which are back and forth, primarily parallel to the isobaths, which can reach speeds in these water depths of 20 to 30 cm/sec.

Because the canyons cut northward into the shelf and the mean flow and the oscillatory currents, the strong ones during which we see most sediment transport are parallel to those isobaths. These currents, both the mean and the storm-generated currents, can cause flow of sediment into these canyons, and they can essentially act as one-way traps.

Depending on whether the flow is to the east or the west, the particles can enter those canyons.

Now I'm going to describe some details of the canyon experiment. I'll be primarily describing the physical measurements, and later this morning, Mike Bothner will take about some of the geochemical measurements made as part of the canyon experiment.

The major objectives of the experiment were as follows:

To describe the currents in Lydonia Canyon and on the adjacent shelf and slope. We concentrated primarily on the canyon in depths deeper than--shallower than about 1,500 meters.

To determine the importance of canyons in transporting sediments on to or off of the shelf.

To try to determine if the canyons are sinks for fine-grained sediments.

Finally, to compare at least two of the canyons on the southern flank of Georges Bank, Lydonia and Oceanographer.

There were several components to the canyon experiment. We had a major moored-array experiment in which we measured currents and sediment transport in a number of locations. We conducted hydrographic surveys to look at the temperature, salinity, and suspended sediment distribution.

We looked at the sediment texture and accumulation rates, rates of accumulation in different parts of the canyon, and we conducted some extensive surveys of the bottom topography and the microtophy [word unclear], using the submersible *Alvin*. Clearly in the 20 minutes that I have, I can only touch on a few highlights.

There is a major report which we prepared for MMS which describes the experiment in more detail.

First I'd like to briefly show the sediment texture on the adjacent shelf. This shows the percent silt-plus-clay and the surficial sediments in the region of both Lydonia and Oceanographer Canyons.

The regional trend is from little silt-plus-clay in the crest of Georges Bank, where the currents are very strong, to greater than 75 percent silt-plus-clay on the deeper continental slope.

So, using the texture as sort of a marker of depositional versus erosional environments. We see non-depositional to heavily depositional on the middle part of the slope. I haven't contoured here the texture within the canyons, that's more complex and I'll show you that in a minute.

The 200-meter isobath is again in blue, which is a good indicator of the rims of the canyon. The point I want to make from this slide is that around the heads of each one of the canyons, even on the shelf, there is an accumulation of finer-grained silts and clays.

This was a fairly thin veneer, only a few meters thick at the outer edge of this, but both around the head of Lydonia and the head of Oceanographer, and on the shelf itself, there is indication of a depositional environment.

It's not clear whether this is happening as the result of current processes or past processes, but there are finer-grained sediments around the canyon heads than on the adjacent shelf a few tens of kilometers away.

If you look down the axis of the canyon, this is a section from the southern flank of Georges Bank, to 1,500 meters in Lydonia Canyon. Here is the depth profile and the ticks on this axis show the locations of the samples. This was retained by *Alvin* in the axis along the floor of the canyon.

I've just shown very crudely, broken the texture into three categories, silt-plus-clay, sand, and gravel. You can see, as you go down the axis of the canyon, the overall trend is for increasing amounts of silt-plus-clay or increasing depositional environment, but there is an area near the head of the canyon in about 300 to 400 meters of water depth, where there is an excess on the order of 30 to 40 percent silt-plus-clay.

The sediments then coarser at about 500 meters, and then get finer again. We'll see that the current strengths mirror this sediment distribution along the axis where they are weaker here, stronger here, and weaker still again in the deeper part of the canyon.

The canyon is really complex topographically. At the moment, disregard the current meter stations on this base map. I just wanted to show you the morphology. Lydonia Canyon is about 5 kilometers wide at the mouth, where it cuts back into continental shelf at the 200-meter isobath.

I'll show you some sections across the canyon at those five red lines. It's essentially V-shaped, definitely the V gets deeper as you go deeper in the canyon. These are now five sections in profile near the head at 500 meters and at 1,500 meters.

The canyon walls in the deeper part of the canyon are about 20 degrees, they're only about 10 degrees at the shallower end. The walls in the canyon are vertical in the canyon are vertical in a number of places. This is sort of an average slope, and in many places it is very step-like and blocky as you go up the slope.

So, here again when we talk about environments in the canyon, we have to think about both the canyon floor, which I showed you before, the sediment texture along the floor, but also the canyon walls which change in character from the deeper parts of the canyon to the shallower parts of the canyon.

I'd like to show you some highlights from the moored-array experiment. This shows the location of all the current meter moorings which we deployed in the canyon during the 2-year period. They weren't all deployed at the same time. We actually had five deployments of current meters, each for 3 to 6 months long.

We maintained four stations as long-term stations, one on the shelf, one on the head of the canyon at 300 meters, one at 500 meters, and one on the continental slope.

The objective of this was to look at the circulation pattern both within the canyon, along the canyon axis, across the canyon axis at several locations, on the adjacent shelf around the head of the canyon and on the adjacent slope both upstream and downstream.

In addition to the moored-array experiment which we conducted in Lydonia Canyon, we made three deployments in Oceanographer Canyon to try to compare those two.

I hate to show spectra to this audience, but this is an attempt to show the difference between the flows, the statistics of the flow on the shelf, and along the shelf and slope and along the canyon axis.

LCA is essentially our shelf station, LCI is the station on the slope. These three stations are progressively from shallow to deep in the canyon axis. All of these measurements are about 5 meters off the bottom. What I'm showing is the energy distribution as a function of frequency.

So, energy here and frequency here, periods from 2 hours to about 500 hours. I want to make two major points. First, and we're showing both the alongshelf, on the shelf, and slope. We're looking at the alongshelf component of flow, and in the canyon we're looking at the up canyon component of flow in the solid line.

I'll only talk about the solid line. On the shelf we see a large amount of energy at lower frequencies, periods of a few days or longer, which are forced by the winds, both on the shelf and on the slope.

That energy is completely absent within the canyon, so we see very little influence of wind-driven motions or low-frequency variability within the canyons.

At all stations we see a major peak in the semidiurnal tide, this major peak mark in red, but in the canyon, we see a major increase in the high frequency motions in the 2 to 10 hour period from the deep stations to the shallower stations.

We'll see in some of the current-meter records that the high frequency motions are very energetic in the canyons, they increase from shallow to deep, and those are virtually absent on the adjacent shelf.

The overall point that I want to make from this slide is that the general statistics, and the currents and the frequencies at which they fluctuate, are very different from the shelf, the slope, and within the canyon, and they change drastically within the canyon.

If we sort of think of the fluctuations in these five frequency bands, high-frequency flows with periods between 2 and 10 hours, the semidiurnal tides, the inertial flows, the diurnal, and low frequencies.

In this region of the continental shelf, fluctuations, if you want to understand the circulation in this area, fluctuations at all these frequencies are important. It's one of the few areas on the shelf in which that's the case.

Just look at the upper panel here. This is a station at LCB, near the canyon head and 300 meters. This shows the orientation of the fluctuations in those five different frequency bands, where at 300 meters deep, we have an instrument at 100 meters just above the canyon rim, and then two within the canyon axis.

The canyon axis is roughly northwest-southeast here. You can see that above the canyon rim the low-frequency flows are essentially across the canyon parallel to the shelf isobaths, whereas within the canyon, the flows are all parallel--essentially parallel or channelled by the canyon.

The most important for sediment transport and circulation is the mean flow, or at least the direction of the mean circulation. I've tried to summarize--let show you first the data from one. This is from one deployment of moored array, in which we had instruments at about 15 stations.

I've tried to highlight, color coded, the observations by shelf in green, canyon in red, slope in blue, and near bottom highlighted in orange. This shows the mean flow over about a 6 month period from November to April.

On the shelf in green, we can see flow essentially parallel to the isobath to the west, as we predicted from the regional picture, and it essentially goes right across the canyon axis.

Any suspended matter which is carried in that mean flow, can then be trapped in the axis. On the adjacent slope, we see in the blue arrows, during this particular period, we see a strong flow towards the northeast.

This is caused by major Gulf Stream eddies to south of the canyon, and the clockwise flow around those causing northeastward flow. When those are not there, we see southwestward flow.

Within the canyon, the flow is more complex and I won't show you on this picture, but I'll show a schematic where we've tried to summarize the flow both within the canyon and on the adjacent shelf.

Again, in green we see essentially westward flow at all depths across the canyon axis. At about 200 meters in the canyon, the red arrows, we saw essentially northward flow on the west side--northward flow on the east side and southward flow on the west side, suggesting at least at that depth some kind of exchange with the slope waters in the region of 200 to 300, essentially the depth of the canyon rim.

On the adjacent slope we saw westward flow when eddies were not present, and strong eastward flow when Gulf Stream eddies were present.

If now calculate the sediment transport around the head of the canyon, I apologize for the quality of the slide here, but we've taken the measurements that were around the head of the canyon, on the shelf, used the current-meter data and a sediment-transport model to calculate the net direction of sediment transport.

These are oriented schematically in geographic orientation with respect to the head of the canyon. On the shelf we see essentially westward sediment transport in the direction of the mean flow with a slight off-shelf component.

At these four stations around the canyon rim, we see a convergence toward the head of the canyon, at both the station at the head and to the west we see flow into the canyon and also at the two stations to the east.

So, at least for this deployment period we see transported shelf sediments--sediments on the shelf, into the canyon axis.

We've done the same thing along the canyon axis. There the pattern is a little bit more variable. We had three stations along the axis, B at 300 meters, S at about 500, and E at about 550. For different deployments, as I mentioned, we had five deployments at each one of these locations, at B we saw--in one case we saw almost no transport and the second deployment we saw some down canyon transport.

At S we saw very strong up canyon transport, and at E we saw--in one deployment we saw down canyon transport and the other case we saw very little transport.

If you try to put that in a schematic cross-section, we have the following picture. Just look at the bottom flows, near the head we saw down-canyon transport, at the mid-depths of the canyon we saw up-canyon transport, and at about 500 meters we weak or little transport.

This suggests, anyway, a convergence towards the head of the canyon of near-bottom transport. I'd like to caution, though, that we're measuring currents at a fixed location on basically an Eulerian current measurement. On such a complex environment, it's not clear whether the particles are actually, over the long term, actually following the Eulerian current field.

That aside, there is strong evidence from all the measurements we've made along the axis of a convergence toward the head, that there is down-canyon transport in some locations, and up-canyon transport in the others.

If you look at the--go back to the sediment texture distribution, again where we found this pocket or increased levels of fine-grained sediments near the head, we find that the measurements show up-canyon transport or convergence toward that "deposit."

I think it's a little difficult for some of us to accept up-canyon transport, but this data at least suggests that.

If you want to compare Lydonia Canyon to Oceanographer Canyon, these are the results, the measurements from the Oceanographer Canyon. Here were only measured at two stations, one at about 300 meters and one at about 550 meters.

At both of those stations we saw net down-canyon flow. I think as Page will discuss later, Oceanographer is very different in sediment texture and topographically from Lydonia.

It's a very smooth-walled canyon, there's very course-grained sediments along the entire axis, and here we saw net down-canyon transport. We didn't see a suggestion, at least at mid-depths, of up-canyon flow.

I'd like to compare the flows in the canyons to the flow on the continental shelf. In a recent paper by Chinadi, et al [phonetic], they summarized the statistics of flow from a number of different experiments along the continental shelf; the Lydonia Canyon Experiment, the North Atlantic Slope Experiment, the SEEP Experiment, MASARS Experiment, and the Baltimore Canyon Experiment.

They separated the flow into two categories. The flow greater than 20 cm/sec, which we took as indicative of erosional environments and flows less that 5 cm/sec, which we took as indicative as depositional environments.

This shows the percentage of time in each one of those flow categories for each one of those different experiments. If you look at the velocity of magnitude greater than 20 cm/sec, and this is organized by depth from 0 to 3,000 meters, so it's without regard to location along the continental slope, but organized by depth.

You can see that the strong currents decrease very rapidly, and by 500 meters, only about 5 percent of the time, or less than 5 percent of the time do the currents exceed 20 cm/sec. We've chosen that as a rough estimate of the erosional threshold for the sediments that exist there.

The converse of that is also true that the velocity, the percentage of time that the currents are less than 5 cm/sec is the inverse of that, and increases to about 40 percent of the time by the time you pass 400 or 500 meters.

So, the 400 to 500 meter isobath is a rough transition zone from an erosional environment, at least based on this, roughly an erosional environment to a depositional environment.

Now, I've plotted on top of this the data from the canyon experiment, but the currents are so strong in these canyons that I had to use a scale which was twice as big. What I've done is transfer to a simpler graph this green line which shows the observations for all the data on the continental slope and compared that to the flows in Lydonia and Oceanographer.

Here off my PC this morning is this beautiful graph. Here's the green line again showing the decrease in the percentage of currents greater than 20 cm/sec, dropping at about 500 meters, and essentially no incidents of strong currents at depths deeper than 500 meters.

The canyon environments are dramatically different. In Lydonia, we see current in excess of 20 cm/sec, at depths less than 1,000 meters 20 to 30 percent of the time. In Oceanographer we see them 40 to 60 percent of the time.

So, the canyons, at least based on this analysis, are much more energetic than the adjacent slope. Those energetic currents cause intense sediment resuspension along the axis in Lydonia Canyon.

I'd just like to show that from some hydrographic data. I'll show you the light transmission or beam attenuation profiles from three stations in the axis of Lydonia Canyon and compare that to the adjacent shelf and the adjacent slope.

Beam attenuation is, under some assumptions, directly proportional to suspended matter concentration. The solid lines are the stations in the canyon axis, the dotted lines are the stations in the slope, and the dashed lines are the stations on the shelf.

First, the suspended-matter concentrations are always higher in the canyon than over the adjacent slope at comparable depths. At all stations within the canyon axis, we see increased suspended sediment concentrations near the bottom, particularly near the canyon head, in depths of 200 to 300 meters.

This is in that area of fine-grained sediments, and we attribute this to resuspension by the strong bottom currents there.

I could show you other examples of that, but this is a good typical one, where we see essentially a tranquil slope, resuspension in the axis of the canyon, resuspension decreasing at depths deeper in the canyon, and also some resuspension on the adjacent shelf.

Dick alluded to the fact that currents change dramatically within the canyon, and this is just a 10-day period record of temperature, beam attenuation (again proportional to suspended sediment concentration), bottom current speed, and up- and down-canyon flow of one station near the bottom in the head of Lydonia Canyon.

I call your attention first to the beam attenuation where we see changes of over two beam attenuation units, which is probably at least 5 to 10 mg/l on a daily basis. We see major increases in suspended sediment concentration over very short periods of time.

We see very rapid changes in the bottom current speed from near 0 to greater than 40 cm/sec in a period of a few hours. We see also both up-canyon and down-canyon flow. At least in the head of Lydonia Canyon, although there's fine-grained sediments there, if this is a sink for fine-grained sediments, it's a very active fine-grained sink.

As Dick said, you can be in a submersible and you can be here when it's very tranquil and you could wait a few minutes and you would have a current of over a knot.

What that does in terms of long-term depositional--what that means for the long-term accumulation of sediments, is probably best addressed by the long-term tracers, which Mike will discuss in his talk later this morning.

In summary, then, I wanted to leave you with a few thoughts, first, at least in Lydonia Canyon, it's extremely complex topography and there's a wide range of spatial scales. Dick showed us some in his video of the small habitats. We've also seen a change--a wide range of spatial scales over which the topography varies.

The currents in all frequency bands are important. Within the canyon they are aligned with the canyon axis. The Eulerian flow, the near-bottom Eulerian flow, converges toward the canyon head.

Warm-core rings are extremely important in controlling the flow along the outer edge of the canyon, and we have seen that the canyon, apparently, does not interrupt the western flow across the canyon rim. That's important in that sediments from the shelf are carried from the shelf across the canyon.

In terms of sediment transport, the direct measurement suggests that the transport converges toward the head, and that there is direct transport from the adjacent shelf into the canyon. We've seen that the environment, at least at depths shallower than 500 meters is much more energetic in the canyons than on the slope.

Again, I'd like to say that there is a wide variety of sedimentary environments, at least in Lydonia Canyon, we've seen areas that are potentially depositional and also ones that are potentially erosional.

I'd like also to point that we've only made measurements now--there are only direct current measurements in only two of the nine canyons along the southern flank of Georges Bank. We basically now define two: one, Oceanographer, which is strongly erosional, and one, Lydonia, which seems to have both erosional and depositional environments.

It's unclear, really, from the available data, what the other canyons are like there. That suggests, I think, a problem with the--at least we need to consider the erosional and depositional classifications of canyons that was proposed in the two hypotheses which we've been presented.

It may be that we have more of a continuum, I think erosional and depositional is a good way to think about it, but there may be a continuum--there obviously is a continuum between erosional and depositional environments in the canyons that we've looked at so far.

Thank you.

DR. TEAL: You have a hypothesis for the difference between Lydonia and Oceanographer?

DR. BUTMAN: About why they're so different?

DR. TEAL: Yes.

DR. BUTMAN: Only a working hypothesis.

DR. TEAL: Fine.

DR. BUTMAN: I think that the strength of the currents is a function of the bottom slope of the walls of the canyon and the bottom slope. The density stratification, which is imposed on the outer part of the canyon by the deep-ocean circulation, and those control the propagation of the high-frequency energy which we saw and I think that there are several theories for it. It's hard to describe in a few minutes.

The propagation of energy both into the canyon and up the canyon axis depends on the bottom slope and the density distribution. I think there is a complex interaction between those, which in some canyons intensifies the flow near the bottom, and in some cases energy won't be allowed to propagate up the canyon axis and intensify near the head.

In other cases, for particular combinations of bottom slopes and density distributions, the energy will be reflected back out of the canyon. I think that is what we're seeing in the difference between

Lydonia and Oceanographer, that those ray paths of energy from the outside in the deep ocean are different.

I don't know quite how the canyon comes to equilibrium, there is some complex balance between the erosion of those currents as they propagate up--the erosional potential of those currents as they propagate up the current, and the response to the seabed.

There may be some structural control in the slopes of the canyons also, but it's a really complex oceanographic problem looking at how energy propagates into these topographic features and how it mixes. I think that's my simple explanation about why they may be different.

DR. BOEHM: What do you feel is responsible for the silt and clay environment on the shelf around the head, the entire depositional feature?

DR. BUTMAN: I don't know. I don't have a good hypothesis for that?

DR. BOEHM: Do you feel it's an active deposition?

DR. BUTMAN: Mike, are you going to address that at all about the accumulation rates around the edge of the canyon?

DR. BOTHNER: I think that the data that I have is more inside the canyon head. To answer Paul's question, I wonder if [inaudible] is it really a little bit deeper, it wouldn't have to be much.

DR. BUTMAN: I've looked at it a little bit, and it doesn't look like that there isn't a bathymetric feature which is causing that. As Mike says, it would only have to be a few meters to make a difference in terms of wave base, which is what is causing a lot of the movement there.

I think the high resolution profiles, as I recall, show that that's a fairly thin layer of fine-grained sediments overlying coarser sands underneath, which suggest it's depositional.

I don't have a good--actually it was very surprising to us to actually define that feature that there were these lobes of fine-grained sediment apparently associated with the head of the canyon. I don't have a good hypothesis for why it's there.

DR. TEAL: You looked at this whole system for a couple of years and one of the things that perhaps controls them in the long term are occasional events of much greater severity than anything you observed.

I guess the question is: Were you out there or did your observations encompass any particularly violent, unusually violent activity?

DR. BUTMAN: No. We maintained the station at LCB in the canyon head at 300 meters for 2 full years. If you look at the statistics, and that was five deployments of about 6 months each, and if you look at the statistics by deployment, it doesn't change very much.

I was actually pretty surprised that there may still be other catastrophic events, but for that 2-year period, you could have picked any 2 months and they would have been representative of the other 24 months.

I think that goes back to the spectra that I showed, that there is very little low frequency variability in the canyons at periods longer than several 100 hours. Most of the variability is in those very high frequencies, and that changes rapidly on a daily basis.

There is some suggestion that that high frequency variability is modulated by processes outside the canyon. For example, there is a very weak statistical correlation between the presence of warm-core rings and the strength of those high frequency fluctuations.

We needed about a 2-year data record to start seeing that correlation, and it was marginal at best. The most obvious correlation was the presence of rings on the outer edge of the shelf, causing flow over the outer edge of the slope and the top part of the canyon.

I also looked very carefully trying to correlate meteorological events on the shelf with flows within the canyon. There, again, the correlation is marginal at best. I think Mike, though, will show some very strong seasonal fluctuations in the sediment flux into the canyon from the shelf.

In terms of the strength of the flows within the canyons, the correlation between what's going on in the shelf and what's going on within the deeper part of the canyon was marginal.

DR. KRAEUTER: You know what you just said, you have a flux of sediment going into the canyon, you've got a depositional area at the head of the canyon, where is the sediment coming from that's coming into the canyon?

DR. BUTMAN: Where does it come from? I would guess it's coming from the shelf.

DR. KRAEUTER: So it bypasses that depositional area to get in there?

DR. BUTMAN: The depositional area is the first place that it comes to. The depositional area in Lydonia Canyon, if you call that a depositional area, is the shallowest part of the canyon head.

So, if you think about stuff coming in from the sides--this data suggests that things come in from the sides, uniformly over the canyon rim, that some of it accumulates in the head and some of it is--what accumulates in the middle part of the canyon may be transported up the canyon and accumulate in that depositional area also.

Some may leak out, but we don't have a good handle on how much that is. You've brought up a really question, though. This has really been a process-oriented study, trying to understand what processes are important, and the time scales that are important in moving sediment around.

The question you ask is rates, how much is moving, how fast is it moving, and at what rate is it accumulating where? This kind of data addresses what processes might be causing those rates, but in terms of calculating those rates, that's a whole other ball game.

As I say, I think the geochemical tracers tell us more about the rates and they may not tell us the whole story either. I think that's a real important question to keep in mind. With the hypotheses we've been asked to address, the rate is the important question.

I think that we don't have a good handle on rates.

DR. MACIOLEK: Our third speaker this morning is Dr. Page Valentine from the U.S. Geological Survey in Woods Hole. The title of his presentation is "Sedimentary Environments in Submarine Canyons and on the Outer Shelf-Upper Slope of George Bank."

PRESENTATION OF DR. PAGE C. VALENTINE

DR. VALENTINE: Thank you. Today in discussing the sedimentary environments in the Georges Bank canyons, I want to focus on several areas where we have, it seems to me, fairly clear evidence for sediment erosion, transport, and deposition.

The observations and interpretations are based on the distribution of sediment texture, sedimentary bedforms such as ripples and sand dunes, on measurements of the strength of bottom currents from submersibles, and from data reported by Brad Butman and his co-workers from long-term current-meter deployments.

The results show that sedimentary patterns do exist in the canyon regions, and suggest that in part we might be able to predict sedimentary environments in areas where we have little or no data.

(Slide presentation.)

This is a bathymetric map of the northeastern part of the U.S. margin. It shows the isolation of Georges Bank. Unlike the New England Shelf and the Middle Atlantic Shelf, Georges Bank is completely isolated from continental sediment sources by the Gulf of Maine and by the Northeast Channel and by Great South Channel here.

So, in effect, and broadly speaking, we have an erosional environment here on Georges Bank. This is a sediment texture map of Georges Bank based on John Schlee's data from the 1960s.

The double-ended red arrows show the orientation of the major semidiurnal tidal flows across the bank, and the dotted arrows show the clockwise mean flow that's been alluded to previously.

We see that on the top of the bank, sand and gravel predominates in areas of strong currents, the tidal currents reach up to 1 knot on the bottom in shallow areas and diminish as water depth increases towards the edges of the bank.

The finer-grained sediments are being deposited around the edges of the bank shown in blue and purple colors there. The mean current is thought to transport the fine sediment that's winnowed from the coarser sediments by the strong currents and is thought to transport that fine sediment southwestward to a depositional area south of Martha's Vineyard, shown in that purple area.

We're going to be looking at the region on the southwest margin of the bank. In this area here, where we have several canyons of varying sizes and some study areas on the shelf and upper slope and outer shelf near the canyons.

This shows our main areas of interest here, it includes large canyons such as Oceanographer and Gilbert, medium-sized canyon Lydonia, a small canyon called Heeltapper Canyon over here, and several shelf/slope areas. One of the shelf/slope areas is near Filebottom Canyon and the other is to the west of Heeltapper Canyon.

Now, the color code here yellow represents a high-energy environment, broadly speaking, the blue is moderate energy and the purple represents a low-energy sedimentary environment.

Before I go on, I'd like to enumerate the sorts of sedimentary environments we're going to be looking at. One of the areas we are going to be looking at is in the region of the upper slope.

We're going to be looking at a region of the upper slope here where we have an erosional band along the upper slope caused by the erosive activity of warm-core ring currents. That occurs in this area and also over here.

Below that in the gullied part of the upper slope is a depositional area where we have accumulation of fine-grained sediments. The canyon rim sediment is transported across the canyon rims into the canyons on all sides, but in particular on the eastern rims of several of these canyons there's increased current activity and the more vigorous sediment transport.

The walls of the canyons, to varying degrees, are undergoing bioerosion from crabs and fish, as was demonstrated in Dick Cooper's video tape.

Around the heads of the canyons there are areas of deposition of fine-grained sediment and the canyon axes also vary in character of sedimentary environment from very vigorous--from deposits of course sand that are subjected to very vigorous currents, from which the fines are winnowed, to other canyons where fine-grained sediments are being deposited.

This is the outer shelf/upper slope area east of Oceanographer Canyon. These show our sediment sample sites here. As you proceed from outer shelf to the upper slope, in this region the rather course shelf sand becomes somewhat finer-grained in this area, picks up some silt and clay, but then from about 200 to 300 meters, the silt and clay is absent and we have a course band of sediment in this region.

Below 300 meters down into the heads of these gullies, the finer-grained component increases to about 30 to 40 percent silt and clay. We have the same scenario to the west of Heeltapper Canyon. So, we know this occurs at least in two regions.

We hypothesize that the fine-grained sediment is being eroded from this 200 to 300 meter water depth band by warm-core ring currents, which flow here at speeds up to 50 to 60 cm/sec, based on current-meter observations from Brad's data to the east of Lydonia Canyon.

This is a depiction of the warm-core rings in July, 1985. Basically they are packets of warm Gulf Stream water that pinch off the Gulf Stream and drift to the west and southwest and basically bump up against the continental margin and drift to the southwest and are eventually reabsorbed by the Gulf Stream.

During this month there were four warm-core rings affecting the margin of Georges Bank. These rings can be from 50 to 150 kilometers across and they drift 5 to 8 kilometers per day, and can remain for many weeks in one area.

This is a summary of 8 years of satellite tracking data of warm-core rings, and shows the margin has compartmentalized into segments here. The first number you see here is the number of warm-core rings during the 8 year period that affected the margin.

The second number is the total number of months rings were present in that segment. So, that around Oceanographer and Lydonia Canyon, there were 19 rings present during that 8 years and they were present in that segment for 17 months, or on the average about 2 months a year.

If our data interpretations are correct, these are important erosive agents on the upper slope and outer shelf.

Now, if we look at the deeper sedimentary environment into the heads of the gullies on the upper slope, we see that the walls of the gullies are 30 to 40 degrees in slope, covered with a very silty sand, the burrowing is very minimal because of the cover of fine-grained sediment here, although the clay that you saw in the video tape that Dick Cooper showed is present beneath this cover.

There is too much of it for the organisms to really do an extensive borrowing job.

This shows what happens when the submarine gently touches the wall of that gully. You can see resuspended silt and clay. This is the floor of a gully which shows an accumulation of dead worm tubes and venus fly trap anemones. We saw no indication of sediment movement in the bottoms of these gullies, no bedforms.

As I said, the sediment is 30 to 40 percent silt and clay, and appears to be, at present, a depositional environment. Now we are going to look at the shelf area around the canyon, around the canyon rims.

As I mentioned, sediment is moving into the canyons from the shelf and around all sides of the canyon, in particular we want to look at the rims of the canyon. This is a map of Oceanographer Canyon.

The green color depicts the presence of gravel pavement, across which very strong pavements flow, up to 50 cm/sec that we have encountered in our submersible dives, and that Brad Butman has documented in a similar area in Lydonia Canyon.

This is a view looking up the canyon wall on the east rim of Oceanographer Canyon showing this gravel pavement, showing build-ups of shelf sand that have moved through the pavement and are in transit into the canyon. So, this is an area of sediment transport where shelf sand is being transported through the gravel into the canyon.

The western rim of the canyon is a combination of gravel and sand, and the currents are much weaker over there and there is no development of a gravel pavement. There is a similar gravel bed on the east wall of

Lydonia Canyon. This was documented with side-scan sonar by Brad and his colleagues.

He deployed a current meter there that documented this strong westward current. We dived on that gravel patch to confirm that it was actually gravel. It's very similar to what we saw in Oceanographer Canyon.

This is the head of Oceanographer Canyon, the green areas are gravel patches, we can show areas where currents are fairly strong. The point of this slide is to show depositional areas of very fine sand, which is 4-fee [phonetic] size sand, which is just about the silt size.

This sand travels in suspension when it travels. So, the yellow color indicates areas where there's less than 10 percent of very fine sand. Those are areas of the canyon axis and of the west arm here, and of the east arm where currents are vigorous.

The blue and the purple show areas of increasing concentration of very fine sand so that the purple areas of 40 to over 50 percent very fine sand.

Now, these appear to be areas where currents are weaker around the canyon, we've seen that there are strong currents around other parts of the canyon.

These two areas appear to have weakened currents, and it's possible that this very fine sand is partly coming from the shelf, but could also partly be coming from the canyon, since Oceanographer Canyon has a very vigorous axial currents, which I'll describe in a minute.

The walls of all of these canyons are underlain by a Pleistocene silt and clay, which is fairly stiff, but it's not hard or rocky in any sense. This shows a bioeroded segment of the canyon wall.

Some canyons have more exposures of this silt than others, and thus are more bioeroded than others. The fragments that are eroded by these organisms are mixed with the shelf sand coming over the rims of the canyons.

This combination makes it way down the walls and onto the canyon floor. This is looking up the west wall of Oceanographer Canyon, looking up the slope of wall, so that these ripples are oriented parallel to the slope of the wall, and shows this silty sand combination of bioeroded silt fragments and shelf sand, making its way down the wall and to the floor, presumably being transported by axial currents along the walls.

Now, on the floor of Oceanographer Canyon, which is one of the most energetic of the canyons we've studied, there are large bedforms that range up to 3 meters in height, sand dunes, basically, which are very clean, have very low concentrations of silt an clay, and we feel that the fine-grained sediments are being winnowed out as the sand is being formed into these large features.

The sand is moving both up and down, on the basis of the asymmetry of these bedforms, the sand is moving both up and down the canyon in different parts, and there doesn't seem to be any net transport out of the canyon. So, this could be considered a depositional area of course-grained sediments.

This shows the gravel on the east rim of Oceanographer Canyon, plus the area where we have course sand along the axis and in the head.

Areas where we have observed large dunes are indicated here, and these arrows indicate the net-transport direction at the time that we made the observations, based on the asymmetry of the sand dunes, so that in this region it's down canyon, in this region it was up canyon to about here, and then down canyon in this region, up canyon in that region.

So, there doesn't seem to be any uniform transport direction in the canyon.

The numbers indicate these are observations we made during submersible dives of bottom currents greater than 75 cm/sec, and the other two are Brad's deployments in the canyon, which showed maximum currents of 75 to 100 cm/sec.

So, this slide is an attempt to summarize the sedimentary environments, in that the yellow color indicates high energy, blue moderate, and purple low.

So, since there is a correlation between sediment transport and deposition and erosion and current strength, we see that the blue areas represent sediment transport from the shelf into the canyons, around the margins of the canyons and off the shelf onto the slope, where warm-core rings affect this band of the upper slope eroding fine-grained sediments, which is then carried into deeper water or carried to a deeper part of the canyons.

The shelf sand moves across the rims, down along the walls. If the canyon is being heavily bioeroded, it's mixed with silt from those outcrops and proceeds to the canyon floor. If the canyon floor is relatively tranquil, such as Heeltapper Canyon or some parts of Lydonia Canyon, then that silt and clay will remained mixed with the sand and not be winnowed out and will form a deposit.

Superimposed on this are areas of much stronger current activity, for instance the east rims of these large canyons, at least in these two instances, experience a strong westward current for long periods of time, have this gravel development, which indicates strong current strength, and sand is being transported much more rapidly across those rims than the others.

In addition, we have depositional areas of fine-grained sand around Oceanographer Canyon, and this is fine-grained sand around Lydonia Canyon. Now, Brad's data, he was showing data on silt and clay, which I'm not, I'm showing the fine-grained sand fraction, which is slightly coarser, but also travels in suspension.

Then along the axes of the canyons, we have varied energy levels, Oceanographer Canyon being very energetic, showing large bedforms and coarse, clean sediment similar to--Heeltapper Canyon and Lydonia Canyon are, in some cases, similar in that their current activity is less, and they show more silt and clay.

In the literature, there is some evidence that the shape of canyons, Brad alluded to this already, the shape of canyons affects or strengthens currents which are flowing in them. In some work done on Hudson Canyon, the authors hypothesized that long, straight, parallel-sided canyons would enhance tidal or internal tidal currents and strengthen them.

On the basis of these observations on sediment texture and current observations, there seems to be a correlation between Georges Bank canyon shape and the energy level of the canyon, so that large canyons, large, long canyons, with a deep mouth at the shelf, at the 200 meter isobath where the canyon cuts the shelf, with a deep mouth, fairly long, and steep walls at the canyon mouth, those canyons seemed to be the most energetic.

The moderate-energy canyons are not as long, generally have a shallower mouth, and the walls are less steeply angled at the mouth, and so on, in the lower-energy canyons down to canyons like Heeltapper, which are really embayments of the slope, are even shorter and shallower, and have less steep walls.

So that based on observations in more than half of these canyons, I've categorized them as to their energy level so thatOceanographer, Hydrographer, and Gilbert, all of which we have observations on the canyon axis where we see bedforms and clean sand--by the way, the canyon axis is--the nature of the canyon axis is a real good indicator of the nature of the canyon's energetics.

They all are classified as high-energy canyons, even though Gilbert's a little short, but has a deep mouth. The moderate-energy canyons would be Lydonia, Powell, Welker, and Veatch. Low-energy canyons would be Heeltapper, Dog Body, Shallop [name unclear], Atlantis, and Alvin Canyons.

I'd like to sum up with this overhead, if you can read it, where we have a comparison between the different canyon energy levels here so that these are generalizations, so that in high-energy canyons with a length of 13 to 25 kilometers, this is from the head of the canyon to shelf break, where the canyon passes through the 200-meter isobath, the depth at their mouth, the deepness of their mouth, is from the 750 to 1,000 meters, and the angles of their walls at the mouth are 15 to 35 degrees.

So, on the rims of these canyons we would have shelf sand and gravel, which is rippled, sand would be in transit to the walls, transported by tidal currents and this westward current that we've pointed out, there's gravel on the east rim, which is stationary and then very fine sand is accumulating around the canyon heads.

The question is: Where does it come from, could it come from the canyon? Well, we don't know that now.

The walls in these high-energy canyons would have many silt and clay outcrops, they'd be rather steep, extensive bioerosion, and it would be a mixed shelf sand and silt and clay, and rippled on these walls in transit to the floor and transported, presumably by axial currents in the canyons.

On the floor, we would have sand, which is rippled and formed into large dunes or sand waves. They are transported up and down canyon by these strong, semidiurnal axial currents, the fines would be separated, carried out of the canyon and perhaps up into the head region and the sand would be deposited.

Now, in moderate-energy canyons, it would be very similar to the high-energy canyons, excepting possibly in the degree of bioerosion on their walls. If their walls are less steep and more covered with sand from the shelf, they would possibly have less bioerosion, so there would be less bioerosion.

Since the axial currents are weaker, you, on the canyon floor, would have a buildup of silty sand, which is rippled, few large bedforms, also transported up and down the canyon by more moderate currents, but they would not be strong enough to extract the fines and so you'd have sand, silt and clay deposited on these floors.

In the lower-energy canyons, shelf sand would also enter these across their rims, but there is a question about whether or not there would be gravel on their east rim, since they don't extend very far into the shelf.

The question would be: How far does it have to extend into the shelf to be affected by this westward current and cause a gravel lag to develop? There would be few silt and clay outcrops because of the low slope of the walls and the buildup of sand from the shelf moving down into the canyon.

There would probably be few ripples as the currents in the walls and along the floor are weak. There is a question about whether these canyons, such as Heeltapper, which incised a shelf only a few kilometers, could be affected by warm-core rings or not. Do they reach into the canyons? Are they an effective erosional agent?

The floor would be mainly silty sand with few ripples, not large bedforms, and it would be a depositional area of sand and silt and clay, similar to the gullies on the upper slope.

Thank you. Any questions?

DR. BUTMAN: Page, in Lydonia Canyon schematic, you showed the wall currents--the wall environment as being more energetic than the axis. What's the data which suggest that?

DR. VALENTINE: The wall?

DR. BUTMAN: You said the wall is more--the currents on the wall are stronger than in the axis in Lydonia.

DR. VALENTINE: I said the currents on the rim, I was talking about the strong currents on the rim.

DR. BUTMAN: In your schematic map you showed light blue and then pink, I guess. It was light blue on the walls and pink in the axis.

DR. VALENTINE: My reasoning was that the silt--that you get more of a silt and clay buildup, silt and clay from the bioerosion in the canyon, plus the shelf sand coming in tends to buildup on those walls, because the currents aren't as strong as they are in Oceanographer Canyon.

Whereas in Oceanographer Canyon, the silt and the clay and the sand mix together, but the currents are fairly strong, especially along the lower walls, and you don't get as large a buildup, and you also get more bioerosion because of it.

In areas where you have buildups of this eroded debris plus lower-sloping walls, that would cover up the silt and clay and, in effect, reduce the bioerosion.

I'm not saying that Lydonia Canyon doesn't have areas of steep walls which are heavily bioeroded, but I have the feeling that

Oceanographer Canyon shows more of it because of its shape and the strong currents that are flowing in it.

DR. BUTMAN: Here's what I was asking, though. In Oceanographer you show essentially an active axis and more tranquil walls, in Lydonia you show it the other way around, more tranquil axis and more active walls; the tranquil axis and active walls.

I don't have much data on the walls, but I would have said that my gut feeling is that the axis is more active than the walls.

DR. VALENTINE: I think I was trying to depict the floor of Lydonia as being a depositional area. Maybe it's not a perfect analogy, but I was trying to--in my mind I was saying, well, the purple areas are depositional areas.

The amount of sediment going into Lydonia or onto its floor is too great for the currents, even though they are fairly strong for them to move it out of there, or even winnow out the fines.

DR. HECKER: Page, I just want to point out that my experience with Lydonia, up around not the very head region up there where you've got the big purple circle, but further down, most of what I find is yes, you've got a fine-grained sediment right in the axis.

You're talking about a relatively narrow axis, but the walls are solid cliff. I get the impression from that, that that is sediment-covered area, and those walls are out and out vertical and cliff throughout the whole length of Lydonia Canyon.

DR. VALENTINE: Well, not throughout the whole length. In the deeper parts, as in Oceanographer you have Cretaceous outcrops down there. I'm not talking about--I can't depict vertical cliffs on this kind of a thing.

The floor below the vertical cliffs is a depositional area. Those vertical cliffs of Cretaceous and Eocene outcrops are--the Eocene are not hard, but they are hard rock. we're not really talking about, at least I'm not really discussing that here.

I'm talking about mainly the shallower parts of these canyons. On a diagram like this I can't--you're right, though, there are vertical walls, but still below it is the depositional area.

A PARTICIPANT: In Lydonia, then, there is still a net transport of sediment out of that canyon or is that strictly a depositional environment that's just building up?

DR. VALENTINE: Nobody knows that. It's the same as in Oceanographer Canyon, that sediment is moving around a lot, but we don't know if any is going out, or if it's just going up and back and forth, back and forth.

I know that at about 1,300 meters in Oceanographer Canyon, there is just a fine-grained sediment on the canyon floor, there are no bedforms and no coarse sand, that's at 1,300 meters. We have data down to about 750 meters.

So, we don't know where the transition is from energetic, coarse bedform environment down to this rather tranquil, fine-grained sediment. We don't know if anything is leaving the canyon.

I think the fine-grained stuff is leaving the canyon, because it's coming down onto the floor, but it's not in the sediments. If you look along the edge of the lower parts of the canyon walls, you can see lots of fragments of silt and clay that have been bioeroded in small buildups of fine-grained sediment.

Out in the axis itself, that sand is very clean. If you look closely, these little--I don't want to go on and on about this, but these little fragments get into the sand, the coarse sand, and are rolled around and soften up, we've found them armored with sand grains as they are disintegrating, and eventually they are eroded away.

DR. HECKER: I just wanted to point out that, sort of to support that, that I had an *Alvin* dive in Oceanographer Canyon at about 1,500 meters to 1,600 meters, well-developed sand ripple marks all long the axis.

I've not seen a similar area in Lydonia at that depth, but very, very well--such that there's hardly any fauna in there. So, it must be in constant motion. There's no attached forms or sea pens or anything.

DR. VALENTINE: That's interesting.

DR. HECKER: That was 1,500 to 1,600 meters, very low.

DR. VALENTINE: You saw a lot of sea pens in Lydonia, didn't you?

DR. HECKER: Yes.

DR. VALENTINE: You see nothing like that in Oceanographer?

DR. HECKER: You do in Oceanographer on the walls, but not he direct axis, and a little ramp going up from the axis at that depth in Oceanographer. So, I think the erosion goes much deeper.

DR. VALENTINE: Thank you.

DR. MACIOLEK: Next on our schedule is a coffee break. Because Dr. Ayers is not here today, we can plan to just start again at 10:45 and be back on our original schedule.

(A brief recess was taken.)

DR. MACIOLEK: Before I introduce the next speaker, may I remind the speakers, please, the rapportuers will be preparing summaries of all the talks, and they would like to see the slides and/or overhead projections that you are using with your talk.

At the conclusion of the session if you could see--I was going to say Jim Hain, he's not here at the moment, but he's sitting in the front row--if you could let him borrow your slides and/or overheads for a short time, he will return them to you.

Our next speaker is Dr. Jim Ray from the Shell Oil Company, and he'll be speaking on "Recent Developments in Industry Sponsored Research."

PRESENTATION OF DR. JAMES P. RAY

DR. RAY: Good morning. I hope Don Aurand notices that I am wearing my designer-pink sweater this morning to go along with the hotel and I do have my tie off, so I am ready for this meeting.

I am traveling without my sidekick Dr. Ayers today. He is so embarrassed that I made it last night, that he is apparently coming tomorrow.

This morning what I would like to do is first of all totally ignore the abstract that is in your booklet. It was to be determined, it was still being determined yesterday as I was sorting slides on the plane up.

What I would like to try to do today is give you a brief overview of three recent studies that are not even complete yet, we are in still in draft final reports on three different studies, but they relate to some of the types of information that you are interested in with regards to this canyon workshop.

I am focusing in primarily on trace-metal levels and distribution in sediments around different types of drilling operations, because I think these would be some of the areas that you will be dealing with over the next 2 days.

I am going to be summarizing three different studies, recent studies done by contractors for us. One study we did because we wanted to, the other two studies we did because we had to. We have different categories of studies in the industry.

California was a study that we did because we had to, Alabama was a study we did because we had to, and the study that was done off the coast of Texas was done as part of our API research program.

In these studies, this was an exploratory well in California, this is up near Santa Barbara, it is called our Molino prospect. This was actually a Shell operation. It was an exploratory well, one well, about 3 miles offshore, 73 meters water depth.

The well depth is right at about 11,000 feet, almost 11,000 barrels of mud was discharged, the quantity of barite, was about 860 metric tons, and this was done back in 1984. We are just finishing up the final reports now.

In Alabama this is a well that drilled off of Mobile Bay, it was exploratory, a single well, about approximately 5 miles outside the Barrier Islands at the mouth of Mobile Bay. Water depth at this location is about 12 meters. The well depth is almost 24,000 feet.

This is a very deep, unusual well compared to most of the ones we deal with. The volume of mud discharged was approximately 46,000 barrels. We went from a discharge to a no-discharge situation at this location. When we got past 15,000 feet we broke over to an oil-based mud and quit discharging.

The quantity of barite was in the range of 300 metric tons of barite. The study date was over the periods of '87 and '88. We just completed the last cruise a few months ago and are just now looking at the draft data from that last cruise.

The study off of Texas was a combination of exploratory and development wells in a field. There were 6 developments and 4 exploratories, its about 12 miles off shore, water depth was about 25 meters, average well depth of all these wells was in the range of 15,000 feet.

The total quantity of barite is close to 17,000 metric tons of barite from the total of all these wells. The study date was in '86 and '87. If can remember all those facts and figures, I will go back to the individual studies and just try to give a brief overview of what was done in them.

The first one is the Molino study in California, the exploratory. We are looking at both the temporal and spatial variability of trace metals in the sediments around the rig, and then also a very detailed study dealing with the bioavailability and potential toxicity of the various different trace metals to benthic invertebrates.

I am going to try to cover a lot of data this morning and go fairly quick because I am just trying to give you an idea of the kinds information that are there, and for some of your future work some of it may be of interest to you.

In California here is Santa Barbara, Point Conception, the Molino location is located right along the coast here. To give you some ideas of the barium concentration in these drilling fluids, these are different well depths, 2,300 meters, 3,500 meters and 3,800 meters.

By neutron activation, the barium concentrations at the shallower depths are a little over 60,000 ppm, going up to 350,000 to 400,000 ppm range.

As most of you know that have been working in the studies here on the Atlantic Coast, barium is your key tracer in the sediments with distance from the platform, and that is one of the reasons why we tend to focus on it, because that is one of the only metals we really can follow to any great distance.

One other thing that was done in this study that was interesting is they went to a weak leech technique with 1 normal hydrochloric to try to get some ideas as to the "bioavailable fraction" of the metals that were present in the sediments.

Just for comparison you can see in the same sediment samples by weak acid leech the quantity of barium that actually comes out as compared by neutron activation, the same here, 413 versus 351. So, you can see by weak acid leech, you very quickly bump into a level where you can't get much more barium out of the sediments.

A lot of the information, I'll give you the bottom line on a lot of what I'm going to cover this morning, is basically when it comes to the metals and the distribution, we are not seeing anything much different than we have seen with other studies.

You see a lot of the same kind of patterns, depending on your current directions, it will determine your deposition patterns and I guess the key point on this one is that any time you are dealing with designing studies or looking for impacts, you can't just work with nice symmetric circles of concentration around a discharge point.

Your currents and your net deposition patterns are going to give you patterns like this. This is the density patterns of the barium seen in the sediments from this particular well. Because of this view graph you really can't see the different concentrations here.

What we ended up doing in this study is once we have done the sediment sampling to find out what the surface top 2 cm distribution was of barium, then designed a 5-station gradient crossing through these different levels of barium in the sediment in which to set up our biological stations because one of the key reasons for this study was to try to determine the bioaccumulation in the organisms and also try to determine where in the cells a lot of the metals were going.

These are the five gradient stations in this study. Once they were selected by neutron activation, they were checked for barium to see the levels. You can see with distance the decreasing concentrations of barium in the sediment. When the samples were actually taken for the biological work, these were the actual levels of barium seen in the sediments at the different stations.

They were at 160, 360, 602, 880, and 1,500 meters for this particular part of the study. The same stations by weak acid leech, you can see the concentrations and again these are in micrograms/gram dry weight, ppm. So, you can these relative numbers.

DR. COOPER: What was the background level of that?

DR. RAY: 700 hundred to 900 was background level, they were measuring in that area. California, along the coast, you will see numbers like that. Further offshore we have seen numbers as high as 1,500 to 1,800 ppm for background. It's kind of variable depending on where you are there.

This is just, again, showing the decrease in barium with distance along those five gradient stations. These are the weak acid leech numbers that you saw a minute ago. These are pre- and post-drilling. This 0.18 kilometers is downstream, this 1.5 kilometers is upstream.

Pre- and post-drill you can see at these stations the increase here, a very definite elevation at the enclosed stations that was 160 meters, 360, and then it starts tapering out.

We are still seeing statistically significant elevated barium out to about 2,500 meters in this downstream direction in this particular well.

Just to see some of the patterns in the other metals, zinc, lead, copper, cadmium and the units they are using in this particular case are nanimoles/gram. You can multiply these out. This is about 4.9 ppm here, this one was about 8.7 ppm, this is about 4.1, this is a little over, this is about .4 down here with the cadmium.

It's just to show the gradient. In the first and second stations, you could see some spatial elevation that appears to be related to the drilling. When you get much beyond that 300 to 400 meter range, you are getting down the level where you really can't detect levels that are much above the background.

DR. BOTHNER: Is there chromium data on that, in that list, I didn't hear?

DR. RAY: No, it is in the next one here. Here is nickel, chromium, and mercury. It is very interesting, with both the nickel and the chromium, there is a trend towards increase with distance. There was nothing in the rest of the data with grain size and the other metals that indicated that it was related to the drilling.

They are not really sure why they were seeing this general trend with distance. We are seeing it in the pre- and post-drilling here downstream, but we're also seeing it in the pre- and post-drilling upstream.

So, there seemed to be some kind of a change in the area, we're talking over years period of time here, a general increase in the area. We've seen this in some other areas, too, we saw some of this kind of a general change in the patterns of the area in the Alabama work.

In this particular study, they ended up working with three different species cyclocardia, which is a clam, pectinaria, the ice cream cone polychaete, and two species of nepthys, which are also polychaetes.

These are more burrowing deep-feeding polychaetes. This is a surface feeder, this is a surface, filter-type feeding organism. There was really three different feeding strategies involved with these three species, so they were interesting to look at, and some of the results seemed to bear out their feeding types.

Just to show you the kinds of information we've got, there is a homogenization and fractionation of the tissue material from the organisms, and actually fractionated them into different fractions, granular, nuclear, mitochondrial fractions, microsomal and then the soluble fractions, high molecular weight and metalithine [phonetic] and low-molecular-weight fractions.

With each of the metals trying to see where in the cell they tended to go. Probably one of the questions that has been the most predominant in people's minds is that we have seen total body burdens of barium increasing in organisms that have been on high-barium substrates.

In this study we are finding that the barium, over 97 percent of it in the case of the clams, is located in this granular fraction of the cells and they think that primarily this is dealing with material that is remaining in the gut and the indication that I've been getting from research, they think that this represents most of the barium that they seem to be seeing is still in soluble form.

In the soluble fractions down here there is very little barium present. In the case of pectinaria, the same thing again, in a granular pellet, over 90 percent of the barium is located there, a little bit in the nuclear fraction, very little in the soluble fraction. In the case of the nepthys species, they were seeing no elevation in the barium.

I have some of the other biological results from this, but let me jump past them and just hit quickly some of the other metals information and patterns that we've found and then if we get time I'll come back to the biological. Or, I can do like the other speaker did and I'll talk the extra 5 minutes and we can discuss it during the break.

This particular study was done off the coast of Texas, down off the San Antonio Bay, Matagorda Island, 622, this is about 12 miles off shore. This is the development location, the six development wells, four exploratories.

There is a variety of data, most of these studies were taken doing grain-size distribution, total organic carbons, in this case calcium carbonate was one of the measurements, trace metals, aliphatic and aromatic hydrocarbons, and this was a study to look in changes of macrofaunal assemblages, both juvenile and adults as it relates to these other parameters.

This is just along one transect. Here you see the barium in parts per million. In the near-field stations, 10 meters out to about 75 meters, it spikes up to around 20,000 ppm. Normally right near a rig you'll see as high 40,000 or 50,000 ppm in some locations with the gradient dropping off.

Cadmium, which is supposed to ppb, not ppm, same thing. In near-field here the spike up to around 600 ppb, a quick drop off by 75 meters in a trend like this. The same thing with chromium, the iron in percent, the drop off and then this general increase out here with distance.

Mercury in parts per billion, you see a spike in here directly below the rig and then the gradient out. Just to be fair and show you that you don't have nice gradients all the time when you're working with it, you go out on another transect you see a drop here and you see a spike here.

One of the other problems in development fields is you don't have single point source. A lot of times you'll have satellite wells drilled and other things like this, and it really complicates the picture when you try to interpret your data, especially in the Gulf of Mexico to try to and find any area that is not influenced by another is rather difficult, especially in development areas.

Again, you can see these spikes, you can see the iron with distance and a lot of times this can be--you have to go back and take a close look at your grain size distribution to see if it answers the reasons why these iron percentages change and ratios change.

Again, there is a little increase in mercury with distance, here chromium, the distance. Sometimes you get jumbled patterns like this and you have to look at all of the data to really ferret that out.

The one other thing in the data that's important to look at--anyway, what this is showing, this is barium concentration, chromium and iron with distance, 10 meters, 150, 750, 5,000 meters from the source. These are core samples, 0 to 1 centimeters, 1 to 2 centimeters, 3 to 6, 6 to 7, 9 to 10--8 to 10, and these are in percent.

Depending on the areas you are located, the bioturbation zone can vary in depth, in some areas it only goes down a few centimeters and one of the earlier studies we did, anywhere from 7 to 10 centimeters you can still see active reworking and redistribution of the surface contaminants being worked down into the sediments.

Here you see down to 2 centimeters, this is in the 2 percent range. Then when you get down to that 3 to 6, it is dropping down to .9 percent. You've got a spike down here back up to 1.8 percent, this is directly beneath the rig.

By the way, in this particular study all of these samples were done by hand-corer. In these shallower water depths we find that we can get better cores, especially when we have a lot of sand bottom, working with divers with hand-corers than we can with surface grabs.

This problem of bioturbation really makes for interesting work when you're doing laboratory work. If you're trying to do sediment exposures in the laboratory where you're overlaying a sediment with a contaminated sediment, if you don't watch out within 2 or 3 days your actual exposure levels in the surface few centimeters will be greatly reduced because of the reworking of the sediments.

A lot of people overlook this. The other thing that was done in this study and in the Alabama study also, is that we are routinely now doing both fine fraction and bulk sediment samples for comparison.

I think a lot of the lead came off of this from the Georges Bank work that was done a number of years ago, using the fine fraction to have a more sensitive measure of being able to detect increases. So, in all of our samples we are doing both in barium, chromium, iron in this case.

You can see the fine fraction is 23,200 ppm, bulk 19,500. We've got this kind of data for all of the stations.

To finish up this, just real quickly talk about the Mobile Bay study, this was about 5 miles off of the mouth of Mobile Bay here. This is block 132, located right off of the main fairway here.

We figured out that--I'm trying to remember what is was. I think in 1 day the Corps of Engineers was dumping something like--I can't remember how many wells a year we could drill to be equal to 1 day of their dumping out here and it's been a little bit of a concern of ours in conducting this study.

We've set up additional control stations over here toward their disposal site to see if we could intercept any of the stuff from their location because of the size of their barge loads. I think we came up with something like 7,000 cubic yards of material from our total well, and they're dumping 42,000 cubic yards a day.

It was a comparison like that. So, needless to say, it would cause concern in a study when we're worried about benthic changes.

In this particular study we had one of those wonderful design-by-committee studies which never make logical sense, so this was not only a bullet pattern, but part of it was driven by--out of logic one came because it was regulation. So, we got 300 foot, 400 foot, and then we go to 500 meters and 1,000 meters.

So, as to the logic of these distances, I really can't defend any of them. This is the basic pattern from which this data was generated.

This one view graph has got more information than you'd ever want to look at at once, but it's really kind of good. Here are you different metals, aluminum, arsenic, barium, cadmium, chromium, copper, iron, lead, mercury, zinc.

What you see here we call this our "lollipop diagram." You're seeing the four cruises, pre-drilling, right after drilling starts, the third one you'll see is right after drilling stopped, and then the last one here is about 8 or 9 months after drilling.

These are each transects. This is 300 west, 300 south, 300 east. So, each of these are the transect and so what you are able to see is before drilling, right after it started, right after drilling ended, in that 8 or 9 months, whatever it was, at the end of it.

So, you are seeing here on this transect the pre-drilling level, a pretty good spike at 300 feet, soon after drilling began it dropped off a little bit and then that 8 or 9 months post-drilling.

So you can see the trends. All this data you are seeing here with these trends like this is whole fraction, bulk sediment.

Now let's look at fine fraction. In fine fraction you see a lot less deviation in the concentrations. In barium, which is our key tracer again, right here at the post-drilling point it really spikes, as you get a little further out it's coming down.

Cadmium, you saw an early spike in here, in close, as you get a little further out it's lower. Chromium is pretty flat, an interesting spike in copper here, 8 or 9 months after the fact. Don't ask me why because I don't know.

Iron is pretty constant, you see a little deviation in the lead levels, at least in part per million here. Mercury, one little spike here, but it stays pretty flat.

In a couple of these studies one of the other things of note, and it's because it's tying back to the regulation, for the most part we're not finding that mercury covaries with barium. This has been one of the raging regulatory issues; that is, the trace levels of metals in barite, especially mercury and cadmium, because there's plans to put regulations on those levels.

With sediments, with distance and also in the bioaccumulation work, we are not finding any correlation between the mercury present and the barium levels. Anyway, in these two view graphs it tells you an awful lot of what was in that study.

I think takes care of it. That was fast. Thank you.

DR. MACIOLEK: Were there any questions for Jim?

(No response.)

I'd like to remind the speakers and people asking questions to please give their name before hand.

DR. TEAL: Cadmium did covary with barium, it looked like it did?

DR. RAY: We had in the Molino study in California, there was some covariance with cadmium and zinc in the first two stations, beyond that you couldn't see it. In those first two stations we were seeing some covariance with the barium, but with the others we weren't seeing that.

DR. BUTMAN: Why would you expect those to covary with barium? If the base metals adhere to the fine-grained particles, barium acts differently than those natural fine-grained particles anyway.

DR. RAY: Part of the thinking behind that is that from a regulatory standpoint we know that barite can range--clean barite usually will have less than 1 ppm mercury, 1 ppm cadmium as part of the barite tied up in the matrix.

Dirty barites, you can get up to 4 or 5, as high as 10 ppm in some cases, depending on what area in the world your barite comes from. As a regulatory thing the agencies have decided that they want to regulate the trace amount of mercury and cadmium that are in barite.

So, it's been a scientific argument versus a regulatory argument as to why are you regulating that. Is there a relationship in the environment between mercury and cadmium and the barium levels, and is the mercury and cadmium bioavailable from the barite.

In the abstract I mentioned some of the other work we did. Jerry Neff and some of the people at Battelle did some of the bioavailability work.

We find that the mercury, for the most part, stays fairly insoluble and is, for the most part, not bioavailable. There is some bioavailability in the cadmium. That's a little bit more soluble out of the barite. Anyway, that's why the question is raised, do they covary.

In other words, if you've got a lot of barium in the environment from the drilling, is the mercury elevated significantly. So, that's why the question is asked and we're trying to answer that question.

DR. COOPER: That's an impressive volume of data that you've got. I'd like to ask sort of a devil's advocate question. So, what? What effect does this have on the marine life there and the prey organisms and the flounders and the lobsters that people end up eating?

DR. RAY: Well, hopefully over the next 2 days with the information you already have, and some more of this, which is a lot of the same, it talks about how much is there and where it is and the effects that you've seen from these studies on changing grain-size distribution.

Everybody else can ask the question so what. I'm biased, I'm from industry. You know, looking at a lot of these things I have a hard time understanding why, especially when you get outside of an immediate deposition zone and the trace levels that I see, that you have biological problem. Grain size seems to be one of the driving factors.

Now, on the work that was done in Molino and the rest of that comes out, the barium part has been published, the rest of the metals data comes out, I think that's going to help answer some of the questions.

The barium work that should be coming out in final publication in another month, they actually took a look, as I mentioned, they quantified the barium concentrations actually in the soluble fraction of the cellular material, where it may be having an impact.

Part of the theory is that the barium +2 ion will compete with the calcium +2 ion and disrupt the metabolism in the cell, some of the metabolic processes.

When they actually look at the concentration, from a conservative standpoint of the barium +2 that would be in that fraction of the cellular material, it's anywhere from one to--[word unclear] orders of magnitude lower than the calcium concentrations.

So, hypothetically the authors of the work feel that at least form a barium standpoint it does not appear there's enough barium in those organisms that were tested to actually have a toxic effect on the organism.

As to whether that hypothesis stands up, I don't know, and I'm not going to defend it. I'll let the authors defend it when they publish the rest of it.

Anyway, that's why a lot of that detailed information, dealing with the cellular fractions, and where the metals go in the organism, was done, and it's trying to get a better answer. Whole-body burdens of metals have very limited value in really trying to predict what the impact is going to be on a particular species.

You've got to go to finer detail to really start having an idea of whether or not the animal is going to be impacted.

DR. BOTHNER: Jim, going back to your field measurements for a moment, some of which showed a decrease of barium with time after the drilling stopped, I'm wondering if you ever assessed the amount that may have been decreased because of dissolution of the barium in sea water, rather than transported away from the site?

DR. RAY: No. We haven't done any of that type of work. That question has come up before and we've had a lot of discussions with Paul Booth at A&M, who has raised that as one of the possible mechanisms for the loss of measurable barium in the sediment.

We've never tried to pursue that and quantify what the real loss would be. In some of Booth's work, it was very interesting, he raised some very interesting questions.

In trying to do mass balance work on the Gulf of Mexico, it doesn't all add up, you know, making liberal guesses as far as transport off the shelf, depth of barium distribution in the vertical profile in the sediments, the amount going in and everything else, it doesn't add up.

There's not as much there as there should be. It is much less than you would predict would be there, based on the volumes both from the industry versus the estimates of what nature's putting in. It doesn't all add up.

We haven't gone any further with trying to answer those questions, but Paul would love to have someone ask him that question, because he's real interested in it.

Thank you.

DR. MACIOLEK: Before I introduce the last speaker of the morning, I'd like to just mention that Bob Ayers will be here tomorrow and is planning to give his talk first thing in the morning before the panel session starts, so his talk will be at 8:00 tomorrow morning.

Our last speaker for this morning is Dr. Michael Bothner from the Geological Survey in Woods Hole. His presentation is on "The Flux and Composition of Resuspended Sediments in Two Submarine Canyons from the Western North Atlantic: Implications for Pollutant Scavenging."

PRESENTATION OF DR. MICHAEL H. BOTHNER

DR. BOTHNER: Thank you, Nancy.

(Slide presentation.)

This is sort of an unusual topographic perspective to just give you a look at our study area here in the North Atlantic off the New England States. I show it just to illustrate that there are a large number of canyons in the sides of the southern flank of Georges Bank.

The data that I'd like to show you today really is concentrated on just one of them, just in Lydonia Canyon. I wish there was more data on some of the others, because they're all a little different as both Page and Brad have alluded already.

I'd like to deal with the issue of pollutant scavenging in Lydonia Canyon. There are four lines of evidence that make me think that the potential for pollutant scavenging is greater in Lydonia Canyon than it is on the adjacent continental slope or on the adjacent continental shelf.

The lines of evidence that I'll be discussing, the intensity and the frequency of sediment resuspension, which is much greater in the canyons.... Let me diverge and stop for a moment and just say that the reason that's important is the observation that the availability of fine-grained particles and the surfaces of those particles in absorbing contaminants from sea water has been well-documented.

So, if you have a mechanism that puts absorbers into the water column to remove pollutants, those pollutants then may be carried by the fine-grained materials and they may control the transport.

The second line of evidence that I'll be discussing are some recent rates of accumulation that we've measured in piston cores in the area. Then I'd like to show you some trace-metal data from surface sediments that suggest that the canyon axis is, indeed, an area of preferential deposition of contaminants.

Finally, and perhaps the most compelling evidence, is the distribution of radioactive isotopes plutonium and lead-210, both which can be considered as analogues for contaminants--for sediment and reactive contaminants in sea water. I'll show you that there are inventories in the canyon axis that are greater than in areas outside the canyon.

First let's talk about sediment traps and the flux of resuspended sediment. This diagram shows the different types of sediment traps that we used during the Lydonia Canyon experiment that Brad summarized earlier.

These traps were put on the moorings and on the tripods to collect material for future analysis. We used different shapes and sizes to collect material where we expected different fluxes.

Although no one knows what the efficiency of sediment traps are in areas where high currents exist, we've gotten around that problem in a relative sense by conducting experiments where all of the trap results are compared to the smaller two-trap, which was more generally used.

So, our results from area to area can be compared in a relative sense. A photograph showing the instrument package that was used in the deeper parts of the slopes is shown here. This is the current meter that was positioned a few meters above the bottom.

We had a benthos camera that took pictures of the bottom. Some of Brad's data for the transmusometer [phonetic], the beam attenuation recording continuously on an instrument located here and here's our part of the puzzle here, it's a sediment trap that collects some of the material that's in suspension.

The first deployment in Lydonia Canyon had the best coverage, so I'd like to show you what we see on an aerial basis in that region.

This map diagram shows the flux in grams/meter^2 per day of the trap sediment over the whole study area. The black dots represent locations of the moorings and more impressive, the diagrams here, the histogram showing the colored bars represent the flux of the sediment collected by the traps at various heights above the bottom.

In this particular location you see that 5 meters above the bottom, between 20 and 26, and the green represents above the bottom.

The main points in this particular diagram are that Lydonia Canyon at the head, in about a little less than 300 meters of water, you have the greatest flux of material collected by the traps. That was consistent over all five deployments.

As you move away from the bottom, higher up into the water column, there is a dramatic increase in the collected material. This points up to the fact that what we're really collecting at the bottom is the source of this material collected.

The other important point is that the axis of Lydonia Canyon, both at 300 meters and at about 600 meters, have very high levels. The canyon axis, even at almost 1,500 meters, has an appreciable level near the bottom.

In fact, a level here at 1,500 meters is quite comparable to what we observe on the continental shelf at 125 meters of water. Shallower on the continental shelf, somewhat higher levels.

The final point is that compared to continental slope, located here at the same depth as shown here, we have just the smallest amount of resuspended bottom sediment found in these bottom traps.

So, this diagram, I think, shows pretty dramatically that the action, in terms of resuspended sediment, is indeed in the canyon axis, compared to other areas surrounding it.

To illustrate that point again in another way, we've now plotted the results of all the traps on the same diagram. Here we're showing meters above bottom and on a large scale, the flux of trap sediment.

The red represents traps that were collected in the canyon axis. These two stations are in the upper reaches of the canyon between 300 and 600 meters. This is deeper in the canyon at 1,400 meters. That's comparable, as I said earlier, to the values found on the continental shelf. Way back here, bringing up a distant third, is the area of the continental slope.

I really don't have much data to talk about Oceanographer Canyon in comparison, but I will point out that in one deployment where we had sediment trap and current-mooring arrays in Oceanographer Canyon, that commensurate with its greater current velocity, we found a greater flux by about 30 percent of the trap sediment collected in Oceanographer Canyon at exactly the same depths measure in Lydonia Canyon.

So, this then illustrates the fact that there is a fair amount of intensity in the canyon axis with respect to resuspension and if my hypothesis is correct that this offers an opportunity for absorption of contaminants, that then is the first step in the argument that we would expect greater accumulation by this process.

Now I'd like to just show you that not only is it intense, but it is quite frequent in terms of resuspension activity. Here is a summary diagram that shows a number of things. The first is an x ray of the sediment trap sample itself, showing variability in texture.

The darker layers represent sand lenses, actually, that were collected as a result of a rather intense current event where resuspension of the coarser sediments has taken place.

This particular sediment trap had, in addition, an instrument inserted that discharged a layer of teflon every 10 days during the deployment. The layer of teflon is shown by this reddish-brown layer in the figure just above the x ray.

Actually, if you see the original x ray, you can see the teflon layers. If you split the core, they are beautiful white bands just like bars on a 10-day interval. The advantage of using this extra wrinkle in a sediment trap, is that you can measure the volume, the mass, and the texture of sediment between the 10-day intervals; correlate that, perhaps, with currents.

You get a feeling for the variability in mass collected throughout the deployment period on a 10-day interval. This histogram at the top of the slide, indicates just what that variability is. Here we're showing the percent of the total weight collected over each 10-day interval as identified by the layers of teflon.

The hachured levels in this particular diagram are designed to show where we have sandy sediments. You can see that there is a factor of almost 10 in the flux of sediment in every 10-day period. This correlates fairly well, but not perfectly, with the current stress measured by the combination of currents and waves at the bottom.

You can see that there's a major peak here, about the 25th of November. It corresponds to a sand layer and to a flux, which is maximum throughout that period. There is also a smaller storm before the 16th of October, which is shown here, and another event at the end of the deployment period where we find an increased flux.

It's not always perfect. For example, in this particular event on the 15th of November does not seem to show itself in the sediment trap.

There are other examples, I was wishing I had another one to show you, actually, which was simpler and more consistent, but where we do have some connection between the activities, particularly on the continental shelf and the fluxes and the texture of the material collected.

I'd like to show you that the opportunity for scavenging occurs over a fairly thick section of the water column in the axis of Lydonia Canyon. This line, I think, illustrates that fact. These are x radiographs of the sediment traps, which are, in a sense, a core sample. They're in a tube, let's put it that way.

A tube sample containing the trap material, when x rayed, give a wide variation in exposure or in texture throughout its length. We think we could correlate pretty well the sample that was located 20 meters above the bottom, with samples higher in the water column at 56 and at 102 meters.

So, I think that when there's an event that causes resuspension in the canyon axis at LCB, which is just under 300 meters water depth, this kind of data suggests that the large part of the water column is influenced.

My argument is there is a fair amount of scavenging that could take place, providing these particles are active with respect to absorption of whatever contaminant is available in the water column at the time.

Well, we make the point here that there's lot of resuspension, but there's also some arguments that the sediment are accumulating in Lydonia Canyon and I'd like to summarize some of the evidence chronologically that lends some credence to that hypothesis.

I think the first data comes from a map generated by David Twitchell, who used a combination of high-resolution seismic reflection and sidescan sonar to identify sediment fill, which he called recent sediment fill on the basis of their morphology and their orientation in the canyon axis.

Some of these sedimentary deposits are up to 25 meters thick, and they are thought to be deposited beginning when sea level first started to rise some 12,000 to 15,000 years ago. He also surmised, however, based pretty much on intuition, that these accumulations would be continuing in the present.

He used, as I say, the basis for this hypothesis that the morphology of the deposits related to little channel cuts and so forth, and the fact that the current measurements were suggesting that Lydonia Canyon could be a trap for material coming off the shelf.

Well, I'm here to say that the more recent geochemical evidence implies that there is still a place for intuition in marine geology, because we cored the upper regions of the canyon, we measured the carbon-14 activity as a function of depth in these cores, and we found, in fact, that there is a fairly linear increase in age, which suggests a fairly continuous rate of sediment accumulation at two locations in the head of Lydonia Canyon at about 150 meters water depth.

The rates of accumulation in this particular area are on average 60 centimeters per 1,000 years. That represents about 2 grams/meter^2 per day. That's at least on an order of magnitude less than what we can catch in our sediment traps, which I will remind you does not have good information about the efficiency.

If you assume that the efficiency is okay, that tells you that there's recycling a number of times before this material actually ends up in a sedimentary record at the head of Lydonia Canyon.

I'll go to the next slide, which shows something about where the sediments are coming from. Our sediments in Lydonia Canyon, this is at LCB at 300 meters. The deployments were carried out before drilling began on Georges Bank and continued throughout the entire drilling period.

We measured the fine fraction of the material collected in the sediment traps near the bottom, and found that the concentration of barium in these sediments started off at a low level, which we assumed to a consistent representation of background, and then increased systematically with time as the drilling progressed.

At the time drilling ended, there were about four wells that were cleaning up and moving off, discharging whatever drill muds were discharged, and at that time we see the largest kick in the barium.

The closest well, if I'm not mistaken, was about 9 kilometers away from this particular location where the tidal ellipse is on the order of 3 kilometers, and, therefore, one has to make the argument that the mean flow and the other currents impacting the Georges Bank area are sufficient to move material introduced to the continental shelf to the canyon axis.

I think this is fairly good evidence that in fact there is communication between the shelf and the canyon axis.

There is other data that we noticed in the Georges Bank Monitoring Program in both the heads of Lydonia and Oceanographer Canyons, looking very carefully with this fine-fraction technique, which concentrates the signal of barium, we indeed saw a few percentage points increase, and above the analytical error, that would suggest that continental shelf material is making it to the heads of both of these canyons.

Now I would like to show you some of the results of the surfaces of box cores collected during the MMS-funded deep-water monitoring program on the continental slope, comparing samples from the axis of Lydonia Canyon at 550 meters with a sample nearby on the continental slope at the same depth.

What I am trying to show is that a few of the transition metals will show an increase in the canyon axis compared the shelf, once some normalization takes place for differences in grain size. Let me show that histogram.

Let me explain that I first devised a normalization factor by dividing the metal concentrations to aluminum, taking advantage of the fact that there is an excellent correlation between aluminum and the percent of fine-grained sediments.

So, if you take the bulk sediments and you divide the metal levels by aluminum, you are immediately normal as to variations in texture. The canyon-axis samples are slightly more coarse than the continental slope.

The other thing I have done just to give a handle on the magnitude of the concentrations that we measure is compare it to average shale, this is world average shales, fine-grained sediments, which I like to assume is a commodity that is not impacted by anthropogenic inputs.

So, this is non-contaminated sediments, if you compare it and it's about the same, you can assume that the sediments are not highly contaminated. If you were to plot data from Boston Harbor against average shale, you'd find that the histogram goes off the scale to the top.

We plot average shale as an enrichment factor, we assign a value of one to that value. You can see two things, first of all the histograms for the other metals, cadmium, chromium, copper, and lead, are not dramatically higher than what we find for average shales.

I can point out, however, that lead is the only metal that we found on the continental slope at all which has an enrichment in the surface sediment compared to deeper levels. That story is a consistent one all along the East coast in fine-grained sediments.

I think the argument stands that this is a reflection of the use of [inaudible] lead in gasoline making its way to the marine environment and showing up in the surfaces of some of these cores.

The other important point from this diagram is that in each case, for these particular metals, in fact, I should say for only these metals, the 12 we analyzed, we find a consistent pattern of cadmium being higher in the canyon axis than on the continental slope, similar for each of the four metals.

So, on the basis of these very low levels of metals, we still can see a consistent increase in the canyon axis where increased scavenging is expected than on the continental slope.

I would like to get into the last line of evidence that suggests there is greater scavenging in the canyon axis than on the continental slope, and this comes from just a very small amount of data, two cores, taken from about 630 meters in Lydonia Canyon and on the open continental slope some distance to the west.

Plutonium, as you know, is an isotope that has been introduced to the atmosphere due to atmospheric nuclear weapons testing. It began, of course, in 1945 and the peak activity was in the early 1960s.

This material is introduced to the surface waters of the ocean. It is a fairly reactive element with respect to biological and inorganic particles. It finds its way to sea floor in areas of enhanced scavenging.

A measure of that scavenging, I think, is seen in a comparison with the Lydonia Canyon axis versus the continental slope, showing the open triangles here. You can see at all depths the activity of the plutonium, recorded in dpm's per gram is considerably higher.

The inventories, if you were to just determine the amount of plutonium on an area basis, is about two and a half times greater in the axis of the canyon than it is on the continental slope.

The other important point to make here is if I explain that our best estimates of sediment accumulation on the continental slope is quite low, on the order of 13 cm/1,000 years or .013 cm/year, the life that that plutonium has been in existence would be constrained to the upper half centimeter or so, if there was not biologic reworking and just accumulation was accounting for this profile.

I was real glad that Jim made a comment about bioturbation and the effect of bioturbation on reworking contaminants into the sediments. This, I think, is a clear example of that type of bioturbation.

Not only on this core do you find a subsurface peak, which could very well be the bottom of a certain deposit-feeder who moves material from the surface to depth, but the fact is you find plutonium to tremendous depths--tremendous--down to 20 or 25 centimeters almost.

In fact, that core isn't long enough to really determine where the maximum exists. I think this is a good illustration of the fact that contaminants are moved into these sediments.

I'd like to make a point right here that I believe that in areas of Lydonia Canyon where a small percentage of fine-grained sediments exist--emits a coarse-grained sediment, that this type of reworking, where organisms exist, may, in fact, be important in scavenging particles and moving the contaminants that are associated with them into the sediment column.

Not only do we find that the plutonium shows this sort of a trend. I'd like to illustrate that lead-210, which has a little different introduction, lead-210 is a naturally occurring isotope, it hasn't been pulsed in as a result of man's activity. It has a major source from the atmosphere, but it also has a source from the decay of radon-226 and seawater.

It shows in inventory and in profile a very similar shape and magnitude as the plutonium. That is that the inventories of lead-210 are about two and a half times greater than they are in the continental slope sample, and that the depth of penetration of lead-210, which has a half life of 22.3 years, is far greater than you can expect without invoking significant biological reworking in this particular area.

Let me close by showing you a map that shows the inventories of lead-210 in the areas where I've had a chance to measure it. You find it on Georges Bank. Page said clearly that it was an erosional area, and you can see that in the coarse sediments of Georges Bank there, there is essentially no excess lead-210 in one particular sample in very coarse-grained sediments.

In the mud patch, so-called, the anomalous area of fine-grained sediments that exists on the continental shelf, south of the islands, we finda significant amount of lead-210. In the canyon axis, however, in Lydonia Canyon, we have the highest value that we've measured anywhere on the east coast.

I think this argues for the fact that there is a potential for the accumulation of contaminants in the axis of this particular canyon, on the basis of some of the sedimentary processes that we've documented in other phases of our program.

Thanks a lot.

DR. MACIOLEK: Any questions for Mike? I have one, of course. You showed us some information from 550 meters from the North Atlantic study, did you look at the pattern at 2,100 meters, the slope/canyon comparison there; station 8 in the canyon?

DR. BOTHNER: I've got that data with me, Nancy, and I did look at it and I didn't find anything that was striking, but what I did in each case was to compare--we took three samples. We took samples at three different occasions at each of those sites.

The data that I showed was only those metals that showed a consistent pattern, that is every time we sampled at station 7, we found higher metals that we did at station 4. At station 8 compared to station 6, my recollection, and this is just a recollection, is that it wasn't a one for one consistent pattern, and so I didn't illustrate it.

DR. MACIOLEK: So it's not as much of a difference?

DR. BOTHNER: I think the variability at 6 and 8 was so great that you couldn't make that good a comparison. I would like to say there is an opportunity to take some of those same cores and measure the lead-210 and the plutonium because of the half lives of both and that is something I'm very interested in doing.

I think that, more than the trace metal concentrations of ambient trace metals, will tell us a lot more about the potential of scavenging in that deeper part of the system.

DR. KRAEUTER: Two questions. One, on your last couple of slides showing the active reworking of the bottom sediments, it looked like the two different techniques or two different isotopes are measuring a different mixing rate, can you explain that?

The curves are substantially different in the kind of reworking that's going on down on the bottom. Are they two different areas? I couldn't quite piece that together.

DR. BOTHNER: In the report that we submitted as part of the Lydonia Canyon experiment, there is a fair amount of discussion about modeling that mixing curve. Because the half lives are different, that, I think, explains in part the different shape.

Actually, we were quite surprised to note that when you apply the models for mixing in both locations, that although the mixing is greater in Lydonia Canyon in the absolute sense, if you apply the regional arrows bars, they come out to be very similar.

Fred Grassle and I have been going around and around about that, just to decide why that's the case, but that's what those profiles suggest on the analysis, using the simple mixing models which assume that biological reworking is very much like diffusion. That's the best we can do mathematically. It may not be a very accurate prediction of the real world, but in the trade it's an established first step.

In using that technology, we get about the same mixing rate. You can make a case that it's a little bit greater in the canyon axis, but I really need to sit down with a biologist who has analyzed the organisms in those different samples and see if it's reasonable to expect that the accumulation rates might be the same--the mixing rates might be the same.

DR. KRAEUTER: The second question, one of your slides of your sediment trap data, the deep one, about 1,200 meters, it looked like there were two things going on, you had a peak, I guess it was about 100 meters above the bottom on it, and then the next level up, which I couldn't get down was almost zero and then it came back up again.

I was wondering how you get that, how you explain that difference?

DR. BOTHNER: Any chance we could see the slide again, I've forgotten. I think it's the second one--third slide, from the start.

DR. KRAEUTER: The very bottom center, right in the axis, you see you've got a red bar and the an orange bar and then nothing, and the deep one is about the same.

Is there just no sample in between them?

DR. BOTHNER: There's no sample in between them.

DR. KRAEUTER: So it's just the fact you didn't have a sampler there?

DR. BOTHNER: Right.

DR. KRAEUTER: I understand that.

DR. BOTHNER: You make the point that--it's interesting, isn't it, that between 20 to 26 meters above the bottom and 100 meters above the bottom, it's about the same.

DR. KRAEUTER: Right.

DR. BOTHNER: I guess that just says there's a fairly uniform layer somewhere above 20 meters.

DR. KRAEUTER: I didn't know whether there was just a sample missing or that it was actually a drop and then an increase?

DR. BOTHNER: Right. I guess we've got the answer, there's just no sample.

DR. HECKER: While we've got this up, this is also on my question. Now you find very high lead in say the 500-meter depth interval in the canyon, is that because the fine material is being continually resuspended so that's it doing a very good job of actively scavenging? Is it because of the resuspension?

DR. BOTHNER: Yes, I think it's because of the resuspension. There may be a biological cycle in the scavenging of lead as well. We may be getting it from the surface waters and pumping it down, so it's quite high.

In fact, I have data which I didn't show that takes the sediment trap material and compares it in the same way that I compare the bottom sediments of Lydonia Canyon, remember the histogram comparing it to average shale.

The interesting point is that the resuspended sediment in the canyon axis has got more lead in it than the continental slope in the upper reaches of the canyon, the very head of the canyon.

I showed you the enrichment factor as being very close to one on those four metals that were measured on the bottom sediments. For lead the enrichment factor is three and a half, as a minimum, for all the suspended matter that is collected in this sediment trap study.

I think that tells me that whenever the suspended matter is having an opportunity to collect lead, that's it's doing a pretty good job. It's only in the canyon axis that we find a very, very high level. So, the scavenging seems to be having an effect on the lead concentrations.

DR. BUTMAN: Mike, just to emphasize what you said, the high levels of lead in the axis can be due to two things. One is resuspension and scavenging of lead from the water column, or second, additional deposition of particles carrying their lead from somewhere else.

You can't really distinguish between those two.

DR. HECKER: That's what I was trying to ask.

DR. BOTHNER: You actually can distinguish between the two, and I think the way you can do that is to look at the lead-210 values, the inventories. The inventories is the amount of lead-210 that's there.

You've integrated over a core or something, and you say that's a certain number of dpm's/cm^2, integrated for the whole depth in which you'd find excess lead-210, which I'm going to say is about 30 centimeters. So there's x number, say it's 30.

If you look at the samples and you compare that inventory with somewhere on the slope and where it's, say half as much, then you look at the specific activity of the lead-210, that is how much lead per gram of sediment, and we find that on a per gram basis, the lead-210 is much higher in the canyon axis.

That's says it's not that lead-210 is carrying in more particles, but it's of a higher concentration on the particles. I think that's the argument that suggests that the scavenging is greater in the canyon axis.

I can't do that for total lead, because I don't know--I can't get inventories of total lead because it's part of the real world, I mean it's part of the natural.... I can't subtract the background very well in these particular samples. I suppose it's possible, but the signal is pretty small.

The enrichment in the bottom sediments is only about 1.5 above what you'd find in average shales, whereas excess lead-210 in particularly plutonium, there is nothing there, the background is zero, it's a non-natural isotope.

So, that's where the isotopes are really helping us out.

DR. MACIOLEK: Any other questions? Thanks, Mike.

That concludes the presentations for this morning. I'd like to thank all of the speakers for very interesting and informative talks, and to ask them once again to please let Jim Hain as the senior rapporteur see any slides, overheads, or notes they may have on their presentations.

We'll break for lunch and start again at 1:00 p.m.

(A luncheon recess was taken.)

AFTERNOON SESSION

(1:23 p.m.)

DR. VALENTINE: This afternoon we're going to hear some presentations on the biology of the canyon region. Before we start, I'd like to tell the speakers that the transcripts of their presentation will be available tomorrow morning at 7:00 o'clock in a packet on the table outside the door.

They would appreciate it if you would edit those transcripts and have them turned in by 7:00 p.m. tomorrow evening.

Our first presentation this afternoon is Paul Boehm, formerly of Battelle Ocean Sciences in Duxbury. He's going to present a talk on "An Overview of the Biogenic and Anthropogenic Hydrocarbon Distributions in Sediments Along the North Atlantic Margin."

PRESENTATION OF DR. PAUL D. BOEHM

DR. BOEHM: I have a hypothesis to test on my own, and that is that you can give an effective talk with lousy slides, lousy graphics. So, we'll test the hypothesis right now, although it was tested this morning as well with somebody.

(Laughter)

I'm not sure what the results were, so we'll test it again.

In many respects the nature of the talk will parallel the discussions on particulate sediment transport and some of the metals discussions. In other respects the discussions of the hydrocarbons are different, because unlike metals when you talk about hydrocarbons you are talking about many different types of compounds, different sources.

While to some of you this is a pain in terms of analyzing and interpreting data, actually it's a blessing because by looking at hydrocarbons we can look at different sources and try to infer points about distributions of hydrocarbons presently and distributions of hydrocarbons that may result from drilling or spillage activities.

In any discussion on hydrocarbons there are several questions that come up, and I'm going to try to be discussing aspects of all of these. Nothing will be discussed comprehensively here.

Basically what are the concentrations of different types of hydrocarbons? What are the compositions? If there are different types of compositions, different types of hydrocarbon inputs, how are they distributed in the study area?

Where, perhaps do they come from? Where they go? What is maybe the ultimate fate of the hydrocarbons? And, what physical and sedimentological processes drive the distributions that are observed?

So, I will be touching on all of those aspects to some extent. The state of knowledge of hydrocarbons basically comes from several sets of studies, the MMS-, formerly BLM, funded studies on Georges Bank and then the slope/rise studies.

The DOE slope studies--DOE SEEP studies and then publications on the hydrocarbon geochemistry and different aspects on all of these studies, and then the Georges Bank Monitoring Program, which actually focused on the drilling activities and the distributions.

So, basically, that is the known published data. There are other bits and pieces of information on the chemistry from NOAA studies as well.

The concentrations of hydrocarbons, nothing very surprising here. When you look at total hydrocarbons, that is the total mass of biogenic and anthropogenic material that you can quantify by a number of analytical methodologies.

Not surprising that on the shelf and the bank itself being coarse-grained and high energy, there are low total levels of hydrocarbons. When you get into some of the depositional areas on the bank proper or on the edges of the shelf, concentrations do rise.

Then the slope and rise is slightly higher, but not by more than a factor of two, so obviously these distributions are driven by the--and you'll see later by the silt-clay content. The range of concentrations are just barely two orders of magnitude.

We want to keep our eye on the polynuclear aromatic hydrocarbons, because those really should be the main focal point of any of these types of studies. Again, they range from the low part per billion in some of the coarse-grained areas to the two part per million total PAH's. We'll talk about that in a minute and what that means.

In the depositional areas, the mud patch areas to the west of the study area, and then the slope and rise, again, sort of intermediate, but in the 100s of ppb that's fairly typical. So, high pollutant levels in the slope and mud patch areas.

We'll be looking at some of this, but just in summary, the PAH compounds are generally--that we find in the whole system are generally of a non-fossil fuel origin. What I mean by that is they are basically a source of combustion of fossil fuels, more combustion processes, presumably on shore and then transported off shore.

We'll see in the PAH composition that this conclusion arises from that. That this fossil-fuel index that we sort of invented several years ago to define the ratio of fossil fuels to total PAH's, are generally in the 20 to 50 range. It would be 100 if they were all petroleum material.

So, 20 to 30 percent of PAH's generally can be ascribed to fossil, the vast majority of it is of a combustion origin. So that's the concentration overview.

Through the Georges Bank benchmark program, especially, and to a less extent in the other studies, we have a reasonably good picture of the seasonal distributions of hydrocarbons. These are the conclusions and I'll back them up in the next few slides.

In general, and I don't mean these as absolute, but on a gross level, defined by our sampling scheme and by the seasonality being four times a year, total hydrocarbons do not vary seasonally in the entire study area.

The terrigenous plant material, and these are plant waxes which, for the most part, make up the large part of the hydrocarbon composition. These also did not vary seasonally. The implication here is that there is a steady state in the study area, the study area being the bank and the slope, the whole margin.

Basically in the steady state with respect to the total hydrocarbons and plant material, the biogenic compounds, with pristane being a classic example, does vary seasonally. I'll show that in some of the data.

Presumably the seasonality is determined by the seasonal deposition and erosional processes. There are sporadic incidences of petroleum residues, presumably tarball materials in the sediments.

For the most part these observations are fairly limited. This parenting, when you look at the hydrocarbon distributions, but they are fairly limited and they are short lived. We may see them in one season and as a general phenomenon and not another season.

Conclusion four is also part of this whole depositional-erosional--the whole dynamics of the area.

A, B, C, D are seasonal snapshots of total hydrocarbon distributions, winter, spring, summer, and fall, the intensity of the black dot here is basically concentration parameter. What we're seeing, if you look, is an overview.

The hydrocarbon distributions are the same in all four seasons for total hydrocarbons. So, this supports the observation that the totals do not vary much seasonally. The distributions are largely dictated by the depositional environment.

In the Gulf of Maine area, the mud patch area, the canyon head area that Brad talked about a little bit this morning, are all areas of elevated silt-clay levels and they're elevated levels of total hydrocarbons, around 10 ppm or greater. For the most part this is a static steady state picture.

If we take a parameter like pristane, which during the bloom period and the production of biogenic lipids is produced in the water column and presumably is deposited in the sediments.

If we look at the intensity of the dots, we see that in winter we are down at very low levels, detectable but background levels.

As we get into the spring and early summer, we see increased levels of pristane in the sediments, but for the most part these are short-lived concentrations. When we go through a whole seasonal cycle, we basically see concentrations of pristane back to original winter levels.

Presumably this is not a degradation phenomenon, I don't think it is. I think this is just basically a depositional and erosional phenomenon, and we're seeing a cycle here of the deposition of pristane.

Again, this is linked to the dynamics of the study area, but there is some seasonality in the biogenic imprint on the sediments. There is also some seasonality in the distribution of tarball materials.

This will be the only gas chromatogram I show, and I need it support the next slide. The overall distributions of hydrocarbons, there are several components. If you look at these peaks above this baseline, we are looking at hydrocarbons which are sourced in terrigenous plant materials, plant waxes.

These are primarily the odd-chain carbons and alkanes, normal alkanes. Underneath this distribution is something called the UCM, the unresolved complex mixture. This is a distribution associated generally with fossil fuel material, degraded petroleum.

A composite source, composite material would have plant material and some UCM. There are some chromatograms and some samples from the bank which basically just contain the biogenic plant waxes, not much of the degraded petroleum material.

There is a distribution which looks very much like tarballs. In this chromatogram we don't have the odd carbon preference, we have basically a smooth distribution of hydrocarbons with some UCM.

This is very characteristic of petroleum or a tarball material. If you look at these three groups, group A, groups B and C, I'll try to explain the next slide.

One of the conclusions that we found in studies of the hydrocarbons on the bank are that a lot of the hydrocarbon distributions are composites of several different source materials.

If we take a total hydrocarbon parameter, this is saturated hydrocarbons, this is increasing concentration, and we take basically an odd-even index, the higher the number here, the greater the odd carbon preference, and the higher the number the greater the predominance of these plant waxes that I talked about in the previous slide, you see that these are several seasons of data from Georges Bank.

We see two subjective groupings of hydrocarbon compositions. We see, just looking at group A and group B now, group B, if we go back a few slides, is source material that is predominantly plant wax.

You go to group A when you add some of this unresolved complex mixture presumably sourced in petroleum. So, the difference between group A and group B, at the same ratio of odd carbons is just we're adding more of this UCM material in group B.

If we look where these stations are, if we look at the silt-clay content of group A stations, we do see that these are, in fact, all the stations that have the higher silt-clay content. They are stations at the mud patch, they are stations at the head of Lydonia Canyon, they are stations at the Gulf of Maine and distributed elsewhere.

What we're seeing here in group A is that with a little bit of fine-grained material, a little bit of clay material presumably, that material is enhanced in the degraded petroleum material.

So, if we sprinkle a little bit of clay on top of this sandy distribution, we're getting increased--we're not changing the amount of plant waxes, but we are changing the hydrocarbon distributions.

These are the composite compositional distributions on the bank. Group A with the fine-grained material having two sources, group B having the plant waxes.

We do see a group C, and in the previous slide you saw the chromatogram which looks a little bit like--a lot like petroleum material. We saw in one of the seasons, this was in the winter, I believe in '77, a distribution in the sediments, it looked like we had tar specks in a lot of sediments of about ten different stations.

The following season we did not see any of these group C hydrocarbon distributions present. Presumably these tar specks had been swept out of the area and the distributions that are left a season later are the two group A and group B types of distributions.

So, we're seeing dynamic implications for the hydrocarbon distributions in terms of these tar specks and in terms of the biogenic materials.

There are some strong geochemical relationships that the hydrocarbons exhibit. Hydrocarbons covary very strongly with TOC and clay content throughout the study area. This is true on the bank, it's true on the slope and on the rise.

To a large extent, the entire study area is one hydrocarbon province, it's just mixing, it's distribution, but this is a very strong covariance. PAH's, which are small fractions of the hydrocarbons, also covary strongly with TOC.

I'll show one slide. If we look at different study areas, not just from the Georges Bank area, but from other OCS sediments, there is similarity in the PAH to TOC ratio.

We see that the PAH's vary somewhat with terrigenous plant material and, therefore, is either considered to be sourced on shore and distributed with the plant material, or is introduced by aerial transport, is deposited and then remixed with the plant material.

In any event, there is a strong covariance with PAH's and plant material. As I demonstrated before, the fine-grained silt-clay fractions, especially the clay fraction, contains a strong anthropogenic signal, mainly this unresolved complex mixture.

There are many such examples, as the next one, fairly good correlations of hydrocarbon parameters, in this case these are all four seasons of data, total hydrocarbons versus organic carbon throughout the study area, correlation coefficients about .9.

If we look at individual aromatic hydrocarbons such as the three-ring phenanthrene compounds and we combine four seasons and a study that was funded by NOAA several years later, a strong, fairly constant relationship between these individual aromatics and total organic carbon, good correlation coefficients.

So, this is a fairly predictable geochemical environment in the absence of point sources of hydrocarbons on the bank.

PAH versus the single terrigenous plant wax. There is a very strong correlation between PAH and the plant material, implying either that they are mixed together and distributed similarly with a fine-grained fraction or they are sourced similarly on shore and distributed along the slope and the rise and ultimately distributed from the Gulf of Maine types of areas.

One of the interesting facets of having looked at hydrocarbons from a number of these OCS environments, this is a composite plot of another aromatic series the flouranthenes and pyrenes versus total organic carbon.

I've combined North Atlantic data, mainly from the shelf and a little bit in the canyon, the head of Lydonia Canyon, with some of the south Texas study area. Generally we see fairly similar relationships from the two areas.

Why these are fairly similar, I don't know. Probably because the aerial transport of these combustion ratios are fairly universal in a lot of the OCS environments.

The slope and rise program focused on a fairly small number of stations over several years. This was the reoccupation of the SEEP transect, this Lydonia Canyon station, high and low topographic areas. I want to show you some of the hydrocarbon data from these areas.

They generally support the same observations of the well-mixed steady state environments, with the hydrocarbons largely forced by the grain-size distributions. I tried to color code this nonsense here.

Basically these are examples of PAH distributions on the slope and rise, and what we are seeing again is that the PAH's, this is an FFPF, the fossil fuel index, remember it goes from 0 to 100, depending on how much of the distribution is petroleum or fossil material.

It's generally low, 20, 30 percent, sometimes getting a little higher into the 50s. This implies that most of the hydrocarbons that are found on the slope and rise, as we found in the depositional areas, are sourced in the higher molecular weight PAH's which are of combustion origin.

So, the compositions of materials, whether they are deposited on mud patch areas, heads of canyons, or the deep areas on the slope and rise are very similar types of compositions.

Presumably this is all the same type or the same source material, the same source regime, which is then just mixed throughout the study area. One of the slides that Brad showed this morning, if you recall, showed that the sediment texture, the silt and clay content, had some interesting features.

The one that I found over the past few years very interesting is the lobe of silty, I presume it's silty clay material, on the shelf area at the heads of the canyons. Oceanographer to some extent, certainly Lydonia Canyon. All of the hydrocarbon measurements are enriched in these areas as well.

We've had several stations from the Georges Bank study, the first study that I summarized, which indicate that they are elevated by factors of two and three and up to five, elevated levels of total hydrocarbons and PAH's in these silt-clay lobes at the heads of canyons.

One of the working hypotheses that still remains a working hypothesis is since this material is compositionally very similar to any of the depositional environments and compositionally similar to the slope hydrocarbons, that this material is sourced in the deeper environment.

Through up-canyon transport and deposition at the heads of these canyons we are seeing hydrocarbons in a steady state throughout this area as well. Whether this is material being deposited or being transported down-canyon or whether it's material coming up canyon in transport, I'm not sure.

It's probably a combination of both. I believe that this feature has been observed fairly persistently over the past several studies and that this still is a working hypothesis that the up-canyon transport is responsible for these depositional areas of fine-grained sediment and, of course, of hydrocarbons as well.

So, as far as the ultimate fate of hydrocarbons, along with fine-grained sediments and any associated pollutants, I think we can make from all these data some general implications. This is from the SEEP study, through studies of different types of organic material, lignin and other types of organic matter.

Vecutesin [phonetic] and coercas [phonetic], determined that about 40 to 50 percent of the organic matter is exported, there is a net export from the shelf to the slope area and it is deposited there.

Secondly, that the canyon heads are sites of the fine-grained sediment accumulation, which is pretty clear from the previous presentations, and along with the sediment accumulation is a depositional are for PAH's and hydrocarbons as well. Right now these PAH's are largely just a combustion source.

Because they are presently at low level, there is a very sensitive type of measurement. We will easily be able to see any fossil inputs or changes in the composition of this PAh material with time at the canyon heads or other depositional areas.

The up-canyon transport of the fine-grained sediments, perhaps leading to the deposition of the canyon heads, is presently a working hypothesis that is certainly suggested by the data. There are other hypotheses as well, but I think we've been talking about this possibility for several years and it is something that perhaps we need to focus on a little bit more.

So that's the hydrocarbon story, it's similar to the fine-grained sediment story and the metals story, except there is a lot more information as far as compositional detail in the hydrocarbons, in the PAH's, that are available to look at these different sources throughout the study area.

DR. TEAL: Has anyone looked at the waxes on living plants, trees I was thinking particularly, which stick way up into the atmosphere to see whether they accumulate the combustion products? Is there solubility in that?

DR. BOEHM: There certainly are some conifers that produce PAH's.

DR. TEAL: I know that, I'm talking about pollution PAH's from combustion.

DR. BOEHM: I don't recall what studies, there have been some studies, but the direct analysis I have seen of plant waxes, show that to be a very, very minor element.

DR. TEAL: Yes, but if you degraded the plant waxes, which they do, and were left with the others--it's a wild idea.

DR. BOEHM: Well, it's not such a wild idea. It's not clear how, the plant waxes certainly degrade. It really depends on what the physical form of what the plant wax is, if it's available for degradation.

The plant waxes seem to be on the coarser particles here. The anthropogenics seem to be on the finer particles. They seem to be the same particles and I don't have a lot of data where people have taken sediments and done that size fractionation in this environment.

There are data from other environments where the size fractionations have revealed different compositions of different particles.

So, if you look at the mixture I think you'll find the PAH's and the plant wax. I think if you look at the coarser materials, the PAH's, for the most part, drop out, if you look at the plant wax.

That certainly may be true of certain plants as far as a screen for PAH's, as a generality explaining some of this distribution, I don't know.

DR. TEAL: You expect the combustion products to be associated with the fine-silt particles in any case, they tend to be more concentrated there. I was just wondering if there was any direct evidence of the filtering of some of this out by living leaves.

Then, of course, there would be a reason for them to be co-distributed.

DR. BOEHM: I think I see the study. I think the leaves from New York or from Boston, I'm not sure.

(Laughter)

DR. NEFF: This is a related question. Do you have a feel for what the solid phase to which the PAH's are absorbed is? In other words, what is the solid phase, is it organic or clay?

DR. BOEHM: That's a tough one. I do believe it's on the fine particle. What the phase is that makes up the fine particles, I'm not sure. I would hazard a guess that within the fine fraction you do have clays and you do have soot and that there are different types of particles in that fraction and you could separate them, and they're probably different particles.

The implication here is not that all the fine particles are the same, they probably are different particles. The PAH's are probably with the organic part.

DR. NEFF: The silt and clay is covered with organic coating, so it could be organic.

DR. BOEHM: Yes, there's probably a mixture of fine particle sources here, if anyone's ever looked at that.

DR. BOTHNER: Paul, I'd like to ask given the statement that you made that Georges Bank and the slope area is pretty much a mixing pot where you find the organics associated with the fine-grained material in proportion to the amount fine-grained material that is there, I'm wondering--I don't quite understand why you can say there's up-canyon transport to account for the lobes of organic-rich or the PAH and hydrocarbon-rich sediments at the head of the canyon.

Why can't that just be from the continental shelf going off shore?

DR. BOEHM: I can't say that definitely. Up-canyon transport has been documented. Without a topographic explanation for that catch basin, without some physical reason why the material ought to just drop out at the heads of the canyon, it remains a working hypothesis that there is material that is spilling over at the top of the canyon, and that there is some steady state.

I certainly don't have any proof of that, but physically I don't know if you can explain that lobe of material physically by just direct deposition.

DR. BOTHNER: It looks like a blowout to you.

DR. BOEHM: It looks like a reversed river delta.

DR. BOTHNER: I see.

DR. BOEHM: Whether that is episodic, whether it takes several big storms to move up silty clay material or whether that is the steady state phenomenon, I'm not sure. I'm certainly going far beyond what my data supports, but it just looks interesting.

DR. VALENTINE: Were these hydrocarbon analyses conducted on the whole sample or just fine-grained portion or both, I wasn't quite clear on that?

DR. BOEHM: That's the whole sample and generally the top 2 centimeters of sediment. I don't believe we've done the size fractionation of sediments of purposes organics analyses. From other environments, Gulf of Maine and others that size fractionations have been done on, you do see the different compositions and different size fractions.

So, putting all the pieces together, I think we are seeing composite types of materials, different types of source materials and different particle sizes. I'm pretty sure.

DR. VALENTINE: If there are no further questions, thank you, Paul.

Our next presentation will be by Dr. Jerry Neff of Battelle Ocean Sciences. He's going to talk about the "Potential Effects of Drilling Effluents on Marine Organisms."

PRESENTATION OF DR. JERRY M. NEFF

DR. NEFF: The first so-called offshore oil well was drilled from a pier on the southern California coast in 1898, and in the last 90 years, well over 25,000 additional offshore wells have been drilled in U.S. waters.

Of those 25,000, approximately 10,000 or more are still in production. Now, the major concern associated with this offshore exploration and development, is that the operations themselves or discharges associated with these operations, whether they be intentional discharges or accidental discharges, may cause serious harm to various marine and coastal environments.

This first slide summarizes the types of discharges and activities that could result in adverse effects to the marine environment.

The mere physical structure of the platform, especially if it is sitting on the bottom, as would be a production platform, can cause local reef effects, causing a localized erosion, or attracting a different fauna and flora and, therefore, changing the local environment.

There are a variety of so-called reef effects, either physical or biological that can occur and have been documented in several cases. Of course, in the Arctic you have artificial islands which obviously are a physical disruption of the local environment.

During drilling there are drill cuttings produced continuously during the drilling operation, and then drilling fluids used for the drilling operations, and generally about a 1,000 metric tons of cuttings are produced per well during exploration, less for production wells.

Nearly about the same amount of drilling fluids. During drilling operations these materials are discharged in bulk quantities several times during the drilling operation, so there is a net discharge over the period of time when the drilling is going on.

In addition, there are a variety of other discharges that are permitted by MPDS permits, including cooling water, deck drainage, ballast water, domestic sewage, and so forth. All of these effluents usually are treated on an on-board treatment system before they are discharged.

Actually, the domestic sewage is treated better than most on-shore domestic sewage before discharge to the ocean. Of course, there are sources of metals and sacrificial anodes. Any time you put a metal structure below water you have to have these anodes to prevent corrosion of the structure, and these contain high levels of a variety of metals, but the total amounts here are very small.

During the actual production phase, you have produced water, which is water generated during the production of the oil. It is usually fossil water and has a composition different from that of sea water, even though usually it is a saline brine, it has elevated levels of several metals, and according to current permits, a certain concentration of hydrocarbons are allowed in produced-water discharge.

Then, of course, you always have the danger of an accident from a blowout or an operational spill. So, these are types of potential problems that could be encountered during offshore exploration and production.

The major concern is related to the drilling fluids, because that is a so-called manmade product that is discharged intentionally during drilling. Because of that concern, there have been a large number of bioassays performed to determine the toxicity of drilling fluids from different offshore sources

This is an old slide, I am sure there have been several thousand additional drilling-mud bioassays because it is part of all current Gulf of Mexico permits, and I believe West coast permits, too, that you do a bioassay periodically during drilling.

Anyway, I think the results would be very similar, even if we updated this with all the latest data. Basically when I did this there had been at least 400 bioassays done with all kinds of offshore drilling muds.

Of these, 79 percent or 41, actually these are cumulative, but 79 percent of the assays gave results in the greater than 10,000 ppm range, which means practically non-toxic. By the time you get down to just under 10,000 ppm, or the LC-50 value, 91 percent of the assays were slightly toxic to practically non-toxic.

In looking at the composition of these muds it became clear that the more toxic muds were those that contained elevated levels of hexavalent chromium salts or diesel fuels, which is added quite frequently, or at least in the past, to drilling fluids for various purposes.

So, the industry and EPA jointly developed a generic drilling-mud program where basically the composition of the muds were characterized and you might say categorized into several, I think there were eight or more generic drilling muds.

Again, at that time I did this slide there had been 60 bioassays reported for these. In this case, the toxicity is generally lower, 94 percent or practically non-toxic or completely non-toxic.

So, again, laboratory data seem to indicate that drilling muds for the most part are not a serious toxicology problem. There can be other problems in the marine environment.

In addition, a large number of studies, mostly in academia, have been performed on sublethal and chronic effects of drilling fluids. Generally the concentrations causing sublethal responses are in the range of 1 to 160,000 ppm during exposure times lasting from 5 minutes to 100 days or longer.

A variety of responses, alterations in behavior, especially in lobsters and so forth, embryo-larval developmental changes, growth changes, changes in metabolism or long-term survival have been recorded.

Also, several people have layered drilling muds on the bottom of aquaria and then introduced animals and observed the effect. You get some behavioral responses, some changes in the recruitment of larvae to the benthos, but generally at fairly high concentrations if you think of a layer up to several millimeters thick laying on the bottom of unfractionated drilling mud.

As a generalization, these chronic effects and sublethal effects are generated at concentrations anywhere from just barely lower than those that are acutely toxic to maybe 100 times lower.

So, using a very conservative so-called application factor in the business of 100 fold, in other words a 100-fold dilution of the acutely toxic concentration, we can estimate how long and for how far around a drilling platform drilling muds might remain toxic.

Basically, if we have a criterion value of 10 ppm that we don't want that concentration to be exceeded in the environment, then that would protect virtually the 100 percent of the animals out there. We can see that we require a 10^5 dilution of that effluent.

At normal current speeds of 10 cm/sec, it requires about 30 minutes to dilute the drilling mud that much, and the distance from the platform, given a 10 cm/sec current speed, which would be very normal for Georges Bank, that is accomplished within less that 200 meters of the platform.

So, basically what these calculations imply is that dilution of the drilling mud is sufficiently rapid that even having to dilute it down to 10 ppm or less to protect all the animals out there, based on laboratory studies, would be accomplished within half an hour and within 200 meters of the platform.

Based on data like this, the National Academy Panel came to the conclusion that it is very unlikely that we would ever see any impacts of drilling discharges on planktonic and pelagic animals and plants, but basically the dilution is rapid and the area of potential impact is so small, that nothing would happen measurable in the water column.

However, 90 percent of the drilling mud and virtually all of the cuttings upon discharge rapidly settled to the bottom. Obviously where they accumulated in significant quantities, they are more persistent and they could cause impacts where drilling fluids are accumulated.

Going on to, you might say, field-validate the last graph, several years ago I took the data from several field studies in five different locations, Louisiana, New Jersey, Alaska, California, and another Alaska one, and plotted the transport time, that is the distance versus the current speed, versus the concentration of total suspended solids in the water column during actual discharges.

You can see there is a logarithmic decrease in total suspended solids, which is a good measure of drilling mud concentration. These values over here are the background concentrations, and you really need to normalize against what the background suspended-solids concentration is.

So, in Norton Sound over here, the study was done during a period when there were very high background concentrations.

So, the decay period, you might say, for Norton Sound drilling fluids is not as steep as that for these other areas here where there are very low concentrations of background suspended solids. The general pattern is that you have rapid dilution and within an hour or so you are back down to virtually background concentrations of drilling fluids.

So this, again, you might say is field-validation of the calculations that you do have rapid dilution of the drilling fluids.

As I say, the muds may not cause problems in the water column because of the rapid dilution, but on the bottom they tend to accumulate and not dilute, at least initially. One of the major concerns about accumulations of drilling fluids on the bottom is that they do contain elevated levels of several metals and that these metals could be a toxicological problem to resident biota.

This is the concentration range of various metals in drilling fluids and this is taken from a fairly large database, and compare it to the range of "background concentration" of metals in natural marine sediments.

There is no such thing as the concentration of a metal in sediments, as Mike Bothner, I am sure, can tell you. There are wide ranges, for instance, natural levels of 8,000 ppm barium have been detected in apparently clean sediments.

If you look across horizontally here at all these, what you find is that in drilling fluids in general, barium is nearly always higher in concentration in the drilling fluid than in the sediments.

Chromium very often is, especially if chrome-lignin-sulphonate is used as the defloculant or thinner, you can get very high concentrations of chromium, though chrome-lignin-sulphonates are being outlawed in several areas, they are not permitted for offshore discharge.

Occasionally copper is higher, though some natural sediments have very high copper. The other two metals potentially posing a problem are lead, as you can see we can have very high concentrations of lead occasionally in drilling fluids, and zinc. Again, zinc is naturally high in most marine sediments, but it can be much higher in drilling fluids.

Most of these metals that I just mentioned, copper, lead, and zinc, in addition to cadmium and mercury, which are sort of on EPA's hit list as being super-toxic metals, most of these are associated with impurities in barium, and so are in a solid matrix, they are not readily exchangeable or easily leechable metals.

They are not in the reducible phase that can be leeched off if the sediment becomes an oxic. Instead they are in the form of sulfide mineral inclusions in impure barite, or they are associated with the mineral lattice of clay material. Bentonite clay is the second most major ingredient in drilling fluids.

So based on this, obviously, there is some concern, though, nevertheless, that you do have elevated levels in some drilling fluids and these could be a problem. One way to look at this is through bioaccumulation studies.

Again, I mentioned earlier that cadmium and mercury are of particular concern to EPA, and as a result EPA has established guidelines for the maximum concentration of cadmium and mercury in drilling fluids.

This is just a comparison of two barites, one with high trace metals and one with low trace metals, and then the kind of dilution or the concentrations you might see in sediments where you have barium accumulated from drilling muds.

So, if the increment in barium in the sediments where you are monitoring is 100 ppm over background, which we did see in a few places on Georges Bank, then these are the expected concentrations of cadmium and mercury in that deposited drilling mud, if you had a very impure barite or if you had a very clean barite.

You can see all these ones in the square here are below any expectation of detecting them above natural background. Generally cadmium in sediments--both cadmium and mercury in natural, clean sediments are rarely present at concentrations higher than about 1/10 ppm.

So, basically all these numbers within the block here are going to be undetectable in the environment. Only if you had an extremely high accumulation of barium from drilling mud and you had very dirty barite, would you have possibility of detecting cadmium and mercury in those sediments.

Going on, in our lab at Battelle, we tried to address the question of whether barium and chromium, the two most abundant metals in drilling muds, were bioavailable and if they could be passed through a marine food chain.

Basically what we did here, this is a study we did with juvenile lobsters, and this was for 99 days, we fed them either uncontaminated sediment--we put them in aquaria with uncontaminated sediment or sediment that had been contaminated with the solid phase of the drilling fluid.

We fed them either depurated food, uncontaminated food, or contaminated food. The contaminated food was polychaete worms that had been allowed to dig in and ingest drilling mud contaminated sediments for several days.

The depurated food, we just took the live worms out of that contaminated sediment, put them in clean sediment for a day before introducing them. This was continued for 99 days so that the lobsters actually ate their own body weight worth of food several times over. I think it was four or five times their own body weight in food.

Basically at the end of 56 days, there were two groups of--the vertical lines connect results that are not statistically different.

So, at 56 days, with respect to barium, the two groups that seemed to have accumulated barium, and those are the animals that were on contaminated sediments, but the food was not a source of barium to either group, because there is no difference between animals fed uncontaminated food and those fed contaminated food.

Chromium, there was no uptake by any group from the contaminated sediments or clean sediments. At the end of 99 days, the chromium results were essentially the same, but barium we had now three groups statistically.

Strangely enough, the animals fed uncontaminated food had the highest concentrations of barium in their tissues. Basically what all these results imply is that there was some minimal accumulation of barium from sediments, but virtually no food-chain transfer of barium.

We did the same study with winter flounder. Both lobster and winter flounder are major commercial species on Georges Bank, and basically got the same results. In this case, there were two groups, again the group fed contaminated food and living on contaminated sediments had the highest concentration of barium in their tissues.

The differences in these two groups are fairly small, but they were statistically significant. Again, the contaminated sediment is a main route of uptake. It is very minimal uptake, no accumulation of chromium.

So, basically these two studies showed that there was minimal availability of these metals associated with sediments. We did physiological studies on these and were able to show actually that the flounder on the contaminated sediments and fed contaminated worms actually grew faster than the others, they were heavier, maybe it's all that barium in their diet.

They were heavier at the end of the experiment than the other group. There were minimal indications of stress in these animals. The lobsters were slightly more stressed by the drilling mud contaminated sediments.

So, the conclusion is winter flounder and lobsters were able to accumulate small amounts of barium but not chromium from the sediments. Neither species accumulated significant amounts of barium and chromium from food.

The lobsters but not flounder were mildly stressed by exposure for basically 100 days to sediments heavily contaminated with drilling muds.

That's the lab studies, now into the field. They had a lot of wells drilled out there and not surprising there have been a lot of field studies of the impacts of drilling operations mostly on the benthos because of this appearance or the perception and conclusion that the water column impacts would be impossible to demonstrate.

These studies have been performed in the Gulf of Mexico, California, on the east coast, in the North Sea, a large number of studies in the North Sea, mainly by the British, and basically the effects--community responses, this is the benthic community, effects are seen only in the benthos in the vicinity of mud and cuttings discharge and they are most pronounced in low-energy environments where mud and cutting solids accumulate.

All the evidence to date is that there is substantial recovery within 1 year. In the North Sea, wherever they have used water-based drilling fluids as opposed to oil-based muds, there is substantial evidence of recovery or beginnings of recovery within one year after cessation of discharges.

On the East coast in the mid-Atlantic, there was also some evidence of the beginning of recovery within 1 year. There has been a limited amount of study of bioaccumulation of metals in the field.

There has been some indication of uptake of barium and chromium immediately after drilling again on the mid-Atlantic and in the Beaufort Sea and so forth. The other metals appear to be virtually unavailable or at least not distinguishable from the natural variability that you get in marine animals, especially benthic fauna.

I am not aware of any real studies of accumulations of hydrocarbons from drilling fluids, at least water-based drilling muds, and so there is no documented evidence of uptake of hydrocarbons by benthic animals in the area of drilling mud discharges.

Most of what I've said so far is fairly generic, let's get closer to home. I'm sure many of you have seen this slide before. This is the Georges Bank Benthic Monitoring Program, which was performed from '81 to '83 on Georges Bank. The round dots are the regional stations sampled quarterly for three years, a total of 12 cruises.

The stars are locations of exploratory drilling rigs. There were two sites where we did site-specific monitoring. One was at station 5 here and the other was at station 16 farther out in deeper water, about 140 meters of water, this is in about 80 meters.

So, as I say, we did regional and site-specific sampling over a 3 year period. This is the-site specific array, an array of 29 stations located around the platform. The solid circles are the primary site-specific stations, those are samples we took and analyzed completely both for chemistry and biology.

Mike Bothner did the metals chemistry, Battelle and Woods Hole did the biology. As I say, we sampled these stations quarterly for 3 years.

What Mike Bothner found in terms of the chemistry was that there was an accumulation of drilling-mud solids as evidenced by barium accumulations in the immediate vicinity of both of the two platforms we monitored in block 132 and 140.

The increments over background were several fold, five to seven fold, I believe in the bulk sediment and then much higher in the fine fraction because the sediments on Georges Bank are fairly course.

So, the barium is always in the clay-sized fraction. So if you separate out the clay-sized fraction you get a much greatly magnified signal; you might say signal-to-noise ratio. So, there was definite evidence of accumulation of drilling-fluid solids based on the barium data and also there were observations of drilling cuttings, coarse, angular particles in the sediments.

There was a general trend over time for the incremental barium to move away from the platforms, especially in the site-specific array that I just showed you over time, again indicating a migration of these, resuspension, redeposition, and dilution.

I believe Mike showed evidence of a half-time per washout of barium of about half a year, .4 years, I believe it was. So there was a chemical signal in the environment.

There was little or no evidence of hydrocarbon accumulation in the sediments. Jim Payne from SAIC did the hydrocarbon work. The platform in block 132 obliged us by using diesel in their drilling fluid and discharging approximately 1,600 liters of diesel fuel in their drilling fluids.

There was some indication of a slight signal right at that time near that platform, but that pretty much was obscured by the natural background which is approximately 1/10 ppm. So, there was an increment maybe to 2/10 or .5, and then that went away fairly quickly.

So there was a little accumulation of metals and very minimal accumulation of hydrocarbons around the platforms. Looking at the biology, and I'm trying to summarize 3 years, 12 cruises of data in a slide or two, this is station 5-1, which is approximately 200 meters downcurrent from the rig in block 312.

Looking first at the diversity, again, here is period of drilling right here in the middle and then we followed for several years afterwards. The diversity, they are seasonal trends but no obvious impact of drilling here.

In terms of average number of individuals, it would appear that the number actually increased during the drilling operation and then settled at a higher level for the remainder of the period when we monitored.

This may be an artifact that we had lower abundances than normal just before drilling and then they went back to the normal range during the drilling and immediately afterwards.

Then the number of species, the same kind of trend, it actually increased during drilling and then settled sort of a gradual increasing trend over the next several years.

So, basically at this site-specific array or at the station closest to the shallowest platform we monitored, there were, for all intents and purposes, no real biological impacts that we could attribute to drilling discharges.

Looking farther offshore in block 410, here drilling started just after our first cruise. It would appear that the diversity did a nose dive during drilling and perhaps there was some decrease in the number of species. Then if you look beyond this, there are seasonal trends that sort of obscure any change here that could have possibly been due to drilling activities.

For all intents and purposes, at this depth of over 100 meters, you don't see much of anything. The total average number of individuals remained essentially constant the whole time, the gradual rising trend, again, more typical of the lack of seasonal variation in offshore communities.

The one thing we saw that we thought was important initially was the amphipod species that are really sort of semi-epifaunal, they sort of attach and sit on sand grains on the surface of the sediment. There was a nose dive in their local populations. Again, this is at station 5 right near the platform.

The drilling period occurred right in here and these populations just basically vanished from the immediate vicinity of the drilling platform. Then in May, which is cruise number four, there is an increase again and then the populations went back up, generally.

Then looking on the longer scale, we can see seasonal patterns of this and it appears in many cases it's what's happening here because these are animals sitting right near the surface that major winter storms can redistribute these animals, perhaps wash them from one place and they set up home in another place.

You get this seasonally, and it turned out that in February of the first year there was a major winter storm, one of the worst in many years, and I know many of the people on the cruise will attest to the fact that it was not a nice period of time out there, so it is quite possible that some of this or most of this was due to changes in sediment texture due to winter storm events.

In any event, what all these biological results imply is if there were any effects directly attributable to drilling fluid discharges in accumulation of drilling fluid materials on the bottom, that these effects were very small in scale and were practically indistinguishable from natural variability in the benthic populations and that any impacts, again, if they actually did occur as a result of drilling, were very transitory and basically the natural annual cycles were back to their normal range almost immediately after drilling stopped.

So, for all intents and purposes there were no impacts on the benthos that were of any significance beyond the natural variability for the system.

Thank you.

DR. VALENTINE: Thank you, Jerry. Do we have any questions?

MR. VILD: In the studies that we're looking at, the winter flounder and the lobsters, you mentioned that the lobsters showed signs of stress. What sort of stress were you talking about, repressed respiration?

DR. NEFF: No, we measured growth and a few other things, some biochemical changes. The major thing was there was a slightly elevated mortality in the experimental group. It was significant, but it was small. Juvenile lobsters are hard to keep in the lab, so it's a difficult situation to assess that.

There was slightly elevated mortality and also the other parameters, there were slight differences in growth rates and food ingestion rates. As I say, these are fairly minor. Obviously mortality isn't minor, but in terms of the difference between the control groups and the other groups.

DR. BUTMAN: Could you put the block 312 slide up there with the one you had with the three indices before and after drilling? It seemed like two out of three of those showed an effect.

DR. NEFF: The 312 one?

DR. BUTMAN: Yes, I think that was the one. This looks like the upper one is low before and high after?

DR. NEFF: Yes, that's what I indicated. If there was an effect, it was to increase the abundance of animals and the number of species. It's interesting that that seemed to persist fairly uniformly for the rest of the period.

They remained higher and that's why I thought it may be that there was something anomalous here early on. I don't know if you want to comment on that.

DR. MACIOLEK: To put it in a slightly different context, we saw the same pattern at almost all the other regional stations that we sampled where the second and third years of the program we got higher abundances. Diversity was pretty much the same, maybe a little bit higher.

It occurred to me that if the program had started at a different point in time, say it started at the beginning of year two, where we started at a high point in the cycle, we could have gotten the opposite results; instead of having the drilling during a low year of abundance and diversity followed by two years of high values, we could have just seen the opposite because of a different point in that cycle.

I don't think we know how long that cycle may be. I think the main point is that when you consider the same pattern was seen in stations all over Georges Bank.

DR. BUTMAN: I thought you were saying that that wasn't a significant change? It is, it's just you can't correlate that?

DR. NEFF: We can't correlate it with the drilling discharges per se. We did try and do some correlations between increments in barium and biological responses, and none of those were significant. Again, that's a hard thing to do.

DR. RAY: Jerry, in the study you were talking about with the lobster effects, as I recall in that study you prepared those test sediments to mimic different concentration levels of contaminated sediment based on barium concentration?

DR. NEFF: Right.

DR. RAY: What was the level in those sediments in the tests where you saw the lobster effects that you were just talking about?

DR. NEFF: They were actually sort of worst case. We decided to go for the worst-case scenario. In the literature I've seen increments up to 10,000 ppm barium above background, that is near a production platform. It was in that range, 5,000 to 10,000 ppm barium above-background concentration.

So, this is three orders of magnitude higher than we saw on Georges Bank, for instance. So, that is an important consideration. We tried to maximize the potential impact, you might say, to see if we could detect a biological signal.

It's still a concentration that could be envisioned under worse-case situations where you're developing a large field. So basically we saw very little.

DR. RAY: Usually those concentrations are only seen in the very immediate vicinity of the deposition area?

DR. NEFF: Right, exactly. The one instance I mentioned, I think it was just one location had that kind of increment.

DR. COOPER: In your experiments with these juvenile lobsters as a function of barium, did you also have any of your other trace metals, copper, zinc, iron in there as well?

DR. NEFF: What they were exposed to was what we call the settleable fraction of drilling mud. That's the stuff that settles more rapidly than the light clay fraction. So, all the metals that would normally be in these drilling muds were there.

These were natural muds from the Gulf of Mexico, because that's where active drilling was going on at the time. They were field muds, they weren't made in the laboratory.

DR. COOPER: Lobsters are notoriously and extremely sensitive to zinc and copper in very, very low concentrations.

DR. NEFF: The evidence we've seen so far is that these metals like zinc and copper are not present in ionized form in ionic copper. Most of the copper is associated with sulfide minerals in the solid phase, so you don't get an increase in the ionic copper.

The key here is the form of the metals. You do get elevated metal concentrations, but most of them are in forms that are like the metal in your pocket in coins, it's in an unavailable form.

DR. MACIOLEK: Jerry, the studies that you summarized on impacts on benthic communities, can you tell us what water depth those communities were in that were significantly impacted? When I talked about recovery within a year, was that in terms of species diversity or abundances?

DR. NEFF: In the mid-Atlantic, the water depth was about 100 meters or so. That was a fairly quiescent area of high-clay fraction in the sediments. Obvious evidence of significant accumulation of drilling fluids. There was basically a mud and cutting pile next to the platform.

The major indices there were species abundance and diversity and so forth, species composition. In some locations right near the platform, everything was virtually wiped out because you've got a big pile of stuff.

The abundance of individuals and the diversity and so forth began showing changes almost--they did two cruises, one immediately before and immediately after drilling and one a year later.

Unfortunately, the big problem with that study is that they compared those results with a benchmark station nearby that was done several years earlier. It looks like the benchmark data were anomalous.

So, you are comparing with something that really isn't comparable. It wasn't a good control, so it's hard to say whether the immediately after drilling impacts that were observed, except those where everything was wiped out on the bottom, how big they were and what the normal population size was at that location.

So, that's a big problem. In the North Sea, most of the studies were in 50 to 100 meters of water, and there has been a lot longer follow-on on that, for several years in some cases.

DR. NEFF: Right.

DR. RAY: The two studies this morning I talked about, Nancy, two of those had biological studies associated with them, too.

In the preliminary information, one was in 73 meters of water and in that particular location the main effect that you can see is within about 75 meters of the platform, but the changes in the community seemed to be related to the changes in the bottom substrate directly beneath the platform, a combination of debris from the platform, i.e., manmade and some of the larger hard filing [words unclear] organisms associated with the platform, seemed to have changed the community composition directly beneath the platform slightly.

Out beyond about 75 meters, using the variety of abundance and diversity indices, there does not seem to be any correlation between that and any metal levels in the sediments and hydrocarbons.

In Alabama, in that study in 35 feet of water, there doesn't seem to be any association in the changes in community type associated with the contaminants that we're measuring in the sediments.

That's preliminary information, but that's what we're seeing. There are two more studies that Jerry hasn't see yet that actually have very detailed community studies associated with the chemistry.

DR. NEFF: You mean there's a study I haven't see yet?

DR. RAY: We're ahead of you, Jerry.

DR. NEFF: Were there some other questions?

DR. KRAEUTER: Are there any studies that have looked at recovery in the substantially deeper areas? These are all 100 meters and shallower where you would expect, perhaps, seasonal variation in things that would influence recovery, what about something 500 plus meters? Do we have any kind of information on that?

DR. NEFF: None that I know of. Of course, there is an ongoing program on the west coast. Unfortunately, they're not doing much drilling there right now so it's hard to study recovery. There has been some at platform Hidalgo, but that's mostly hard-bottom substrates around that.

So, inadvertently we're looking at recovery now, because had hoped that they would develop that field for quite a while yet. In the North Sea I think most of the depths are quite a bit shallower.

I'm not aware of anything in deep water in the gulf, which is where most of the deep water production and develop is going on.

DR. MACIOLEK: Just a comment on that. We did some recolonization experiments at about 2,000 meters. They were not so much in direct response to drilling or oil impacts, but I'll be talking about those very briefly later on.

DR. VALENTINE: Our next speaker will be Dr. Barbara Hecker from Lamont-Dougherty Geological Observatory. She's going to speak on "Megafaunal Populations in Lydonia Canyon, with Notes on Three Other North Atlantic Canyons."

PRESENTATION OF DR. BARBARA HECKER

DR. HECKER: The data I'm going to present on Lydonia Canyon was collected over a period of time in Lydonia Canyon. Basically it started with the Canyon Assessment Study, which occurred in '78 and '79, and then we did more work in Lydonia Canyon from 1980 to '83.

Basically these are five study areas that we did for canyon and slope. These are five study areas that we did for canyon and slope processes study. This is Lydonia Canyon up here.

Now, in several instances I will be comparing the fauna in Lydonia Canyon to slope fauna. The slope fauna that I'm comparing it to is in this slope area here, which is between Hydrographer and Veatch Canyons. So, basically the things is not just to look at Lydonia Canyon fauna per se, but also to compare it to what may be common out on the slope.

Basically we surveyed the fauna with photographic means, using camera sleds. Unfortunately this is not the camera sled we used in this study, but it's a camera sled we've been using. I grabbed the wrong slide.

This camera sled is designed to be towed along right on the bottom of the sea floor, riding approximately half a meter above the sea floor. It is looking forward and slightly down with lighting coming from the side.

What this gives you is this gives you very, very good, sharp pictures of the fauna in terms of corals, it gives you polyp structure, polyp arrangement, fin configuration on fish, so it's very helpful in terms of identifying some of the, particularly on the corals and that, some of the smaller sessile forms.

Also, the type of resolution we get with this type of photography is a lot better than we were able to get with the bow cameras on *Alvin*. So our resolution in terms of the very small animals living on the sea surface, or the slightly translucent animals is much better with the camera system.

This is named "Babs" and yes, it is for Barbara, but it stands for "Bad-assed Benthic Scientist." It was named on a cruise I was not out on and christened. They painted that on and that is not removable paint.

Basically we did 16 camera tows in Lydonia Canyon. We also dove with *Alvin* and we did 17 *Alvin* dives in the canyon. A total of 114,742

m^2 of the sea floor were analyzed. This means that we counted that many animals and probably the animals you're talking about may be about 750,000 animals that this data set is based on. That's a lot.

Some people say, "Hey, they pay her to look at those beautiful pictures of the sea floor?" Yes, they do. It's just as bad as looking through a microscope, especially after 8 hours of sitting in the dark.

The slides were projected and systematically analyzed, first depth was noted, then surficial geology was noted, any comments we had about current indications, current scour was noted. Any faunal associations in terms of, "Hey, this hanging around the base of this," were also noted and then all species were identified and counted for abundances.

This was done systematically for the photographs. The photographs were analyzed over a series of passes. Basically the first viewers go through and do the analysis of the photographs and then they write questions for me and I go through and recheck it.

Then, at the end, I go through every single shot and make certain that the area estimate has been done, because one of the problems with this is the canyon topography is very, very rough, so a lot of times you have to estimate as to how many meters you're looking at.

We do that based on where the horizon is in the photograph, and in terms of the fauna. We know basically the size of the fauna, so we frequently will have to estimate. It's only when you're on an absolutely flat surface that you don't have that problem.

I've color coded this slightly. The blue was the submersible coverage here using the *Alvin*. You can see most of that was done in the vicinity of the axis.

One of the reasons is Lydonia Canyon, in comparison say to Oceanographer Canyon, the axis is rather narrow, it's quite sinuous, and it is flanked along most of its extent by cliffs along the base of the wall, hence the camera slide doesn't always do such a great job in taking pictures.

We get some spectacular shots going up the cliffs. If you notice, several of the camera tows go right along the axis. This is just so that we could get good axial coverage. We had several tows going right along and then they would branch off at various places when we felt we'd gotten enough coverage of an area.

We also use the *Alvin* to collect voucher specimens of the critters because seeing it on a picture doesn't exactly tell you what it is, but if you see it on a picture and then you have an animal in hand and can key it out--so basically we use the *Alvin* to get areas of high relief and also to collect voucher specimens for species identification.

This is a slide just showing individuals--this is megafaunal animals you can see with the naked eye on the sea floor, basically, individuals/100 m^2 for the slope area, that's slope area 3 and Lydonia Canyon.

One thing that you can notice is that throughout the depth range, except for one little area right here, the canyon megafauna is much more abundant than on the slope. There are several reasons for this. Most of the difference was found in the axis of the canyon in the actual axis, but also on the flanks.

So, it seems that the canyon environment does extend out past that axial part and onto the flanks slightly. Basically these very, very high abundances here were due to what I think is that depositional area right in the axis. You've got a lot of sea pens in that area and you have very, very high concentrations of two brittle stars, two Ophiura species.

When I say very high concentrations, I say they were not countable, they were sometimes stacked five deep over each other right in the depositional part of the axis proper.

Then, some of the high concentrations here at 400 meters and say here at 500 meters had to do with another type of coral, a hard substrate coral, *Unephthia florida*, which is very common on boulders along the upper portions of the wall and also along the cliffs at the base of the wall.

In here the higher abundances with regard to slope were due largely to hard substrate corals that were restricted to hard substrates, and some sponges, but also a soft substrate sponge, *Asbestopluma*.

The higher concentrations down here, again, had to do with hard substrate corals. Down here the higher concentrations have to do with a deposit feeding brittle star, *Ophiomusium lymani*, which is common everywhere, it just happens to be more common in the canyon proper.

Just to give you an idea of the animals that I have been talking about or some of the animals and to give you an idea of what the canyon environment looks like, I'll show you a couple of pictures of some of these animals.

(Slide presentation)

This is the depositional area in the canyon that I was talking about, this is directly in the axis of the canyon and here is *Penatula achiliata* [phonetic], the sea pen, as you can see it's not restricted to hard substrates.

It has a bulbous base that extends down into the sediment and very, very high concentrations of this were found. It was frequently found in association with the two Ophiura species and in the same area. There aren't any on this particular slide.

This is a picture of a cliff at the base of the west wall of Lydonia Canyon at about 400 meters. The whole cliff base is covered by these large worm tubes.

Yes, Nancy, we have no idea what they are, just very, very large worm tubes just covering the whole cliff. Then you've got *Penilla recida* [phonetic], a large beautiful coral hanging down off the cliff here.

Then, this is going down the canyon a little further. This was along the east wall of the canyon where you don't have as sheer a cliff, but here a lot of *Unephthia florida*, they're hard substrate corals and a lot of sponges and hydroids and lot of growth.

This is in the same area you see there are a lot of sponges on the outcrops along the canyon axis. Slightly up on the wall, this is the sponge that I was talking about, this is *Asbestopluma* sponge, and this is common in the canyons. We do not find this anywhere on the slope, and I've looked in a lot of places on the slope.

It seems to prefer areas that are what we might call "soft substrate to the naked eye" but they're really very hard and there is a very firm attachment site, which is, I think, why it's restricted to canyons.

This is *Perimuricia grandis* [phonetic] and an apathella, these are various corals that are found on hard substrates, these are the common ones. Here is *Caraphenoides repestros* [phonetic], a rattail that was found in significant abundances only in the canyon.

This is a super-neat coral, *Peregorgia arborea* [phonetic], it's common in a lot of the canyons, all the way down to Baltimore Canyon. It's also common in one area up on the bank that's called "the trees."

These gorgoneans grow to about 15 feet tall, and the reason it's called the trees is the fisherman don't like to tangle their nets in it, but a lot of juvenile fish like to hang around these corals, probably in terms of protection, a heterogenous environment.

They were also common along the canyon axis, and here's just another picture. This is down at 1,200 meters, this is a cliff right at the base, the axis is down here. You can see all the corals, and then there are little brittle stars that are associated with the *Perimuricia*.

Then another coral that was common along the axis on the cliffs, but also up on the flanks on boulders is *Anthomastus agagazzi*, also very much of a canyon indicator in that sense.

Now, let's look at what this data looks like mathematically. Basically this is a slide showing what we did with the transects. We divided the transects up into 30-picture intervals, or intervals of different substrate.

Then, what we did was we did community analysis and this is a percent similarity index. I don't expect you to interpret everything in here, let me just point out several things.

You can tell the clustering structure is a function of depth, but it's also a function of location. The hot-pink areas, this is the number of areas. We started with a data matrix of 410 sample areas and 141 species.

The hot pink are areas that are only in the axis. Here is another group and here is another group that are specifically clusters that indicate axis areas. This indicates a shallow area and then this indicates a slightly deeper area.

This one is centered around the 200-meter isobath. This extends from about 350 to about 500 meters, and this one from about 500 to about 1,000 meters and then this one from 1,000 to about 1,500 meters.

Then you have the areas below 1,500 meters are very similar to each other in comparison to the other areas. Take a look at faunal--the percent similarity here. We're talking about faunal similarities of about 15 percent, which is not very high.

So, the picture that this gives of the fauna in the canyon is that it's very, very patchily distributed, as Dick was talking about this morning.

I'll show you what it maps out like because it's sometimes hard to follow the clustergrams. What it maps out at, basically, is the first cluster goes around the 200-meter isobath. It goes right around the rim of the canyon.

This is composed of fauna that are in the shallow water groups, say the jonah crabs that you saw pictures of this morning, *Actinauge verilli*, one of the anemones and the white hake, so you're talking about shallow water species, some starfish, asterias, and things like that.

That extends right around the rim. There are a couple of areas here on the west flank that had the fauna that was found along the rim, but also had additional organism like munida [phonetic], galatea crab and the burrowing anemone.

Then, the next cluster that you got was what I'm calling a zone 2, and that does not extend into the canyon at all, and then zone 3 does. Basically the quill worm and several anemones and several small solitary corals are characteristic of this area, as well as the red crab *Geryon*.

When you get down into 3, which extends down into the canyon in a narrow band and back out, you're talking about mostly carnivores, *Geryon quinqueidens*, the large red crab, several deep-sea eels and some rattails.

Also within you've got 3A and B, 3C was the *Asbestopluma* that sponge pen that I showed you that like the semi-consolidated sediment, that you would find on the walls of the canyon in this vicinity.

Then basically moving further down you have this large area which I've designated as zone 4. This does not go down into the axis of the canyon, and that is basically characterized by having low concentrations of deep-sea eels and having an occasional sea pen.

Then, when you get down into zone 5, which extends all the way into the canyon, below 1,500 meters, you do not get so much canyon slope differentiation, largely because that brittle star, the *Ophiomusium* that I mentioned. It's very ubiquitous, it is very, very common where it is.

That does mean that the fauna in the axis of the canyon at that depth was not different, it just means that it's numerically overwhelmed by the *Ophiomusium*.

Basically what I want you to get from this is the majority of the axis of the canyon cannot be characterized. These were the other clusters, and basically it jumps between 7A and 1C and 7A and you could not map these into a band. It's very, very patchy down within the axis of the canyon and up on the walls of the canyon it's exceptionally patchy.

What I'm also cautioning you about is within these regions here where I have mapped zones, I'm only mapping them saying, hey, the critters in this area are only 15 percent similar. This is not a very high faunal similarity.

So, basically what I want you to come away with is one, the animals are all very patchily distributed. Some of this can be explained by substrate differences of what is available, what attachment sites are available, but some of it cannot be.

The *Penatula achiliata*, that *Ophiura* species, the *Asbestopluma*, that sponge pen, this cannot be explained by substrate differences alone, and that they're other things going on in that canyon that are responsible for the patchiness and the distribution of these animals, also for having all of these filter feeders.

Basically the difference, and I'll show you that right here.... What I'm showing you here is trophic strategies, feeding strategies in the canyon versus the slope.

If you look at the pattern, a typical slope pattern here, you're dominated by filter feeders and deposit feeders, deposit feeders right at the base of the slope, a small zone of filter feeders and then you go up into a carnivore, say the whole middle slope here to upper slope, is dominated by carnivores.

You're talking about deep-sea eels, you're talking about the red crab and several rattails. That is sort of the pattern here on the slope, but when you get into the canyon, look at the high concentration of filter feeders that carries right up when you've got just and F[words unclear], you're talking about almost 100 percent filter feeders.

When you've got a C and an F, you're talking about a half-half mix. So, you can see the high concentrations of filter feeders in the axis on the flanks here and on the wall, and the same here on the east wall and the flanks, you've got very high concentrations of filter feeders.

Additionally, the problem with doing species diversity from the type of data we have is we have unequal sample sizes, because you're dealing with photographs and the number of--your diversity increases, but it doesn't increase linearly with the amount of area you're looking at.

So, we did just some very simple mathematics of just trying to calculate the number of animals per 10 m. One thing that we did find out, there's a higher diversity on hard substrate.

You can see this here, this was on the slope, it was the eastern part of that slope run. The mean number of species per 10 m there was 1.15. This was the western part of the slope, there were several one-picture areas that had boulders on them, and the diversity went way up, right off the graph.

The soft substrate areas here had a mean diversity of 1.65 number of species per 10 m^2. When you got into the canyon, the average diversity along here was 2.03, 1.19 in the soft substrate areas and 2.91 in the hard substrate areas.

This was just cobbles. When you got into the canyon axis here, the mean diversity was 4.37, it was 1.86 on the soft substrate, it was higher in the soft substrate in the axis than it was anywhere else on soft substrate, but it was 8.31 on the outcrop, and here you're talking about large cliffs.

One thing we did notice you had the same pattern here of going from 3.56 on the soft substrate to 5.50 on the outcrop. One of the things that we found was that your species--that your species diversity went up with the size of the outcrop.

We have a feeling that this has to do with--if you're talking about cobbles versus large boulders and cliffs and that you're talking just about a large cobble cannot support some of the biomass you're talking about and these animals don't survive and then would fall down.

The idea is you have a higher diversity in the canyons, part of this is due to the substrate heterogeneity of the canyons, but not all of it. Even the soft substrate areas have higher diversities.

I was also analyzing a data set from several *Alvin* dives from three canyons that were done in 1977. Basically Lydonia, what I was just talking about is right here. We had three *Alvin* dives in Oceanographer Canyon, one dive up the wall of Oceanographer, the west wall, and two dives in the canyon axis from about 1,800 meters to 1,500 meters.

We had three dives in a very small canyon called Heezen Canyon over here. Again we did the west wall and two in the axis, and then one up the wall of Corsair Canyon.

These canyons were very, very different. Oceanographer Canyon is a rather large canyon, it has a relatively broad, wide axis, which has lots of ripples in it. It's a lot of sediment and the cliffs along the edge flanking it were not very pronounced or were low and stepped.

What was interesting about Heezen Canyon is Heezen Canyon is a very, very narrow canyon. At times the submersible is maybe 3 meters wide. At times we were risking going up the axis, that is how narrow it was. Several times the submersible pilot insisted on coming off the floor of the canyon so that we wouldn't be stuck under underhangs.

These were Eocene chalk cliffs, and this was just a spectacular, very, very narrow canyon. It does not incise into the slope very far. Corsair Canyon was rather nondescript and did not have that much hard substrate available.

What I want to show you from this data set basically is if you know one canyon, you don't know the fauna in all of them. Each of them is different. When you're talking about patchiness, you're talking about patchiness within a canyon and you can also talk about patchiness between canyons.

Knowing the fauna along a canyon wall does not tell you that the canyon one canyon over is going to be the same. A lot of these differences do not have that much that to do with hard substrate availability.

So, there are other physical factors going on that are controlling the distribution of some of these animals. This is one of the animals that made a difference. I'll show you the patterns in a minute, I just want to introduce the animals to you.

This is *Acanilla arbuscula* [phonetic], soft substrate coral gorgonian. This is another one, *Anthomastus grandifloris* [phonetic], again soft substrate. Another one, this is *Penatula grandis*, again soft substrate.

This is a hard substrate one, *Desmophilum cristigalae* [phonetic], which is usually found on the underhangs of large outcrops. It's also found on some boulders, but it seems to prefer downward orientation because of sediment loading.

DR. COOPER: Barbara, is this Heezen Canyon here?

DR. HECKER: This particular picture is Heezen Canyon, yes.

Here's another one of those clustergrams. Basically, what I just want to show you here is the areas that are shaded with a color are shaded because they're composed of areas that are only from one canyon or let's say primarily.

If you notice here, this is *Asbestopluma*. Basically the critter that is responsible for these areas clustering that tightly together is *Asbestopluma*, that that's sponge pen. It's very characteristic of Oceanographer Canyon.

In fact, it dominated Oceanographer Canyon, the wall that we went up, from 650 to 1,300 meters, it was just found--very low concentrations of this were found in just one little place on Corsair Canyon, but again, very low concentrations.

Basically, this part of cluster 3 is characteristic of Corsair Canyon and this whole part of the wall of Corsair Canyon from 800 to 950 meters was dominated by *Penatula grandis*. Down here, this yellow cluster here was characteristic of Heezen Canyon, that's that narrow canyon.

It was characteristic of the axis where you have a star here, you're talking about an axis area. This was very, very characteristic of that white Eocene chalk, and that's because *Anthomastus agazzisi*, which I showed you before, which is a hard substrate red coral, was very common here.

In the deeper areas you had the *Desmophilum cristigalae* and *Perimuricia grandis*, which was found in both Heezen Canyon and in Oceanographer, but not in Corsair, because we did not find very substantial outcrops in that area.

Just to give you an idea, I tried mapping this out for you. This basically the wall run of Oceanographer Canyon, heavily dominated by that sponge pen and then you had another sea pen. This *Asbestopluma* was very characteristic of Oceanographer Canyon. You only saw very few of them up in Corsair Canyon.

When you're looking at Heezen Canyon, you went from *Acanilla arbuscula*, which was that bushy coral, into an *Anthomastus grandifloris*, dominated back into an *Acanilla arbuscula* and then down into *Ophiomusium lymani*, which is that brittle star and them *Distocoplum gersilli* [phonetic] and then back into *Ophiomusium lymani*.

Basically when you got into the axis itself here, you can see the *Anthomastus agazzisi* and the *Anthomastus agazzisi* by the yellow, and that's because of the Eocene chalk cliffs. They were just covered with this organism.

Then, the *Desmophilum cristigalae* and the *Perimuricia grandis* dominated down in the bottom of the axis. Basically what you had on Corsair wall was you had a little bit of *Asbestopluma* up at the top, but then you got *Unephthia florida* and they inhabited glacial erratics on the upper wall of Corsair Canyon.

Then you went into *Acanilla arbuscula*, which is similar to here. Then when you got into the *Anthomastus grandifloris*, you had a lot of *Penatula grandis* in addition, and then you had some *Unephthia florida* on the hard substrate. You went back into *Acanilla* and then you were basically into the *Distichopilum versilli*.

One thing that is interesting about the status is that if you look at the *Ophiomusium* areas, you notice that they go very shallow in Heezen Canyon.

We found the same sort of pattern out on the slope and we feel that what may be allowing *Ophiomusium* to go further up on the slope is that what you've got is a lot of outcrop here and you have a very heterogeneous environment and we feel that that may be affording *Ophiomusium* protection from predators.

Out on the open slope, it does not extend up the slope, it will only extend up onto the middle slope and areas where we have a lot of hard substrate.

Basically in conclusion I would like to say the faunal densities in general are higher in canyons that on the slope, the distribution of the megafauna in Georges Bank canyons is very complex with a high degree of patchiness in many of the faunal constituents.

In addition to that, most of the canyon fauna is dominated by sessile filter feeders, be they hard or soft substrate organisms, but sessile filter feeders, corals and sponges.

These patterns reflect substrate heterogeneity, but they also reflect other physical parameters, namely the ones we were hearing about this morning in terms of the enhanced current speeds, the complicated pattern of current intensities in the canyon and I feel the resuspension events.

Mike, that's where my question was coming up with the regard to the resuspension events. One of my feelings is that in order to support a lot of filter feeders, not only do you need high current intensities to bring particles past, but you need a constant influx of particles into the water column in that area to support these types of populations.

In terms of, if you want to look at possible impacts, the thing there is just that filter feeders would be liable to a fair amount of stress that would increase the sediment load in the water column. That would be tissue abrasion, clogging of filtering apparatuses, but also because they're sessile, possibly decreased settling success.

That would only mean in terms of if you increase a lot suspension in the water column, but also then you've got to raise the question of if, in fact, we are concentrating things in the access of canyons, these filter feeders may well be the base of this food chain.

DR. BOTHNER: Barbara, can I ask you one technical question? Your sled, I wonder if that is driven blind or does it have a TV camera that let's you view what the sled is seeing on the ship?

DR. HECKER: It's driven blind. It takes a photograph every 15 seconds. The one thing that I found with videos is you cannot get independent results if you've got any feedback going through what you're looking at because you're taking more pictures--I see this from submersible dives--you're going to be taking more pictures of the interesting areas.

If nothing is there, that is data also. So, it's absolutely blind and it's every 15 seconds.

DR. VALENTINE: Our next speaker is Dr. Nancy Maciolek formerly of Battelle Ocean Sciences. She is going to speak on "Benthic Infauna of Lydonia Canyon and the Adjacent Slope Environment," and also give us some information on Fred Grassle's work.

PRESENTATION OF DR. NANCY J. MACIOLEK

DR. MACIOLEK: My presentation today is based on the results of programs funded by the Minerals Management Service, the Georges Bank Benthic Infauna Monitoring Program, conducted from July, 1981 until June, 1984, and a deep water characterizations study that was conducted between 1984 and 1986.

The work was carried out jointly by Battelle Ocean Sciences and Woods Hole Oceanographic Institution.

In both programs the benthos was sampled quantitatively at several stations, including stations in Lydonia Canyon and on the adjacent slope. Those canyon/slope stations were at three different water depths, approximately 150 meters, 550 meters, and 2,100 meters.

The number of times a particular station was sampled ranged between three and eight. Because we saw very few seasonal effects, the way I'm going to present the results to you today will be essentially averaged over time for each particular station.

Now, in order to assess the infaunal benthic community structure, we looked at the species that were present in each of our samples and the abundance, so we did counts for each of the very tiny organisms that we were looking at.

Unlike the people that look at the megafauna, our animals are microscopic and very numerous and we spend a lot of time identifying and counting them. We then use these data to look at which species were the numerical dominance at stations on each sampling date and for the station over all the times sampled.

We looked at diversity using both the Shannon-Weiner Information Index, and the Hobert Rarefaction Method. We also look at similarity among samples and stations in order to look for patterns or relationships among stations.

As Page mentioned in addition to reviewing this type of information today, I'll also give you some information on the recolonization experiments that we did at 2,100 meters.

(Slide presentation)

At the 150-meter depth in Lydonia Canyon, we had two canyon stations at 150 meters. One of them was sampled only four times because we realized we were actually on the wall of the canyon, and the sediments there were very highly variable.

This photograph was taken in November, and this was the photograph taken in February. In general, though, the sediments at this station were very coarse and contained less than 1 percent silt-plus-clay.

After the fourth sampling date the canyon station was relocated to an area of finer sediments, as seen in this slide, unfortunately we didn't pay much attention to the large crab in the picture, we were more interested in the animals you can't see under the sediment surface.

This station had on the average about 30 percent silt-plus-clay. The station that was located on the slope outside the canyon had fine, sandy sediments with about 2 percent silt-plus-clay.

The small objects on the sediment surface here are small onufid [phonetic] polychaetes called *Nothuria britannica* [phonetic] that were very common in photographs from this station on the slope during all the seasons that we took photographs. We didn't see this species in the canyon.

We found that species composition at the three stations was somewhat different, especially so between the canyons, the coarse sediment canyon station and the fine sediment canyon station.

At the coarse station, the dominant species was a polychaete called *Lumbrineris latreilli*, which accounted for 7 percent of the total fauna that we collected at that station. The rank of several of the subdominants at the station varied widely over the four seasons or the four sampling dates.

Rather than this being a seasonal effect, we suspect it's because we really were sampling a different location and sediment type each time that we went back.

At the station with finer sediments, which was sampled during eight seasons, the top dominant was the arthropod *Ampelisca agassizi*, which accounted for close to 12 percent of all the individuals at that station. This species at this station consistently ranked either first or second except on one occasion when it dropped to fourth.

On the adjacent slope station we had the same species occurring as the top dominant on each of the 12 sampling dates. At the slope station, however, that species accounted for something like 35 percent of all individuals at the station.

If we compare the dominant species between each of the two canyon stations and the slope station, we find that there were more dominants that were shared in common between the coarse sediment station and the slope, that is of the top 20 dominant species at coarse sediment canyon station, 13 of those were also dominant at the slope station.

When you make the same comparison for the fine sediment canyon station, you find that only 4 of the top 20 were shared in common between the two.

I would like to make the point that it's not so much that we had entirely different species composition at the different stations, rather what was happening was that the species were present in both places, but in very different numbers, very different abundances.

Therefore, what we saw as dominant species at one station were not dominant at another, although they might be present but in very different numbers.

Density was also highest at the fine-sediment canyon station, an average of over 13,000 individuals per square meter as compared to 9,000 or 8,000 individuals per square meter at the other two stations.

Diversity was measured, as I said, using both Shannon-Weiner and the Hobert Rarefaction Method and we got kind of mixed results at this particular location. Using the Shannon index, the coarse sediment canyon station had the highest diversity, 5.41, followed by the fine sediment canyon station and then the slope station had the lowest diversity of the three.

Using a different method, the Hobert Rarefaction Method, we got essentially the opposite result. The line labeled station 8 represents the slope station, and it has a higher diversity than either of the two canyon stations.

In this method what we're doing is calculating the number of species that we expect to find in a set number of individuals from the station. We calculate this for a range of individuals from 50 up to the exact number that was found in a particular station.

That is why you see dots along these curves. These represent individual points that were calculated in order to draw the curve. Station 8 on the slope had a much higher diversity. Its curve is much higher than either of the two canyon stations, which are labeled 7 and 7A.

For reference, the two lines on the bottom of the slide labeled 13A and 13, represent stations in the mud patch. The most diverse stations in this particular slide were at about 145 meters on the slope. In fact, station 16 was one of the drill sites, it was a site in block 410.

For the similarity analysis one of those clustergrams again. First I just want to show you, if you look at the top part of the slide, and keep in mind the lower the crossbar, the more similar the two samples, because it's on a scale of zero to one. We're using a NESS similarity index. It's not the same index that Barbara used, so the scale looks a little bit different.

The closer any two stations are to each other and the lower the crossbar, indicates that they are more similar to each other. The bottom part of the slide shows you the station--it's an indication of the station depth and the sediment composition.

The thing to note from this particular slide is that none of the three stations we were looking at 150 meters were at all similar to each other.

Here's our fine-sediment canyon station and what this tells us is that it's more similar in terms of faunal composition to the mud patch stations. Station 7 here is that coarse sediment station on the canyon wall. It's most similar to a station that was near the head of Oceanographer Canyon.

Station 8 is our slope station, and it's clustering very tightly with the group of slope stations at about 145 meters. At 150 meters our stations all look very different from each other, in spite of that dominance by *Ampelisca* at both the slope station and the fine sediment canyon station.

At 550 meters we have one station in the canyon and one station in the slope. Again we see that the faunal dominance and composition was very different between the two. The same small polychaete, a seratulid called *Tharyx baptistae* was dominant at both stations, but again the percentages were very different.

In the canyon, this one species accounted for 32 percent of all individuals collected at the station. At this particular depth, this is a most unusual result. You don't expect to find a community dominated to that extent by one particular species.

The same species was dominant on the slope, but it accounted for only 6 percent of all the individuals at the station.

In terms of looking at the top 20 dominant species and how many were dominant and were shared between the canyon and the slope station, we find that there were 6 of the 20 in common between the two.

As we saw at 150 meters, the density/m^2 is much higher in the canyon that on the slope. In fact, it's not quite doubled but it's getting there.

In terms of diversity, this time the Shannon Index and Hobert Rarefaction Method gave us similar results, and that is that the canyon station had a lower diversity than the slope station. The Shannon values are 4.66 for the canyon and 6.0 for the slope.

On this slide we see the Hobert Rarefaction curves. Station 4 is our slope station and station 7 is our canyon station.

You can see that the curve for station 4 is almost identical to that for station 12, which was another station at 550 meters of water depth, but it was several kilometers distant from our Lydonia Canyon station pair. Just for reference this station 11 was at 225 meters on the slope.

When we looked at similarity, we did look at all the stations in the program, but this particular slide examines only stations at 550 meters, and unfortunately I forgot my colored pens or I would have indicated that this group here joined at this level, are all of our samples from station 4, which was our slope station.

The station that it is most similar to turns out to be station 12, which was at the same depth, 550 meters on the slope, but several kilometers distant.

It's not at all that similar to our canyon station, station 7, which is indicated here. You can see the level of similarity is just slightly over .4.

Turning to the 2,100 meter depth, this slide is from Barbara Hecker's photo transects, and it's at about 2,100 meters or 2,160 meters in the canyon. Barbara had labeled it crinoids, so I assume that these are the crinoids that you had indicated.

At the slope adjacent to the canyon, we had two stations that were located on the slope outside the canyon. They both have this type of surface feature, fairly fine-looking sediments, the *Ophiomusium*, some mounds, some burrows, so there is some biological features, but essentially the two stations look very similar in these photographs.

At 2,100 meters, we find a very different story that we saw either at 150 or 550. At this depth we have the same small polychaete, *Aurospia dibranchiata* as the dominant at all three of the stations. It occurs in about the same proportion at each of them, that is it accounts from somewhere between 8 and 11 percent of the community.

This same species is the top dominant in the infaunal benthos, essentially along the east coast of the United States at 2,100 meters depth. While I can say it's at least from the U.S.-Canadian boundary down to as far as Cape Hatteras and then some other organisms take over some of the dominance of the communities.

This is a very common species and we can see that as far as the canyon or the slope stations are concerned, it's dominant at all three of them.

Furthermore, if you again look at the top 20 dominants at these three stations, 11, depending on which slope station you're looking at, 11 or 12 of them are shared with the slope. In other words, the communities in the canyon and on the adjacent slope are very, very similar at this particular depth.

Also, the total density per meters square is fairly similar amongst the three. It's just slightly higher in the canyon, but I don't believe that's significantly different from what we saw in the slope.

DR. BOTHNER: Nancy, are those station number 8, 5 and 6 by chance?

DR. MACIOLEK: Yes, station 8 and I don't remember which is which, I think this is 6 and this is 5.

The diversity, here we see that the canyon station has the highest diversity in terms of the Shannon Index. Again, looking at the rarefaction curves, we see the same thing. Station 8 is the canyon station and, in fact, all of the other stations on this particular graph are at 2,100 meters.

They range from station 2, which was at the U.S.-Canadian boundary, to 5 and 6, which were just outside Lydonia Canyon, and 14 and 15 which were along the transect line, the line that was established by the DOE study.

So, you can see here that the canyon station is more diverse than any of the 2,100 meter stations sampled in the program.

In terms of similarity, this clustergram shows all of the stations sampled in the North Atlantic Deep Water Program. Over here on the left are, again, the 550 meter stations, showing that station 7 in the canyon is very dissimilar, it doesn't cluster with our canyon station, except at a very low level.

Over here, this group, are all of the 2,100 meter stations. Station 8 is represented by this line. It is most similar, as it turns out to those stations that were on the SEEP transect line, but the level at which it picks up and joins with the Lydonia slope stations is greater than a .8 NESS level of similarity, which says to us, in fact, all of those stations are very highly similar to each other.

So, just to summarize those results then, at 150 meters and 550 meters depth, we see some major differences in terms of the community structure between the canyon and the slope stations. These differences become very minimal at 2,100 meters.

Now, there can be some speculation as to what's causing these differences and in some cases, for instance, at the 150-meter stations we think that there are very subtle effects due to differences in sediment texture.

Apparently if you have a station with at least a small percent of silt-plus-clay, you get the *Ampelisca* coming in and perhaps being dominant. The underlying differences at those stations may be related to some differences in sediment texture. It's not quite clear at that depth.

At 550 meters, the sediment texture was very similar between the canyon station and the slope station and yet we saw some very major differences in terms of the percent that the dominant species accounted for at each of the two stations. There were very dominants shared in common.

We think that obviously rather than sediment playing a major role here, it perhaps could be the current regime at that depth in the canyon. Also, Barbara Hecker's results indicate some major differences in the epifauna at that particular depth. There are more *Geryon* on the slope, so there could be differences in predation pressure as well.

At 2,100 meters, Brad, correct me if I'm wrong, I guess we don't know that much about subtle differences in the current regime. There don't appear to be differences between the canyon and the slope at that depth. The sediments are very similar, the communities are very similar.

That concludes the information on the infaunal community structure. I'd like to just give you some results of the recolonization analysis and I can answer questions on either of the two topics later on.

Fred Grassle was responsible for recolonization experiments that were conducted both in our North Atlantic study region and the mid-Atlantic study region. He had asked me to be sure to make two points in particular today for him.

One was that one of the reasons--well, we addressed two major concerns in these particular experiments. One, of course, the primary one was to look at rates of decolonization in the deep sea.

There was also another question as to whether the design of the experiment trays that were used were appropriate. This slide shows you the type of design that has been used in most of Fred's recolonization work. It's essentially a rectangular arrangement of trays.

In most cases some of the trays will be filled with sediment that's been frozen and then thawed to make sure there are no living organisms in it. Other of the trays will be left empty. This is the lid that ultimately fits down and covers the trays as they're being lowered through the sediment column.

Most of the work on recolonization rates in the deep sea has been done with this type of design. One of the concerns had been whether or not the flow of water over and through this type of structure really was similar to what occurred in natural circumstances or whether some artificial barriers were set up.

So, part of the study was to evaluate the design of the tray and to come up with a redesign. That particular piece of work was carried out by Cheryl Ann Butman at Woods Hole Oceanographic. If anyone wants any information on that part of the work, they need to speak with Cheryl Ann.

We have a chapter in our report that discusses the results, but I would not presume to get into that aspect of the study.

However, I can show you the result of the redesign, and that was to come up with a circular arrangement, rather than the rectangular arrangement.

As you can see, all of the sediment is now positioned in the center of the whole array. This design was to insure, based on Cheryl Ann's experiments, that the flow of water over the sediment was as close to the natural conditions as possible.

The way the trays were arranged in that central area was in this sort of arrangement.

In our mid-Atlantic study, we used a combination of the old design and the new design. In the North Atlantic at the Lydonia Canyon area, we had the old design trays positioned at stations 5 and 6, which were at 2,100 meters just outside the canyon.

This tray that was at station 15, I believe, was the new design. As you can see, the trays were left out at two of the stations for 7 months and at station 15 they were left out for 14 months.

Now, the thing to point out here is that these are average densities in the trays calculated per square meter. The surface area covered by the recolonization experiment itself was about .25 meter, so this is some extrapolation from the quarter meter that we were actually sampling.

You can see if you put out defaunated sediment for seven months, you come back with an average density of somewhere between 33 and 37 individuals/m^2. Keep in mind that the densities I showed you for the same depth, 2,100 meters, are closer to 4,000 individuals/m^2.

So, this is a fairly significant difference compared to the natural community. There were so few individuals actually in these trays that it's difficult to say anything about the species composition.

The few things that we can say, the only species that was represented at stations 5 and 6 by more than one individual in any particular experimental tray, was the species of capitella, which is known from shallow water environments as a fairly opportunistic polychaete.

The only other organism that was represented by more than one individual was a tunicate called dicarpa. At station 15, in which the trays were left out for 14 months, the average densities were close to 416 animals/m^2, and again, that's compared to 4,000 individuals/m^2 in the natural environment.

Here capitella was again a fairly common species that came into the tray, and the second most common species was the polychaete *Aurospio*, which is the same species that is the community dominant in the natural sediments at that particular depth.

The question is: Were the old-style trays really giving us a correct understanding of how fast things were settling? Was there some recolonization in the sediments?

These are regression lines. The solid line is a regression line based on Grassle's results from one of his permanent stations at 1,830 meters depth. The dashed line is the same sort of result from 3,600 meters depth.

The X here represents our result from station 15 that I just showed you, and the two almost superimposed X's here represent the results from stations 5 and 6. The open circles represent results from our experiments in the mid-Atlantic, and they fall pretty much, whether they are new trays, old trays, it doesn't seem to make much difference.

Fred's conclusion, and I concur with him, is that the old trays were giving us a valid picture of the rates of recolonization and the new trays are providing a very similar assessment of the situation.

If you look at the lines and look at the numbers of individuals, and here we're looking at per quarter meter squared versus the number of months deployed, I think you'll agree that if any community at 2,100 meters depth is seriously impacted to the point of being completely wiped out by any sort perturbation, it's going to take on the order of several years to return to its natural state.

That's all. Thank you.

DR. VALENTINE: Any questions?

DR. TEAL: Nancy, there's a big barrier on the edge of those trays that the animals have to cross in order to get into them. I can't remember, are those animals planktonic for a short period? Do they move in the water when they're colonizing?

DR. MACIOLEK: For most of the deep-sea species, we know very little about their life history. Most of them, I suspect, would probably--some of them might, in fact, have planktonic larvae, others come in as adults.

One of the common things that we saw in the trays was an ectoparasite of a fish. Those, perhaps, had just come in accidentally in some way.

DR. TEAL: Have there been any experiments where you put in some defaunated sediment not in a tray and then just come back and sampled it with a corer or something, so that there is no artificial barrier?

DR. MACIOLEK: I don't recall any. You might want to ask Fred that when he's here tomorrow. I know in some shallow-water locations Whitlatch [phonetic] put out some sediment cores that I think he sort of sunk into the--I don't want to say ocean bottom because he was in shallow water.

DR. HECKER: Craig Smith has been doing something putting mounds down, but none of that work is done yet.

DR. MACIOLEK: The mounds in themselves are an artificial sort of structure, too. If you want to talk hydrodynamics, talk to Cheryl Ann.

DR. KRAEUTER: Don Bosh [phonetic] and I tried to do some of this on the shelf in the mid-Atlantic and one of the big problems, I'm just struck by your comment about the fish parasites, is you're making an artificial reef when you put a structure down there.

It's very difficult to figure out if whether what you're seeing is the effect of just fish moving around over the sediments and things are a disturbance or other things.

I can still remember going down, I think it was with Brad Butman, looking at his current meters on a dive many, many years ago and watching the current meters going around with fish sitting in the Savonius rotor parts. That's the kind of thing that happens out there.

They make me very leery of trying to do this kind of recolonization thing. Whenever you put anything out there, that happens. I don't know what that does, I don't know how we get around it.

I think John's suggestion of just putting sediment out there and then trying to find it again may be a good one, because you might get very different rates.

DR. BUTMAN: If I could make one comment on the trap design. The idea of making that long, thin lip was so that the boundary layer structure does not change as it goes over this. So, at least you don't have big eddies being shed over the traps. You're right, you probably still have, if you attract large fish or something else to that structure, that's a whole different story.

For planktonic organisms the idea is to try to keep the boundary layer the same across there.

DR. TEAL: If all they move is a centimeter, then there's a hell of a barrier you've put down there.

DR. VALENTINE: I'd like to thank all the speakers and this is the end of our early afternoon session. We'll take a short coffee break and reconvene at around 4:00.

(A brief recess was taken.)

MR. LANE: I'd like to get the final afternoon session started, if I could. We were scheduled to have presentations by representatives of the States of Massachusetts, Maine, and Rhode Island. Unfortunately, Katrina VanDusen can't be here. I understand her child is ill and probably she won't be able to make the session.

The last session today will deal with the State perspectives on submarine canyons and the impacts of drilling operations around those canyons. I'd like to start off with Pat Hughes from the State of Massachusetts.

PRESENTATION OF MS. PATRICIA E. HUGHES

MS. HUGHES: As many of you remember, actually in late 1983 and through most of 1984, the Minerals Management Service worked on, along with the National Marine Fishery Service and the U.S. Geological Survey, worked on the development of a stipulation that prohibited drilling within 200 meters of the submarine canyons in the North Atlantic OCS planning area, and further established as a part of the stipulation a requirement that there would be monitoring of any exploratory drilling activities that occurred within 4 miles of the submarine canyons.

The canyon topography was defined by people from the National Marine Fisheries Service based on some biological criteria with the assistance of the U.S. Geological Survey.

This no-drilling stipulation was established for three principal reasons. One was to protect the unique biological habitats of the canyons.

I think that today we've heard Dick Cooper, Barbara Hecker, and Nancy Maciolek discuss the biological aspects of the canyons. I think Dick and Barbara's videos and the pictures have really vividly shown the unusual habitat types and the species that occupy the canyons.

Dick indicated in the very beginning of his talk that the canyons had a very unique habitat and indicated that the canyons were important nursery areas for a number of species.

Brad described the submarine canyons as complex environments, and I think that Brad, Page Valentine, and Mike Bothner really further defined some of those sediment transport mechanisms that are work, at least in Lydonia and Oceanographer and some of the likely sources of material.

They indicated that while the energies in the various canyons studied do vary, pollutants, at least indicated by Mike's work on plutonium and lead-210, may actually be trapped within the canyons and they slosh around in there. There is some question as to how much of the material moves out and also what the time might be--residence time might be of some of this material.

Brad did point out in response to a question that was asked that we still don't know what the rates of accumulation of this fine-grained material may be. That's obviously important to trying to predict what some of the potential impacts of pollutants might be in the biological communities.

The second reason that the no-drilling stipulation was put in effect in 1984 was the protection of important biological resources. Again, I think Dick and Barbara have described them pretty well.

We saw pictures of lobsters, tilefish, jonah and red crabs, and I think it's fair to say that within this context the important biological resources have been pretty much defined by those that have been or are being exploited commercially.

The third reason that the no-drilling stipulation was required was to avoid any kind of spacial exclusion of fishing activity, and also to minimize conflicts between the conduct of fishing activity, particularly the pot fishery for lobsters and red crab and the long line fishery for tile fish and sword fish... minimize that kind of activity with any petroleum activity that occurred.

That's not at all been the focus of this discussion today, for obvious reasons.

The 200-meter distance was established based primarily in part on the National Academy of Sciences report, "Drilling Muds in the Marine Environment." They found that in general the cuttings were deposited fairly close to the drill site and the 200 meters pretty much encompassed the area.

That was principally to avoid direct smothering of the biological community and direct destruction of any habitat. It will come as no surprise to most all of you that it's my feeling that the information presented today, almost 5 years after the original no-drill stipulation was proposed, that if anything the information that we've heard has reinforced the reasoning for having a no-drill stipulation inside and adjacent to the submarine canyons.

I think it's fair to say that if there was not a no-drill stipulation and we were facing--let's say we'd already had a lease sale and there were some blocks that were sold that had submarine topography in them, I think it's likely that these submarine canyons would be viewed as areas of special biological significance.

I wanted to get the word in clear, that's how it is defined in stipulation two, which is the biological stipulation, and that obviously stipulation two would be invoked and I think it's likely that the debate would then ensue on whether or not they should be allowed to drill inside the canyon, and if they were allowed to drill, should they be prohibited from discharging.

If they were allowed to discharge, even if it was adjacent to the submarine topography, it's likely that there would be very restrictive limitations placed on the activity and monitoring.

That has traditionally been the way the Minerals Management Service has handled drilling in controversial areas. It's been, "Let us get in there and we'll place all these restrictions on the activity so that you really have nothing to worry about."

I think it's probably fair to say in many areas that that has been somewhat of a sensible way to go. I do think, however, that the information that we had up to 1984, and I think the information that we have today, as we sit here in this room, argues for actually not allowing drilling within the submarine canyons and perhaps we could discuss tomorrow whether or not this 200-meter no-drill zone is sufficient, given the presentations that we've heard today.

I would like just to close by saying that while most of the--really I guess all of monitoring work to date on the U.S. OCS has focused on exploratory drilling activities. The potential impacts of development and production I think are ultimately what most people are concerned about.

I think, again, that given the information that we've been presented on the transport of material, the pollutants attached to the fine-grained sediment, the question of the fate of this fine-grained material within the canyons, the fact that this is unique habitat, there are species in the canyons that are found nowhere else on the adjacent shelf and slope, might argue that it's further argument for preventing these areas from being leased in the first place.

Thank you.

MR. LANE: Are there any questions for Pat? Thank you, Pat.

Next we're going to hear from Bruce Vild from the State of Rhode Island about the State of Rhode Island's perspective on the same issues.

PRESENTATION OF MR. BRUCE F. VILD

MR. VILD: Thanks, Jim. Good afternoon and a belated welcome to all of you to the State of Rhode Island.

I was pleased to hear what Don Aurand was saying this morning that our assessment of the two hypotheses in this particular workshop will not be taken as a determination of policy for the governors represented here today, because I'm certainly not prepared to make such a commitment for my governor.

It's good we're talking about science, but we need to remember that science is only one facet of the controversy. Yes, there are heavy political pressures on the governors from the fishing industry in New England and from the environmentalists, and there is the question of economics.

Does economics, at this point and time, favor offshore oil and gas exploration in general, let alone in the canyons? There's also public opinion. Public opinion favors the fisherman.

New Englanders are suspicious of the oil companies, and by extension, the Interior Department. Anybody who doubts what I say should come to one of the public hearings that they have in any city or town in New England about any aspect of the offshore drilling program, whether it's the 5-year program or a specific lease sale.

At these public hearings you'll see people who do represent the fishing groups and do represent the environmental community get up and voice their opposition to the proposal. You'll also see members of the general public doing the same thing.

That's because in New England there's a genuine emotional attachment to the fisherman. The fisherman is seen as a rugged individualist, one of the last of the dying breed, really, who has been around for 300 or more years, who has faced all sorts of adversity, and really faces adversity every day when he's out on a fishing trip trying to bring back a quality product for all of us.

Facing lousy whether and so on and so on, it's a romantic notion, I grant you, but it's a notion that I think is very deeply held by every New Englander, including, I dare say, myself.

The fisherman is also an important part of what New Englanders like to call the quality of life, the sorts of things that make this region unique. In that quality-of-life concept, there's also a notion that there are certain quiet places in the world that should not be disturbed or should be left for traditional uses.

Again, this is a romantic notion, it's a nostalgic notion, but it's a notion that really has to be dealt with if we're to talk about anything as controversial and as emotionally charged as drilling in the submarine canyons.

The fisherman, as I mentioned before, is looked upon as beleaguered, and the environmental groups are looked up as the fisherman's champion, so there seems to be a natural affinity in the view of the general public, anyway, for the two groups.

The point is, whether we like it or not, supporting offshore drilling is unpopular in New England. So, any policy that a governor takes endorsing OCS exploration, has to be tempered not only by environmental considerations, not only by scientific considerations, but also by political ones.

For my governor or any governor to reverse his policy on drilling in the canyons, will require far more than a statement that such activities are relatively benign. As I said before, economics is a consideration.

I suppose we have to ask, if we are political creatures, as the governors are, what's in it for New England? Are the oil companies willing to make any sort of long-term commitment to the economic health of the region that will balance the risks, whether it's perceived or actual, to allowing offshore drilling, especially in as controversial an area as the submarine canyons.

I don't think such a long-term commitment on the part of the oil industry is possible, simply because of the nature of offshore exploration. The odds are against finding anything, and I understand even in a proven area like the central Gulf of Mexico, the odds are still against finding a commercial discovery.

Am I right? So, in an area where you have only eight wells drilled, all of which have been dry holes, there has to be some sort of a suspicion on the part of the general public and the governors as to what exactly New England is going to get from allowing future drilling in the submarine canyons.

There are no guarantees and, unfortunately, I think some of the political folks are looking for some sort of guarantee in order to be able to take the heat. You see, the voters know the fisherman and they know the environmental groups, but they don't really know the oil and gas explorationists.

The oil and gas explorationist has only been around for a few years, in fact, really for a few months when they were out drilling on Georges Bank. There hasn't been enough of a presence by the oil industry in this region to form a counterpoint to that emotional attachment I mentioned earlier to the fisherman and to the environmental groups.

So, if somebody comes along and says that the oil companies are actually a threat to the fisherman, there's no one there to balance that with some sort of a more reasonable argument.

These are political realities, whether we like it or not. Our government is not ruled by a series of high priests who can make pronouncements for the good of everybody, based on the best information that's available. Those decisions are made by elected officials who are very sensitive to public opinion.

I personally am here to learn more about the canyons and to keep as open a mind as possible. As long as I am working on OCS activities for the State of Rhode Island, I'll see that there is an element of science considered. Science, I hope will play a very important role in the decision-making process.

Not to belabor the point, but we have to keep in mind that science is not the only thing that we have to look at and we have to consider.

Thank you. That's the end of the humanities lecture for the day.

(Laughter)

DR. TEAL: A slightly facetious point, if I may. The price of oil and gas is low at the moment and there is not a whole lot of interest in New England. I think it's inevitable that there's going to be some period of time before the companies want to spend very much time drilling on Georges Bank, whether they're close to the canyons or not.

I was sitting here thinking as you were talking about the fisherman, that an environmental group, of which I'm a member, is fighting to try and retain some docking space in Glouscester for the fisherman.

The Portland Fish Auction in serious financial trouble right now and is in danger of closing down. Boston has only a shadow of its former strength as a fishing port.

Newport dockage has been mostly taken over by condos already, and there's only local lobster fishing left, and even in New Bedford, which is New England's premier fishing port, there are proposals--I know of a proposal for condo development on Homer's Wharf, one of the principal docking and fish processing wharfs.

All the oil companies have to do is wait a while and there won't be any fisherman left in New England, and that one source of trouble will be gone. Then from your point of view or your points of view, we have two disasters.

DR. AURAND: I don't go far enough back with this program to know the answer to this, and maybe either one of you or both of you could speculate on this.

I would have thought, if I had not had anything to do with the program, that most of the controversy would have occurred, and in the very beginning there was drilling on Georges Bank, there were no catastrophes that I know of associated with it, and I would have thought that that would have reduced the tension, but it obviously has not.

I was just wondering if either one of you would like to speculate on why--the fact that there was exploratory drilling that didn't cause any environmental damage, has been so unsuccessful in changing anything or, if you look at it from the outside, it looks like it's made it worse.

The fact we were up here at all, historically, has not done any good. I was just curious if you have any feel for why that happened that way or an opinion even.

MR. VILD: I don't know if I agree 100 percent that opposition is any stronger now than it was before or whether it's--it hasn't lessened.

Maybe I should say that I'm not convinced that opposition hasn't lessened.

I think I'm just speaking for myself, I'm politically more mature in the whole process than I was 10 years ago when I started. I think what we know now--with what we know now, governors are willing to accept, my governor is anyway, he's willing to accept a certain degree of offshore drilling.

If this must come to pass, then he will support it, with certain stipulations, one of them being a no drilling in the canyons stipulation or just an outright deferral from lease sale to lease sale.

We're still concerned about the cod and haddock spawning grounds and different distinct areas. I think in general we in Rhode Island still support the notion of offshore drilling, our governor does.

There's a certain degree of political heat that's generated in the governor's way as a result of that. We've had public hearings in Providence where I was the only one timidly approaching the microphone and saying, "Well, the governor does support the thrust of the offshore drilling program, but--"

Then, taking great pains to say exactly where we were concerned, the sorts of areas we wanted to see out of the particular lease sale under question or the areas that we wanted to have covered under that special biological stipulation.

Maybe it's a more accurate assessment to say there's been some consolidation of protest around things like the submarine canyons, because as the years have gone by, and Pat, I think, has pointed this out in her particular talk, there really hasn't been enough evidence generated that would have us urge our governors to reverse their particular position as far as opposition to the canyons go.

We've heard about different current regimes, we've heard about variations even within the same canyon, and observations like that make prediction very, very difficult. Also, getting into the economics thing, there's an energy glut now, there is no real pressure by the oil companies to do anything on the North Atlantic.

Again, they're not providing the sort of counterpoint to the public opinion that would be against not only drilling in the canyons, but maybe drilling in a lot of other places as well.

Those are just my observations. I know, Pat, you have a lot to add to that.

MS. HUGHES: I think partly through the Environmental Studies Program I think generally people have--obviously the people who work on OCS, but I think also generally the public have a better understanding, have more information about what makes Georges Bank work or at least how it functions.

It is in their heads that it is an unusual area of the ocean, very productive, that it has submarine canyons, that there are species in the canyons that are not found anyplace else, that there's high biological productivity, that there are a lot of birds, et cetera. We should have a better understanding of the system, number one.

Number two, that's come parallel with all of the struggles surrounding fisheries management which are very complicated, which has it's own share of resource allocation problems, and should you allow this, and should you allow that, that offshore oil has been faced [with similar struggles] in the North Atlantic as well as in other areas.

[The fishing questions experience] similar kinds of political tensions between the federal governments and the States as offshore oil, but Georges Bank and the Gulf of Maine area have been identified, at least in this region, as the most important fishery area for more than 150 years.

So, there's an improved understanding of how sort of special, if I may call it that, the Georges Bank system is. How important the fishery is and the challenges surrounding conservation and management of the resource that we now share jurisdiction over Georges Bank with Canada, that we have very different attitudes at the federal level regarding resource exploitation, both mineral and fisheries.

So, I think that has all combined to heighten the importance of what happens out there in people's minds. I think as Bruce indicated, if you were to ask the average person in Massachusetts or Rhode Island, "Georges Bank, what does that mean to you," they're going to say to you, "It means fish."

I think the other part of the equation, at least from Massachusetts' perspective, I think it's always been possible to work out a leasing program in the Georges Bank region. I think the difficulty has always been the amount of area that the federal government has wanted to lease.

I think the other aspect of it all is even with the uncertainties around trying to predict what the quantity and quality of the petroleum resource may be underlying the area, that it's lost on most people that we the public should just buy into the, "Well, we never really will know, and the oil companies know better. So, where they tell us they want to go is where we, the federal government, should lease this public resource."

So, I think it's the combination of all those things that makes the question of oil and gas drilling in the North Atlantic controversial and will keep it controversial.

DR. RAY: I haven't said anything controversial yet today, so I think it's time to stir the pot a little bit.

First of all, let me say that I think your comments are right on and that your "Civics 101" lesson was very good. Those are realities, and I think anybody that doesn't understand them is kidding themselves.

Let me come back and make a few comments about reality. One of the things that I have an awful lot of problem with is the hypocrisy I see sometimes from the States because of political reasons, and from the environmental groups that are trying to save the environment.

I was very pleased to see Dick make a comment this morning about commercial fishing. If you went into the commercial fishing--

DR. COOPER: Be very careful of what you say.

DR. RAY: --and had to do a nitpicking detailed environmental impact statement for each of those different fisheries and really evaluated what's going on, and I'm talking about now the public's resources that we were just talking about that we're protecting, the tradeoffs that you're making for the fish that go on your table versus the damage that's going on in that environment, the benthic environment that's being torn up, the resuspension of tons and tons of sediment, it goes on and on, the bycatch, the tens of thousands of pounds of fish that go back over the side because they've hit quota and they don't dare come to port with it because they'll be fined, it goes on and on and on.

The people, either they don't realize or they don't want to know in that industry, which is an important industry, hey, I like my seafood like everybody else, but people aren't being honest with themselves.

Here we are here today dealing with hypothetical impacts, things we may or may not be able to measure, dealing with extreme subtleties, and yet we're talking about a particular activity and industry that we're talking about millions of pounds of biomass, of other species killed routinely, everyday in the operations of different fisheries, and it's an acceptable thing.

So, I guess as part of the public and looking at it from a scientific standpoint, I find kind of objectionable some of the fishing practices we do and the damage that it causes to the environment.

It's interesting that we can talk and we can separate these different....I'm doing it from a pragmatic scientific standpoint, not the public perception, the political standpoint.

I understand the point you're making, but I'm just saying that it's a real interesting hypocritical situation that we can accept and overlook those damages as routine, but yet we're over here dealing with some of the oil and gas issues which we're regulating on.

Anyway, that's my kicker to stir the pot. I know Pat's ready to jump up here now.

MS. HUGHES: No, I'm not going to jump on you. I think that a point to be made is that a lot of the things that you raised, things like bycatch and what some people may find as over exploitation of the stock, et cetera, Dick can talk about it and Bruce Higgins can, the science of resource assessment, and predicting the strength and weakness of a stock size or of a year class, or what are the actual impacts on recruitment and on spawning, et cetera, are as inexact as the science of predicting the impacts of oil and gas activities on resources.

I think the public, informed public, the managers and the people who have to make decisions, and the general public are now beginning to come to the realization that there may be some very significant questions that you raised regarding the conduct of the commercial and recreational fishery that have not been paid that much attention to.

I think it's more--the timing is different. People have been on the petroleum industry for perhaps a lot longer than they've ben on the fishing industry. I think there are some similar questions that are now being asked regarding the conduct of the fishery that are similar to what people have been asking about the oil and gas industry.

DR. RAY: I think it would be great to take some of the money that we've spent in the last 10 years trying to find impacts from oil and gas operations and spend some of that money in trying to better understand the impact on our fisheries, and more importantly, to use some of that money to improve our fishing techniques and to improve the catch and minimize the damage.

MR. VILD: Jim, may I ask you a question?

DR. RAY: Sure.

MR. VILD: Why are the oil companies interested in the submarine canyons? Why won't you just leave well enough alone?

DR. RAY: Neither way--I'm not here [sentence not clear].

MR. VILD: I'm stirring the pot in the other direction.

DR. RAY: No problem. First of all, I don't even know what the level of interest for the submarine canyon area is anyway by the companies, more or less on the East coast.

As you are aware, the general level of interest for the East coast is fairly low as far as the industry in general is concerned. I can't speak for all companies, but generally the impression I get from everybody is very low.

I have no idea as to what interest people may have near the canyons. I think as you get close to sensitive biological habitats, especially where you may have physical impacts, whether it be a canyon head, a coral reef or otherwise, I think there's very logical places where restrictions can be justified.

In answering your question, I don't know of any specific interest in trying to drill in a canyon.

MR. VILD: Would the oil companies entertain a proposal to do directional drilling outside of the canyons and go underneath the canyons for the oil?

DR. RAY: That question usually depends on a couple things. One, in exploratory drilling, generally because of the nature of the drilling and the interpretation they're trying to do, they try to drill straight holes.

In the development phases, that is usually not too big of a problem. They can kick out anywhere from a mile to 2 or 3 miles laterally as long as the well depth is deep enough, because they build up to an 80 or 90 degree angle going out fairly quickly nowadays with the technology we've got.

They could be several miles away and develop from beneath an area like that. It's just in the exploratory phase where they run into the problem with drilling high deviation holes.

DR. COOPER: I hate to see one guy take on an entire audience. Some of you may be surprised at my stance here because I've spent some very enjoyable years of my professional life in these canyon environments and other parts of the shelf, but I am quite convinced that the negative impacts of any kind of production drilling, given an occasional massive spill out there in the canyons, is greatly overshadowed by man's total ineptness at managing it's own living resources.

It's fisherman and the habitats that protect these resources. We're really not talking about issues of cold, hard logic, and balancing the ledger of one versus the other, we're talking about emotional perceptions here.

That's really what--I'm tempted to ask and I will ask the question, 10 years and 3 1/2 months from now when OPEC finally gets its act together and the price of oil and gas skyrockets and our commercial fishery, at the very least, if not a recreational, is 1/3 of the level that it is now, what's the political stance on the part of our various New England governors going to be then?

MR. VILD: That's a good question. Right now I know Rhode Island is looking into a lot of power-plant proposals that have really nothing to do with being powered by oil or gas. Canadian hydropower, for example, coal-fired things, which of course, have their own environmental problems.

You ask a very interesting question. I think I covered that by saying that with the particular energy glut we have now, there doesn't seem to be any great desire to go out and even ask the oil companies to come and take another look at our region.

DR. COOPER: The issue now is an easy one.

MR. VILD: The issue now, yes, you're right, it is a very easy one. I would just like to throw out one suggestion. This is a very nice kind of give and take, but it looks like it's the same people who have been talking for a number of years on the same issue.

I don't know, Jim, what do you think of this. What do you think the prospects are of direct industry-to-industry talks on such things, not only drilling in the canyons, but drilling other places where there may be spatial exclusion, for example, of the fisherman or maybe gear conflicts.

I know Governor Garrahy about 8 years ago, tried to get an industry-to-industry task force going. It went for a couple of years and ultimately it broke down and each side blamed the other.

It had to do with compensating for fishing-gear loss, because one of the big concerns that the North Atlantic fisherman had was, okay, there is a fisherman's compensation program, but the red tape involved in putting in a claim and everything else, just led to incredible delays and, of course, that cost them money, that cost them their livelihood.

Do you think the aspects of industry-to-industry cooperation, directly talking with the people who really are on two ends of the political spectrum here as far as being pro- or anti-drilling, getting the two industries together and maybe come to the consensus that we're trying to seek in this particular workshop?

DR. RAY: I think it's crucial. It's a hard thing to accomplish other activities like that back when Georges Bank was getting really cranked up.

The industry, I think, is learning a lesson and taking their lumps. They have been now trying to work cooperative programs in Alaska with the fishing industry. An interesting example is California.

By comparison to the Georges Bank area and to Alaska and to the gulf, California is a rather small fishery, but politically you would think they were the biggest fishery in the world.

I mean to tell you--talk about combat warfare--they've had a committee between the industry and the fishing industry out there for about 3 years, and it's been a learning process for both sides. It's making progress and it's accomplishing some of the things that have to be done if the industries are going to co-exist and tolerate each other.

It's the space conflict, it's the gear damage, it's all of the above things. The key thing is the communications. I've seen some real positive signs in California with the arrangements between the industry and the fishing industry out there.

Once you get the hardliners on both sides out of the way, and get the more reasonable people talking, you can make progress. Some of the people are never going to change their mind, on both sides.

I think it's imperative if things ever really crank up back here, boy if they don't sit down and start communicating, it's not going to work.

DR. AURAND: In response to your answer, Jim, when asked about why are the oil companies into it, as you didn't want to speak for your companies on deferrals, I don't want to speak for the Department of Interior on whether or not we would have deferrals for canyon heads or not.

As a matter of fact, that never even came up when we were proposing this study. This whole effort was generated internal to the studies program, partly in response to the agency's efforts at outreach, but also partly in response to our internal frustration with being able to plan and execute studies which seemed to resolve issues, which is what we're supposed to be doing.

So, I have no idea whether the results of this particular workshop would be used by the Department of Interior or the State of Rhode Island or anybody else to change any of the decisions that they make on a political basis.

What we're trying to do is get a consensus on those things which we can, where the facts are available and fairly pervasive [persuasive?]...that we can agree on, determine the areas where we can't, and where we might go from there to seek agreement on facts, not necessarily agreement on value judgments.

There's always going to be an element of that, and I don't think we have any business, really, trying to work that out in the Environmental Studies Program, if you wish to be opposed to leasing or if we wish to support it. That's a different kind of problem.

The two get mixed together so often in the debates about the studies program, that for us, at least, ...represents an attempt to try to deal with risk perception and come to some resolutions of at least some of the issues, and that's all it represents.

As a matter of fact, I'm not 100 percent sure that Mr. Coleman [phonetic], who is the new director of MMS, even knows we're doing this, I didn't tell him.

Nevertheless, just the fact that we can put down areas of agreement or disagreement, moves the program in the direction that we in the studies program want to move, which is trying to spend our money on things where we can have some influence to answer questions which concern people.

I wouldn't read any more into than that. There's no subtle part here on the part of the Department of Interior to do something. We're not clever enough to be that subtle, I don't think.

It's really self-generated from within the studies program.

DR. KRAEUTER: Given what you were talking about of trying to get the fishing industry to talk to the oil industry and vice versa, I'm just trying to think through how you would do that.

Unless there's a controversy, particularly for the fisherman, I don't see how you're going to get them,--unless they can see some threat--I don't see how you're going to get them to come to a table and talk.

It's just the nature of the industry, it's small individuals out on boats. They don't have the time to come to a meeting like this and spend 3 days sitting around talking about a problem, they want to go out and fish and earn a living, whereas the oil industry has got a fairly substantial structure so we can get those like Jim here to come and present whatever their side of the argument is.

So, we're almost in a bind in that the people you need to have at the meetings you can't get there unless you've already got the controversy. Of course we don't want the controversy and that's why we're having this meeting.

I just don't know how to resolve it. It's basically the same problem all over.

MR. VILD: Let me tell you what Governor Garrahy did, the governor of Rhode Island back in the early '80s, 1980 I think it was. Through his energy office, for whom I worked at the time, letters were sent out to the chief executive officers of the oil companies who were the successful bidders on lease sale 42.

They also went out to the skippers of as many fishing vessels as we could obtain, however long a list that we could obtain. The idea was to have the chief executive officer from one industry talking to the chief executive officer from another industry, and treating each one of those fishing vessels as it's own separate company.

It was nice. We were down in Newport and we had dinner and the governor made a presentation and people were arranged at different tables and there was a nice mix of the oil company people and the fisherman, and we got a pretty decent turnout.

It took a couple of follow-up calls, certainly. My boss was saying, I could hear him in the next room, and he was saying to some of the people--not only the fisherman--"You said you were coming and I'm really counting on you to come, so please do show up." We did get a very good response.

From that spun off that industry-to-industry thing about gear loss and trying to come up with some sort of alternative mechanism for compensating fisherman who lost their gear, who snagged their gear on OCS equipment, or presumed OCS equipment where there was OCS activity.

So, maybe that's what you have to do, you have to try, bend over backwards a little bit, but you can bring the people to the table. That's been our experience. I don't know if 8 years has changed any of that.

You're right, there was the whole looming controversy of drilling to begin with.

DR. RAY: In California not only did the fisherman have their own organization, they had a representative that goes to the meetings because the rest of them are out busy fishing, but they also have a separate office set up out there that has a full-time staff, as a matter of fact they have one fellow that's full-time staff that's a Ph.D. marine-biologist type.

He's kind of a liaison between the two groups. Between he and the fishermen's chosen representative that goes to the meetings and stuff, that's how the representation of the fishing community comes to that meeting and there's a few designated representatives from the oil industry.

They have a regular newsletter which has updated information on what geophysical operations are going to be in what areas during what months and then the fisherman will come back and say, "Well, you can't do that because we've got a drift-net fishery at this time of the year."

Then they work out all those space conflicts and things of this sort, trying to minimize the problems with gear and what not. It keeps the number of people that have to give time down to a minimum, but there's a good flow of information and you're getting it from both sides.

Aside from the usual personality conflicts and disagreements from time to time, it really, so far, seems to have worked pretty well over the last couple of years. I think it's a good model to build on for some of the other areas where, in fact, some of these problems may crop up in the future.

DR. BUTMAN: I second that. We were doing some work on the California Area Monitoring Program, which is sponsored by MMS, and we've had occasion to use that liaison several times, and he's been very helpful both between the oil and fish people.

MR. LANE: I have one general question, if there aren't any others, for both Pat and Bruce. As a way of trying to measure what progress there's been over the last 10 years, do you think there's been a change in public or political perception of exploratory drilling versus development and production drilling, or is that still to fine a distinction to make for the public?

MR. VILD: Do you want to go first?

MS. HUGHES: I don't know that the general public separates exploration and development and production. I think that certainly within the agencies that I work in within my State, Massachusetts, that there's a better distinction.

However, given the way the leasing program is, one has to assume, I think, for the purposes of policy development and planning that any place where an exploratory well would be drilled, could be a development and production location.

I'll try and infer from your question that if you're looking for is, do people have a better understanding of what are the environmental effects from exploratory drilling versus development and production, I think it's probably fair to say again, within my State and the people that I work with, that 10 years ago perhaps there was more of a naivete about what is it all about and what happens.

There's a better understanding of exploration and development and production and more of a concern regarding the long-term impacts of development and production versus what the environmental impacts might be from exploratory drilling discharges.

MR. VILD: I would respond to that it depends on the people. If you go to the public hearings and you hear Greenpeace, you have to say no, there is no better public understanding it seems of the differences between exploration and development and production.

But, if you talk to the fisheries representatives or you hear what they have to say at the public hearings, yes, there is, there is. They know there is a distinction between the two and I think they know the parameters in which they can work with the federal government or whatever, at both stages.

They know when the comments are due, for example, on whatever lease sale is being proposed or the 5-year program or anything like that.

They are perhaps better organized, even if it's organized in opposition. Maybe it's not even opposition, maybe it's just very, very critical support. We may be moving in that sort of direction with some of the fisherman, anyway.

I think sophistication actually is a good word, because the people who have taken the time to study the program and know what the differences between exploration and development, are able to generate the sorts of comments that the governor can use and certainly the sorts of comments that you can use.

As far as Greenpeace goes, I don't know if they just have chosen to gloss over what the differences are for their own political reasons or whether they just don't understand, whether they are just so suspicious of the program, that they really don't see any difference between the exploration phase and the development phase, because it's all going to lead to the same thing, environmental degradation, at least in their particular perception.

DR. TEAL: Speaking for a conservation organization that isn't quite as far out as Greenpeace, there's a very real recognition of the difference between production and exploration phases.

But, as Pat just said, the fact that under the present leasing program there is no break between the two, if you lease an area and you do exploration and you find something, then production and development will follow.

For opposition to exploration to disappear just like that, then the two processes, the two phases, have to be decoupled.

MR. VILD: What exactly do you mean by that? Do you mean having a separate exploration and lease sale?

DR. TEAL: I don't know exactly what I mean, I mean they have to be decoupled in such a way that one doesn't automatically follow the other.

The ideal situation would be an amendment to the Lands Act so that exploration could be done separately, entirely separately. The government could go out using the oil companies or using geological survey or something and find out and do real exploratory drilling and find out what was there.

Then, the whole question would arise again, but there wouldn't automatically be, without control and without further consideration, development on an area which there was promise of something, and the people who were considering whether to support or not support production drilling, would also know what it was that was down there and was worthy of production.

That, for environmental considerations, can make a very big difference.

DR. AURAND: Actually, the connection is not automatic, even in the existing situation. However, since we've never done anything else, we are hard pressed to prove to anyone that it could, in fact, be decoupled if the resource was sufficient to conduct the lease sale.

So, I can see the perception and I don't know exactly how the government gets out of that bind. Since we have never told someone they couldn't develop something, at least not to my knowledge, I don't think we ever have, even though the law would permit it, I don't think anybody believes that we ever would.

I would be interested in both of your reactions to John's suggestion. Do you think that that would change--in light of the conclusions that we presented, if those conclusions were stuffed in separate little boxes for exploration, and production and development, do you think you would have different responses?

MS. HUGHES: I'll respond first, the debate over separating exploration activities from development and production occurred before the Lands Act amendments in 1978. There are a lot of reasons, given our economic and political system that we don't separate exploration from development and production.

While there may be lots of people who wish that we could do it that way, I think it's very unlikely, number one. I think second you're right, the Secretary does have the authority to say to a company, "You cannot go ahead in development and produce this find that you've delineated for environmental reasons."

It clearly states that in the act, and yes, they have to go through an EIS and through all the development and production permitting, et cetera.

I think you're right, Don, in saying that--I mean I think it's very unlikely for any administration to say that to a company.

"We've gone ahead, we've encouraged you to invest millions of dollars, we've placed all sorts of restrictions on what you could do in the exploratory phase. You've now found a large field that's commercial, well, we're awfully sorry but you can't do anything with it now."

I think that's very unlikely, no matter who it is that's making the decision. I think there are ways, and I'm not an expert at this, but I think there are ways to do something more like what John Teal was describing, which is to provide the opportunity for there to be some delineation of a hydrocarbon resource without automatically there being development and production.

This is not the forum to explore those, although you can't separate them. Part of the overall problem with a discussion like this, Dick Cooper said, "What, 10 years from now, will happen when OPEC has got the price of oil way high?"

We're not going to solve the energy crisis by drilling in the submarine canyons, by drilling in the North Atlantic, by drilling in the straits of Florida. We solve it by the development of a sensible and long-term energy policy and it includes a lot of things that are beyond our discussion, energy-efficient cars, et cetera.

All these things are tied together and we're trying to separate them all to go back to separating them for this discussion. I think the reason that States like Massachusetts say the things that they say is because we accept some of what appear to be the present realities.

We see that there are some creative ways to get around some of them, we don't see that mindset or the willingness to do some of the creative things that could be done among the people that we deal with within the federal government, and therefore, we say, "Look, if we're really going to work hard on wise resource management, just get them the hell out of those places, and then all those other problems don't exist."

It sounds simplistic and some parties will take that position, and just take it and not be responsible about how they came to it, or why they're recommending that particular position.

I feel that as far as the submarine canyons are concerned, I think that Dick Cooper's work argues, Barbara Hecker's work argues, lots of other people here in this room, just the information that's been presented today says, these are unique areas.

Maybe on just that alone, from a resource management point of view, that argues to stay out of them.

MR. VILD: I don't really see a proposal like John's working unless the government was willing to subsidize exploration totally, 100 percent. The carrot of being able to develop something that you find, just as Pat says, is really the only thing that drives the exploratory program now.

Right, Jim?

DR. RAY: I'll make a generalized comment. I'll speak in general terms, obviously I can't speak--especially for my own company, more or less the industry.

Two comments, one is that a two-phased approval process, which you partially have now with plans of exploration versus plans of development, you can maybe have a more formalized way to do it, to make sure that you really do a complete assessment based on the size of an exploratory find in order to define whether or not you have production and how you do it, is probably doable and acceptable.

I can you tell you right now you'd have a hue and cry from the industry like you wouldn't believe at the suggestion that the government get into the oil business and start doing the exploration.

You've probably all heard it before, but the competitive nature between the companies is very intense. Probably one of the more expensive parts of our business goes into a lot of the exploration part of the work and the geophysical work and the computer stuff.

There's a difference between the companies, and how the government would handle getting into the exploration business would be very difficult. The industries would not go for that at all, and they also aren't very interested in trying to share information with each other, either.

Companies can go into an area and drill 50 wells and won't find anything, they can go in and do a geophysical and won't see anything.

Another company can come in and take the same geophysical information, have different ways to analyze it with a computer and see a whole different story, and in fact, there may be oil there that the other companies aren't seeing. That's why you have such weird bidding in some of these lease areas.

Half the companies will say there's nothing there, the other guys will go in there and pick up all the leases, in fact, they see something different. Sometimes they're right, sometimes they're wrong, it's a hit or miss anyway.

Anyway, in general you'd really hear an uproar if you suggested that the government get into the oil business.

MR. VILD: John, did I misrepresent what you said? Maybe I just didn't understand when you were talking about having a sort of bifurcated system there separating exploration from development.

DR. TEAL: What did you say that you were worried about misrepresenting?

MR. VILD: The business of having totally separate exploratory phases and no direct link with the development and production phase.

DR. TEAL: What I said was I have no idea about how it might come about, but the notion in New England, and it was discussed very extensively at the time of lease sale 42 and the court challenges that went on at the time, they talked about it as selling a pig in a poke.

A lot of people object to that idea. I accept what you say about the industry, obviously I have to accept it. We all recognize that as being true.

I'm not saying that it's possible to do, but I'm saying that being able to decouple in some way so that the public knew that they were selling gas leases with--[word unclear] gas on Georges Bank, suppose that is what is there, would be a very different thing than selling leases without any knowledge of what is there.

That's all I'm saying.

MR. LANE: Any last questions?

MS. HUGHES: Jim, I just have a proposal. Some of us were talking earlier in the day and I wondered if we might, as a group, talk about compressing tomorrow and Thursday's discussion into tomorrow?

I wonder if we're not far enough along today to take the afternoon discussions of tomorrow and tie them to the discussion of the individual hypotheses, and then tomorrow afternoon do our summary and recommendations so that we might all be here one less day, basically. Perhaps save some money and make it a little more efficient, I don't know.

I throw it out for people to talk about. I wonder if we can, as participants compress two days into a day.

MR. LANE: There are a couple of ways to approach it. I guess we were thinking that there was going to be a good deal more discussion and dialogue in the sessions tomorrow and I suppose we can just play it by ear and see if that works out.

Otherwise we could take a vote of some sort and determine whether that's the preference of the group and go towards that objective.

I'd like to give Bob Miller, who is the COTR on this project, an opportunity to comment on the feasibility of that as well.

DR. MILLER: We'll have to coordinate that with Walcoff and Associates to see how it's going to work.

DR. AURAND: I would think that you'd be able to tell more depending on what happens tomorrow.

MS. HUGHES: What time do we have to check out of here by?

DR. AURAND: Eleven o'clock.

DR. KRAEUTER: It also makes a difference for those of us who have airline flights and might have to reschedule them, and later in the day it gets to be a little more difficult.

MR. LANE: Do I sense consensus on the idea that we reevaluate this at perhaps morning coffee break tomorrow at 10:00 and see how far we've progressed and whether it's feasible, or would the group rather just accept right now that we're going to work towards recommendation by tomorrow afternoon?

There is one other advantage to that approach, besides saving a day, and that's several people who won't be able to stay on Thursday. They would be able to have their input, listen to the recommendations, and say whatever they wanted to say about them.

So, it's attractive from that perspective, I just don't know whether the discussion can move that quickly tomorrow.

Tomorrow at coffee break, reevaluate? Does that sound acceptable to everyone? Thank you very much for your attendance this afternoon and we'll start again tomorrow at 8:00 o'clock.

(Whereupon, at 5:14 p.m., the Plenary Session of the Submarine Canyons Workshop was concluded.)

DAY 2--WEDNESDAY, FEBRUARY 8, 1989
MORNING SESSION

DR. AURAND: We will pick up the one paper that we left from yesterday before we start this morning's discussion, so we will do that without further delay, I guess. Then, afterwards, we will talk a little bit about what we intend to do for the rest of the meeting and what we hope to get out of it.

For now, we will just go back and let Bob give his presentation on the Toms Canyon. Jim Ray indicated that you have been dodging giving him this information for years and that he was sure that was why you missed the plane. So, Jim, this is your chance.

DR. RAY: Thanks, Don.

PRESENTATION OF DR. ROBERT C. AYERS

DR. AYERS: I am going to take you back about ten years in time. In some cases, my memory is a little rusty, but this was back when we were projecting a $100 a barrel crude by 1990, and everybody was very excited about drilling on the East Coast.

There wasn't any MMS then. We had BLM and we had USGS only. We at Exxon USA had a lease at Block 816 that happened to be near a relatively small submarine canyon.

(Slide)

It is not very clear. Is it out of focus or is it just sorry graphics?

DR. AYERS: This is Block 816. It is 93 miles east of the New Jersey coast, Atlantic City, and near Block 816, actually at Block 815, is the canyon.

Toms Canyon or the rim of the canyon down current was about 3.7 kilometers from the well site and the axis of the canyon about 7 kilometers from the well site.

We did the study because we were required to. Initially, the Mid-Atlantic Biological Task Force had decided we shouldn't drill a well at all, but through a series of negotiations, we finally compromised on drilling a well and doing a study.

They advised USGS that we really needed the study, so we did the study. It was sort of a negotiated thing. We were sort of interested in doing the minimum and they were going to do the maximum and this is what we came out with.

Partly because we had just finished doing the Mid-Atlantic Study, which was the million dollar study that I'm sure most of you know about, and we knew a lot more about the impacts of the discharges probably than some of the people who we were dealing with and were a lot more comfortable with them.

Anyway, this is what we did. We had a predrilling survey. We directed the project at Exxon Production Research Company. It was a bathymetric survey. We took metals and sediment, drain size analysis, clay fraction analysis.

On the predrilling survey, we took biological samples and these were washed, preserved, and stored, so they were to be analyzed in the event that we drilled another well in the vicinity. We didn't, so they were never analyzed.

As a matter of fact, I think we gave them to Stony Brook or something. I can't remember; we did give those samples away, though.

The predrilling survey took place September 1980, for 3 days. The rest of the program consisted of the drilling phase surveys and a monitoring of the discharges themselves. We monitored the quantity and composition of the mud discharges, currents. We used sediment traps as well as sediment samples. We analyzed the top 3 centimeters of sediment.

We did not analyze the fine fraction, as we did not think about it, as you did in Georges Bank, which, of course, is a much more sensitive indicator of sediment samples. We took the top 3 centimeters.

There were four cruises. The current meters and sediment traps were installed here in November right before the--excuse me, right after the rig was on location. This is how long the *Alaskan Star* was on Block

816. Then two turn-around cruises and a final cruise was made on April 28 to 30th.

(Slide)

This gives you a little bit of an idea of--that's a little better than that other one, isn't it? It's not too good, but it is still better than the other one. This is the well site. We had one transect that parallelled the current, the prevailing current to the southwest, that went through a depth of 550 meters.

We were interested in the top part of the canyon, 550 meters up. We had another transect that went along the axis of the canyon and we took samples here. Then we had another transect, Transect 3, where we took some sediment samples down here. This would be south.

This is the way the sediment traps and current meters were located. We had sediment traps, two sediment traps, acoustic release type devices, one at about 20 meters below the surface here. This is about 1500 meters down current, 20 meters below the surface, and 140 meters below the surface which is about 10 meters off the bottom.

Then at the canyon rim, which is right here, that was about 3.7 kilometers away, we had one--we had a sediment trap and a current meter at 150 meters or about 140 meters deep, right about where the canyon starts to dip.

Then, in the canyon itself, we had two current meters and sediment traps, one 10 meters off the bottom at 540 meters and then another one again 150 meters from the surface. So, those were the ways we had our traps and current meters arranged.

(Slide)

This is in a little bit more detail, the same thing, but you can see the contour lines a little better.

(Slide)

The well was a fairly high mud weight and discharged a lot of barite, moreso than we did in the Mid-Atlantic Study. We discharged about 1,000 metric tons here. The mud weight went up to I think around 16 or 17 pounds per gallon. This is the cumulative discharge volume.

This goes from 0 to 25,000 barrels over here. This is both mud and cuttings volumes. Just to maybe give you an indication of what these--these are the monitoring periods during the first period, the second period and the third period, where we collecting material in the traps, so you can see what we were doing there.

(Slide)

This is similar information, except this is mud only. You can see we had about 19,000 barrels of mud total. I am not going to say much about the currents except this. On the shelf, they were generally toward the southwest with a mean speed of between 10 and 21 centimeters per second.

They were pretty constant. There was occasional, occasional reverses to the northeast, but not very many. Generally, it was pretty steady towards the southwest. The surface currents and bottom currents were pretty similar.

In the canyon, we had some down canyon currents to the southeast, up to the north, the northeast, with a slight net flow up the canyon. The larger or higher magnitude current surges were down current, but there was a general slight trend upflow in the canyon, and they were completely decoupled from the shelf currents. There was no relationship we could see.

Sediment analysis. It was mostly sand on the shelf. This is very similar, of course, to what we saw in the Mid-Atlantic Study. As you started going down in the canyon, the sand dropped and the silt and clay went up to the most, the deepest point in the canyon was only 3 percent sand, as opposed to about 90 percent sand up on the shelf.

Silt/clay ratio, this is rough, but we had usually about twice as much silt as clay in most of the samples. It wasn't always the case, certainly, but roughly that.

On the shelf, about 95 percent of the clay was equally divided between montmorillonite, alite, chloride, and kaolinite. In the canyon, there was less montmorillonite with a little bit more chloride and kaolinite. I don't know whether we took enough samples to generalize about that, so I don't know whether that is real or not, but that's what we found. DR. BOTHNER: Are those samples predrilling samples?

DR. AYERS: All these are predrilling samples, yes. These are predrilling samples, also. We did our metals analysis in the sediment by neutron activation so we could be sure we got all the barium, which was going to be our principal tracer.

Barium levels ranged from background levels of 156 to 303, chromium 8 to 45, and 16 to 49 of vanadium, which is the other metal we looked at. These are some outlyers. This is one that was actually at the station at the head of the canyon, the top of the canyon. It sort of illustrates that we saw a lot of variability.

Our metals levels and our grain size analysis numbers are an average of two samples. We had one sample at the top of the canyon that was about 95 percent sand and a duplicate sample was about 1 percent sand. This is the one that had 1 percent sand in it. There was a lot of clay and, of course, that accounts for the high barium level.

This is one that Jim insisted I discuss and that's the reason he really wanted me to talk, because he feels like it supports his theory that originally, all the canyon heads were drilled back in during the Stone Age by men from Mars.

(Laughter)

Ray feels like there is no reason to be concerned, because they drilled these much earlier. I think most geologists find his theories interesting, but I don't know anyone that really embraces it. I think the theory goes on that descendants of these Martian oil men went on and founded Texas A&M.

(Laughter)

Again, the concentrations of metals, again, were generally higher in the canyon, of course, reflecting the higher silt and clay contents in the canyon.

(Slide)

Now, I am going to show you some drilling survey results. These are sediment barium concentrations versus distance. This is background in here, this band, and you can see that these were two different cruises. This is the April cruise, the later cruise, and this is the March cruise.

This is at the well site and these levels are comparable. This is around 5,000 parts per million which is comparable to what we see at exploratory drilling well sites; it is what we saw at the Mid-Atlantic and what we have seen elsewhere.

As always, these levels drop rapidly with down current distance, so somewhere, 1 to 1 1/2 kilometers from the rig, these sediment barium concentrations are back to background.

Again, had we analyzed the fine fraction as Mike did in the Georges Bank study, I am sure these levels would have extended further out and we would have been able to detect higher levels further along.

As you move into the canyon, the silt fraction becomes greater and so it is not as sensitive as it was on Georges Bank where you had such a small clay/silt fraction. Anyway, this was certainly in agreement with what we had seen in the past.

(Slide)

Now, I want to talk a little bit about the sediment traps. The sedimentation rates in milligrams per square meter per day were comparable to what we had seen in the literature, ranging from 30 to 350 in the upper water column, a 100, a 1,000, in the lower water column, and in the canyon itself, higher yet. We had one with this real high rate, that outlyer, which I guess part of the canyon collapsed on our sediment trap or something.

Anyway, we had the metals concentration in the traps, of course, much higher than in the sediment. Again, the upper water column, which is the closest to the rig, had the highest barium levels. That is a high of 67,000 parts per million. Then, as you move further away, the concentrations went down until you got into the canyon. As you, of course, increase the mass in the trap, you saw less and less effect of the rig.

Chromium levels were clearly elevated here, probably here, probably not in the--chromium levels were not elevated in those in the traps.

The assumptions that we used to handle the trap data are really as follows. When you make these assumptions, there are two things, I guess, you can do. You can say all the barite came from the rig if you wanted to, or you could say that part of it came from the sediment, a small amount, and that's really what we did.

It doesn't really change the results that much, but we felt this was a little bit more precise. We assumed the trap contained low gravity solids, this clay silt, from the mud, the barium sulfate from the mud and some organic matter from the mud.

It also contained naturally occurring low gravity solids, naturally occurring barium sulfate and naturally occurring organic matter. Then, if you assume that the ratio of these materials--the low gravity solid to barium sulfate and organic matter to barium sulfate--for the naturally occurring materials are constant.

Then, also, if you say that those same things are constant in the mud over a specific sampling period, with three sampling periods--sampling period one, two, and three--you take the mean value of low gravity solids to barium sulfate for those periods, then you can do some pretty simple material balance calculations.

You can determine what is in the trap and what is in the trap from the mud and what is in the trap from natural sedimentation rates. I can go over that in detail with anybody that wants to, later.

(Slide)

This shows the percentage of mud in the trap solids. In other words, what percent of the trap solids came from the drilling operation. That is what this tells us. These are different. This is in the canyon itself, this curve. This one is at the canyon, but only 150 feet from the surface. This one is 3,600 meters from the canyon at 150 feet. This one is 1,500 meters from the rig at 150 feet and then 1,500 meters from the rig at 20 meters deep.

This is percent mud solids in the total solids. You can see less than a tenth of a percent of all the solids in the trap, in the canyon trap, came from the rig. Right around the others, most of the others fall around one percent, a little bit, you know, well, maybe between 0.2

percent up to maybe 1 or 2 percent, except for this one, which is the 20 meters deep, 1,500 meters from the rig. It actually got to around 10 percent at that one location on the last sampling interval when more of the barite was being discharged.

The mud solids make up a very--they are certainly detectible, but they make up a very small fraction of the solids in the trap and the mud solids deposition rate is small compared to the natural sedimentation rate.

(Slide)

This shows it again and it shows that the discharge--this is discharge rate versus mud sedimentation rate. This is in thousands of pounds per day, so it ranges from 0 to 30,000 pounds a day of what is being discharged at the rig, the mud solids.

You can see there is a general increase of the mud solids sedimentation rate with the amount of solids, so it does correlate, but again, I think probably the most important thing is that the natural sedimentation rate is a good deal greater than the sedimentation rate from the mud deposition.

(Slide)

Finally, my last conclusion slide is that mud solids were transported to the canyon, but not in sufficient quantities to affect the natural sedimentation rate or to be detected in canyon sediments. Barium levels were elevated in sediments out to a 1,000 to 1,500 meters; chromium and vanadium were not elevated at all even at the well site.

This is sort of unrelated to the other, but canyon sediments were generally less sand even than shell sediments and they were even more variable in their composition. There are a lot of other conclusions, but on the graphics, they would only let me get three in one slide. I will stop right there.

DR. RAY: Why did you all measure vanadium?

DR. AYERS: We got it for free in neutron activation analysis. Actually, there is, as you know, some interest in vanadium from petroleum silt. We thought we would measure it, but mainly because it is free and is another trace.

DR. BOTHNER: Could you say a word about the size of the sediment traps you used?

DR. AYERS: They were .05 square meters. I will show you a picture of one here. I don't have a--here you go, Mike.

DR. BOTHNER: It has got a height and width ratio of about two and two below [sentence unclear]?

DR. AYERS: Yes.

DR. BOTHNER: I see. That would keep material in the trap once it fell in.

DR. AYERS: That was what we were trying to do. EG&G designed these things. They know more about that than I do.

DR. BOTHNER: I would like to point out that that high flux that you obtained in the canyon of about a kilogram per meter per day is only a factor of five times higher than we found in some of the canyons to the north, so I don't think you have to evoke the canyon falling in on the traps.

DR. AYERS: I was being facetious.

DR. BOTHNER: I know you were, but I was surprised it was the same order of magnitude of what we found.

DR. AYERS: That is kind of interesting, yes. Does anybody else have any questions?

(No response.)

DR. AURAND: If there aren't any more questions for Bob, we will go ahead and continue on with the schedule.

ROUNDTABLE DISCUSSION

There was some discussion yesterday about what we needed or intended to do for the remaining two days. I thought about that a little last night, while I was watching the end of "Heartbreak Ridge." I don't know if there is any connection there.

One of the things that we were trying to do when we planned all this was to minimize MMS' involvement, but I think it is probably a good time for myself and Jim to make a few comments about what we thought we would get out of this and then turn it loose, because I do feel an objection to keep the discussions going, if that is a problem. I don't know whether it is or not, but I'll try this anyway, and that may prevent any future problems.

We don't really think, I guess, that yesterday's discussions were the heart of the matter. In some respects, what Pat said yesterday afternoon is true. It was a restatement of everything we have heard before and that goes for all of the papers, not just the ones which deal with the uniqueness of the biological communities.

But what has always been missing from this discussion, whenever the presentations have been made, is any kind of coherent melding of the data into something that addresses conclusions about impacts.

This is something that MMS has always had trouble with. It deals with the issue of summary documents and the transfer of information. We thought the best way to attempt to summarize this information, rather than trying to do it ourselves and being accused of all kinds of things, was to bring together the people who were concerned about or who did the data and get them to work together to flesh out what kinds of conclusions they would be willing to draw from their own data.

Our presentation of two hypotheses for discussion is just that. It is an attempt to begin the flow of information. We certainly would hope that those would come out to be a little more detailed, a little more involved, and have a little more data behind them when you all got done with it.

As a matter of fact, there is no real requirement that they even be what you come up with; it is just a place to start. We would like to be able to go through all of the things that are on the schedule; whether we get them done in one day or two is kind of irrelevant to us as long as we get through all the steps.

I believe when I get done, Bob and Jim will have some more to say about that, but I think the important part is that our goal is a good summary of what the integrated results of all this research mean, not so much the individual presentations themselves.

One other thing, I think, is that you can tell from our two proposed hypotheses that we are really interested in the mechanisms and the possible reasonable impacts that you might expect in these environments.

Pat's comments concerning the unique status of canyons and unique biological resources is certainly something that we can talk about and that you may, in fact, wish to include into the conclusions, but that, in and of itself, begs the questions of mechanisms.

That is what we hope to reach consensus on: Whether or not there are any reasonable impacting mechanisms that we can define and, if so, what those kinds of impacts would produce in the biological and physical communities that we are dealing with. We would hope to get more detail on those kinds of questions than we have been able to put together in the past.

Finally, I don't think--on the basis of what I heard yesterday--that it would be a reasonable assumption to assume that it wouldn't be a useful document to put together. I suspect that one of the problems that MMS has always had here and in other regions is that no one can read what we write, except someone who has got the time and inclination to be into the technical literature.

As a matter of fact, I have been told that by some of our own staff on occasion, that if we really wanted to be more adept at communicating scientific results, we would tell all of you that you cannot send us 300-page reports; you can only send us series of focused 25-page papers that someone can read and understand and we should refuse to accept anything else.

There is some merit to that argument. We haven't decided exactly what we are going to do with that but, in fact, there is an element of that, even in this, and certainly in the debates that we have had in California.

You cannot digest what we are producing if you are not a technical reader. I think it is a very important contribution, perhaps, to the public discussion and certainly to the governmental agencies who are involved with this if we can take all of this information and distill it down to something that they can understand and, for that matter, something that all of us can understand.

I couldn't read all the literature that is available on canyonheads in what I hope to be my remaining career at MMS which tells you something about either how long or how short I think it is going to be. I don't know.

But we really think that is an important contribution that MMS can make to the open dialogue concerning off-shore drilling and so, we would hope that we can still focus on those issues today.

Bob did give some thought to Pat's suggestion that perhaps we could figure out a way to finish sooner, and I think Jim has a few comments to make and then Bob has a proposal for how, perhaps, we could accelerate the process to some extent.

Before you talk about that, I wanted to make sure we got a few words in about this being--to us, at least, and hopefully, to the public--the most important part of the meeting. Jim?

MR. LANE: I thought I would start by making a couple of comments about what we heard yesterday, describing the abundant and diverse biological communities in the canyons and why they merit special protection and special consideration.

Certainly, it is clear that there are diverse and abundant communities there, more abundant than the adjacent areas between canyons. I stop at the description "unique," just because I am a stickler, and "unique" implies singular to me and obviously, there is more than one canyon which contains these kinds of communities, but certainly they warrant special attention.

The last existing protective stipulation placed on them that I am aware of was one that had a no-drilling zone within 200 meters of the canyon rim. There were more specific boundaries defined by NOAA, but I think in general that was the no drilling limitation.

There were proposals to have more extensive zones and I think the final conclusion was 200 meters and Pat might want to comment on that.

MS. HUGHES: I think I did yesterday, Jim.

MR. LANE: When we describe the potential effects of drilling operations and drilling fluids on these communities, I think we have to put something in perspective.

That is, in order to have an adverse biological effect, we have to postulate a mechanism that allows sufficiently high concentrations of toxic materials to come in contact with these communities for sufficiently long periods of time to produce those effects. That, to us, has always been the most difficult thing to do.

We have had a lot of discussion of the flux of materials into the canyons and how they might get there and what mechanisms might be responsible for putting them in contact with the communities.

What we do not really know is how much material, in terms of its volume and what concentration levels of toxic materials are really likely to be brought in contact with these communities and how long the residence time is for these materials in the canyons.

I think we have to discuss it in that context, if we are going to postulate a significant adverse environmental effect from drilling around canyons.

Finally, as far as the overall objectives of the meeting, I think what we want to accomplish here as a group--and I understand that there are people who won't be here tomorrow--is making some significant conclusions about these adverse environmental effects if they exist; testing these hypotheses; if necessary, restating or replacing the hypotheses with something more acceptable to the panel.

We do have to draw these conclusions and make our recommendations as a panel and I think that as many people as possible from the panel want to participate in that. I think, very clearly, we what to have these recommendations and conclusions in writing before the group leaves, if that is possible.

We want to have those conclusions and recommendations restated for the benefit of the court reporter and the rapporteurs so that everyone has an opportunity not only to participate in formulating the conclusions and recommendations, but agrees that those are the ones that should exist and there is some consensus on that.

If we can accomplish that, whether it is 4:00 p.m. this afternoon, 12:00 midnight or 7:00 a.m. tomorrow morning, I think we have accomplished the objective of the workshop. I will turn it over to Bob to make a few comments.

MR. VILD: Let me just jump in here for a second. In our discussion about whether or not drilling in the submarine canyons is a benign sort of thing or is a detrimental sort of a thing, I don't want to just restrict our discussion to the influx of pollutants into the canyons.

I also want to talk about a very immediate effect that could come from a routine drilling operation, which would be a smothering of the organisms immediately around the drilling rig by drill cuttings.

From what you were saying, Jim, I didn't know whether we were unduly restricting ourselves to just talking about the influx of pollutants into these places. I think when we talk about oil and gas operations, we should assume that they are going to be regulated in the same manner that they have been regulated in non-canyon areas, which means that drilling discharges are, in fact, permitted under certain conditions, certainly, but still permitted, so that you can have the immediate smothering effect.

MR. LANE: Similarly, besides routine discharges, would you want to consider accidental spills, whether they are oil or--

MR. VILD: Oh, absolutely. I want to look at the whole 9 yards, also. I want to talk about spatial exclusion of the fisherman from the area, also. Let's talk about all the impacts, all impacts.

DR. MILLER: In order to address the mechanics of being able to accomplish the goals that are chartered for this workshop and to be able to focus on topics that have been presented in the last few minutes and to also try to accommodate those who perhaps are not going to be able to be here tomorrow, at least a few of you, we have tried to restructure today's session.

First of all, let me mention what has to be done today. We need your review comments on your presentations to be back by 7:00 o'clock tonight on the table outside. Then, this morning, we will go through the hypothesis testing, chaired by first Don and then by Jim, and then break for lunch.

I would encourage you all to try to be as prompt as possible so that we can get on with this. We will probably shorten up the coffee breaks to maybe 10 minutes or so. That sounds like heresy, but it is something that needs to be done, I think.

This afternoon, starting at 1:00 o'clock, we are going to have combined panels, A and B will meet as they are set up in the room today, rather than separately. There will be an exchange in dialogue between the two panel sessions, so that we get an understanding of one another's positions, both geologically and biologically, with regard to the issues that have been discussed with regard to the hypothesis. That will be from 1:00 to 3:00.

Then, from 3:00 to 4:00, we would like for you to actively write your recommendations and conclusions as to what you feel is appropriate with regard to these issues, and then between 4:00 and 5:00, we want to go on record with these recommendations as a presentation for the court reporter. If it takes beyond 5:00 or 6:00, then we will go ahead and do that.

Tomorrow morning, for those of you who will still be here, we will reconvene, and then edit and review the recommendations that have been made so that if there is anything in there that is in error or you feel that needs to be changed, you have the opportunity to do that at that time. We will have this wound up, then, by noon tomorrow. That is the agenda as it is set forth now.

I would, at this point, like to turn this session over to Don and let him begin with the first hypothesis testing process.

DR. AURAND: First of all, I think we need some discussion as to what the group's opinion of the restructuring is and whether or not that sounds acceptable to each of you. I think there is also some possibility that we can get through some of this this morning with some of the general discussion, perhaps, going a little bit faster, although we are starting late because we had Dr. Ayers' presentation.

Is there any discussion about the proposed change in the schedule?

DR. MACIOLEK: I have a question. If we do get to the point that Bob suggested by 5:00 or 6:00 o'clock tonight, how many of the people in this room will be here tomorrow? I mean, will everyone pick up and leave?

DR. AURAND: Just MMS here to edit your comments.

DR. MACIOLEK: I am concerned about that, because it sounds as if--

DR. AURAND: I don't know.

DR. MACIOLEK: --we could reach a major stopping point.

DR. AURAND: I think that is a reasonable question to ask. One thing, of course, is if you don't check out before 11:00, you might as well stay until tomorrow morning, because you are going to pay for the room.

How many people would be available tomorrow morning if we did it this way?

(Show of hands.)

DR. AURAND: Eight. That's a fair number, I think, not all of them from MMS.

The only thing I would offer for a suggestion is, Bob, were you originally planning to have the two panels in separate rooms?

DR. MILLER: Originally, yes.

DR. AURAND: I would suggest that perhaps the first hour of that discussion might be in separate rooms; otherwise, I see a situation where we have got too much going on in one room.

DR. MILLER: What they were going to do was to divide this room into two, as I understand it, and we would have to re-set these tables.

DR. COOPER: Can't we do it all at one time? There is a lot of overlapping subject matter.

DR. BUTMAN: I was going to suggest in some ways, it makes sense to do a little bit of a synthesis and summary of what we heard yesterday before we discuss the hypotheses. It seems like we are sort of--

DR. AURAND: We were going to use the initial discussion of the hypothesis to guide the discussions of the two panels, but I don't think it is essential that you do it that way.

The idea was that the discussion that would occur this morning then would provide elements for discussion by the two panels, who would then come back together and come up with their conclusions. Certainly, there is no reason that you have to do it that way.

MR. LANE: Also, to give you a chance to further define them or restate them if you found them unacceptable or what-have-you, so that everybody was using the same hypothesis in the afternoon discussions.

DR. AURAND: Bob, I'm getting an itchy finger for either a magic marker or chalk. Is there a blackboard? If we are going to do this, if we are going to try to do summaries as a group, I think we have to have someplace where you can write things down, a flip-chart or anything like that, light view graphs and pens, anything?

DR. TEAL: Don, it seems to me that you've got this organized this wrong way around, that what you are calling the panel discussions, which seems to me to be an effort to define the premises on which you are going to discuss these two hypotheses or some other hypotheses ought to come first.

Really, if you can agree--and it seems to me that there are a number of things that are easy to agree on. For example, on currents, there is a lot of resuspension and strong currents in canyons in relation to the areas around them. There are special environments in canyons that are not found in the rest of the area, all that pueblo structure and boulder fields and so forth.

Those areas are refuges, at least in the sense that they are not trawled over; that there is scavenging by the fines in the canyons, at least the edges of them, an accumulation for potential--perhaps that is all you can say at that level, a potential--accumulation of pollutants that are introduced by any kind of activity in that general area of the oceans in the canyons as a result of that scavenging.

There are a number of things like that and if you can say those things and define them a little bit, I mean, how important are the canyons as nursery areas? We heard that they are nursery areas and that seems unequivocal, but how important are they as nursery areas in relation to what goes on along the whole surface of the bank or the whole area of the slope? That is a question that, until you have some kind of a handle on it, you cannot make a hypothesis about how important an effect might be unless you could dismiss an effect.

DR. AURAND: Okay. Discussion on that point? I think that's a reasonable suggestion.

DR. BUTMAN: In part for preparation for this afternoon, I tried to make a kind of summary list to synthesize a number of the threads that ran through the discussion yesterday and I wrote it on a few viewgraphs.

It would probably take maybe a half an hour or so to go through that, if you want to, as a way to sort of direct the conversation this morning. As a way to direct the conversation this morning, it might be worth it to do that for the first half-hour and then we could go back into the hypothesis if you want.

DR. AURAND: In keeping with John's suggestion, then, why don't we consider letting you go first with the geology and geochemistry and then do biological processes and then come back to the hypothesis? Is that reasonable?

DR. TEAL: Yes, that's what I am suggesting.

DR. AURAND: That's what you are suggesting. Is there a consensus that that is a reasonable way to approach this? I see a consensus. You've got the chair. Everyone should say who you are when you speak up.

GEOLOGY AND GEOCHEMISTRY--DR. BRADFORD BUTMAN, DISCUSSION CHAIR

DR. BUTMAN: I don't know why I volunteer for these things, but anyway, just to make sure we are all talking about the same kind of morphology of canyons, we heard a lot of discussion yesterday about what is what in a canyon.

(Showing of viewgraphs.)

I just wanted to give a sort of schematic of what my picture of a canyon is. I have shown three views here, a plan view and two cross-sections, one across the symmetry of the shelf and one looking up or down the canyon.

Many of us talked about the axis and most of the reason for showing this is to try to say what kind of samples we have to characterize a canyon in these different environments.

Most of the current measurements and most of the geological sampling has all been done along the axis, but a lot of the visual observations from submersibles and from [word unclear], sleds and things like that, have been done on the walls.

Let me first define those environments. We have the axis. We have a large area which I have called the walls, which are referred to as being steep and blocky and in some places they are smooth, but a wall environment, and then rims where the canyon transitions from the shelf onto those walls.

The point I want to make is that the axis, where many of our samples, geological samples, are taken are generally a fairly small part of the total area of the canyon. I think it is important to keep that in mind when we talk about effects on the canyons.

What are we talking about? Are we talking about effects on the walls? Are we talking about effects on the axis? Are we talking about effects along the canyon rim?

As John said, we heard a lot of--I have tried to phrase this summary in terms of questions. We have heard a lot of discussions that canyons are unique habitats or environments as compared to the slope, from the slope at comparable depths.

I have tried to list both the biological features, the physical oceanographic features and the geological features which make that the case. I think it would be nice in the final report, following what Don said, to try to have a very simple layman's summary saying what these things--to enlarge on these. Maybe there is more than this list.

I would like to have some discussion about what other things might be on this list, but I think that should be a centerpiece of the document, of the report from this workshop, what makes canyons unique, in a very simple way.

The first thing I heard was topography, which I just showed in the previous schematic, but also, the roughness in the substrates which are in the features. Barbara and Dick and Page talked about the different physiographic environments in terms of rough substrates, soft substrates, cliffs, et cetera, which make the canyons unique for biological communities.

Both Bob and I talked about the current structures within canyons in that they are decoupled from the shelf and that in many canyons, they are much stronger than on the adjacent shelf or slope at comparable depths.

That leads to the third point, that we see extensive resuspension. At least in some of the major canyons that we have looked at, the resuspension activity at the bottom of the axis is much stronger than on the adjacent slope, and that has implications for pollutant scavenging and for the geochemistry of the canyons.

Barbara showed some nice pictures of the species. Just as another little comment on the report, it would be nice to try to say what canyons we know this about and what canyons we don't know this about, how general these statements are or do we only know them about one or two canyons.

Barbara showed some nice pictures of the species, the diversity of species density, comparing slope versus canyon environments. I think it would be nice to also try to summarize the species composition between slope and density. I think Nancy can also address that.

Dick made a very useful point, I think, in that the canyons are unique in that they are fished. There are topographic features in which we have--

DR. TEAL: Not trawled.

DR. BUTMAN: Not trawled. I will get my pen and change that.

There are a number of us who presented information on the texture, showing that the texture is very different, sufficient sediment texture is very different from the adjacent slope.

Finally, Mike's data on the lead-210 inventory suggests that because of this resuspension, there is a different geochemical environment in canyons versus the open slope.

Those are the things that I heard yesterday and I don't know if we should have discussion now. I have a number of--what I then did is I went through and I asked a number of questions which I think are important for addressing the hypothesis which Don and Jim want to address.

I don't know how you want to organize the discussion. Are there other features we should add to this or is that a fairly complete list? That needs to be embellished or fleshed out, but that is, at least, a beginning list.

DR. BOTHNER: You might want to add the words "sediment accumulation," as well, "enhanced sediment accumulation." Of course, that applies to one canyon that we know of; I mean, that's the caveat in all of this.

DR. BUTMAN: Actually, in some cases, it is enhanced; in some cases, it's not.

DR. TEAL: That's true, perhaps, but not true of Oceanographers; that's what I gathered from the talks yesterday.

DR. COOPER: Brad, we are talking here about features that make these canyonheads as a physiographic environment unique; right? How would you compare the sedimentation here versus the mud hole south of the Cape?

DR. HECKER: Brad, also, it is not just species density; it is also diversity and there is a difference. The predominant trophic structure in the canyon itself is different. I mean, you are going from mobile carnivores to filter feeders to sessile filter feeders.

DR. BUTMAN: Community structure?

DR. HECKER: I would say feeding strategy and community structure is a lot more complex.

DR. MACIOLEK: Well, note that that's for the upper fauna.

DR. AURAND: The court reporter has a strained look on her face. If you could identify yourselves when you make your comments, okay?

DR. BUTMAN: Does that cover it? You are going to get to write a paragraph that says what that means.

DR. HECKER: I would put in just patchiness, faunal patchiness. It's just much higher.

DR. BUTMAN: Variability?

Dick, that's an interesting question, the sedimentation rate. What is that to other deposition layers that we know about? Is it higher or is it lower? That seemed to be a major thread which ran through all the presentations, that some canyons have lack of accumulation and some canyons had accumulation. That was a feature which we all thought should be addressed as to what makes them unique in one way or another.

DR. AYERS: Does that depend on currents?

DR. BUTMAN: Yes.

DR. AYERS: If you've got energetic currents, I don't see how you are going to get a depositional area.

DR. BUTMAN: Well, it seems--

DR. AYERS: In Toms Canyon, we found currents to be weaker than on the shelf.

DR. BUTMAN: It seems like in some canyons, they are weaker and in some canyons, they are stronger.

DR. AYERS: We had more silt and clay in the canyon than we did on the shelf.

DR. BUTMAN: Right. But there are some canyons which are the opposite of that, for instance, Oceanographer Canyon has stronger currents and probably coarser sediments than are on the shelf.

DR. AYERS: You wouldn't consider that a depositional area, then, would you?

DR. BUTMAN: Oceanographer Canyon? No, no. But I might consider Toms Canyon a depositional area.

DR. AYERS: Right. The currents are weaker there.

DR. BUTMAN: Right. We want to decouple that, so they could be--in some canyons, they are stronger; in some canyons, they are weaker. We can't really say. Just like accumulation, in some cases--

DR. AYERS: Some yes, some no.

DR. KRAEUTER: I think, also, in the currents, we need to say something about whether they are up the canyons or down the canyons. They are not only decoupled, but they may be moving--I couldn't figure out whether they were moving up or down or in an oscillatory motion or what's going on.

DR. BUTMAN: I think that it is clear that they go in both directions. They are primarily oscillatory up and down the canyon and the net flow, I think, is up for grabs. In some cases, we've seen it down canyon; in some places, we've seen it up canyon.

Even if you see it from our measurements being up canyon or down canyon, what that means in terms of net transport, I think, is still an open question.

DR. TEAL: I am still convinced that to really define that, I think you are going to have to be out there during, you know, the most extreme events.

DR. BUTMAN: That may be true in some canyons if you want to say extremes.

DR. COOPER: Brad, Dick Cooper. The two hypotheses that we are about ready to address here refer specifically to canyonheads.

DR. BUTMAN: Right.

DR. COOPER: There is a lot of difference between that and a submarine canyon.

DR. AURAND: You don't have to restrict yourselves to canyonheads.

DR. BUTMAN: Let's go back to this little sketch which I showed you first. That's another point which I was going to bring up later, that almost all of the discussion, with the exception of some of the biology work, most of the process work and geochemistry work has all been done landward of that shelf environment.

I don't think we really want to restrict ourselves, but I think just from what we know, we may be only able to say what we can say, we may only be able to say it about the--

DR. TEAL: Certainly, what we care about is concentrated in the upper regions, anyway, that are studied, the best known and connected with commercial resources.

DR. BUTMAN: On the lead-210, should I say geochemistry, in terms of what we should have in there.

I think it would be nice if, in the final report, we could have a very succinct or fairly brief discussion of those things which make canyons unique and that we can all agree on them. We all go around saying that canyons are unique environments, but we must have list we could all tick off.

DR. TEAL: I would, however, like to echo what Jim said. Canyons are not unique environments; they are special environments.

DR. BUTMAN: Okay.

DR. TEAL: I would like you to all cross out the word "unique" everywhere.

DR. BUTMAN: What is the difference between special and unique?

DR. TEAL: Unique means there is only one.

DR. BUTMAN: Okay.

DR. TEAL: Everything is different. It does not add anything to talk about them as "unique."

DR. COOPER: However, if you are talking about the heads of canyons, altogether as a category, then I strongly--may I do and maybe I don't disagree with John here.

I really strongly feel that, having spent a lot of years diving a lot of areas, these canyonhead environments, as a group, the canyonhead environment itself, is a very unique feature, not to be found anywhere else on earth. If, in fact, we are trying to document and, in the world of politics that we live in, create the perception that these are important, special environments, to me, the term "unique" carries more weight there than "special" or anything else.

DR. TEAL: If you use it carefully. It is the overuse of the word "unique" that makes it valueless, so if we are going to use it, let's use it correctly. The canyonheads, as a group, are unique.

DR. COOPER: As a category.

DR. TEAL: Okay. That I don't object to. I still object to "very unique."

MR. LANE: As a category of biological habitat, sure, we use the term "special environment," but in the same sense that coral reefs are unique, even though there are many coral reefs. Let's define that usage.

DR. TEAL: I think that your idea of a paragraph on each one of these to explain your three words, as long as the paragraph doesn't have more than about a 100 words in it and the people who talked about those things yesterday, if they could put a 100 words down, that would say what that means, then that is an enormous step forward in making a valuable document.

It is hard to do. It is much harder to write something in a 100 words than in 500 or 10,000, but then the people who know most about it in the world, I mean, you are sitting here. If you can't do it, no one can.

DR. COOPER: We are a very unique group.

(Laughter)

DR. AURAND: I think that is a very reasonable idea. I think, at some point, and I don't know where, but to have the people who are concerned with each one of those--it doesn't have to be one; it could be a group, depending on the topic--write the paragraph and then bring it back for everybody to look at.

DR. BUTMAN: It would be nice, also, if we could have, because we are all scientists, I'd like to see references in there, not necessarily in this document. But I'd like to see those paragraphs backed up with what data we are using to support that.

DR. AURAND: Actually, that's an interesting comment because when I was looking through this last night, I was giving some thought to that, too.

It would be nice if there were references associated with all of this. I don't know what provision Bob and Jim had thought about with that, but there probably needs to be some way to track that.

Once we start to summarize the individual paragraphs, there needs to be some way to track back to where you got the information.

DR. BUTMAN: For example, one of the statements which we all make about canyons are that they are very productive environments or heavily fished environments. I have never seen, you know, specific numbers of crab pots per unit of canyon area versus open sloping. Maybe that exists.

DR. COOPER: It doesn't exist and you won't see it. We can make it up. There are fisheries decisions that have been made for 100s of years, made on the best guesstimates.

DR. AURAND: In that case, I think that is also an important conclusion to put into the document.

DR. HECKER: The density of lobster pots that I maintain go in canyons is about six fold over the slope, okay? There is your personal communication.

The ghost gear in canyons--diving, I have dived on the slope, at the slope/shelf break and I have dived in the canyon heads with you and with you, and the amount of ghost gear around and the amount of fishing gear around in canyons is much higher than on the slope.

DR. AURAND: When you have a reference, when you know there is data, we should say where the data is available and when, in fact, it represents a best professional judgment, which is a more polite term for saying what you just said, we should say that, as well. I think that is important.

DR. TEAL: Could I make a suggestion? I absolutely agree, but I would like to see the 100 words not references.

DR. AURAND: I agree with that.

DR. TEAL: I would like to see a 100 words without anything except easy-to-read sentences on how many pages that takes, about 4 pages to put all that down on. Then another, maybe, the same kind of thing in the same order, that says where those paragraphs came from.

But the people who read the paragraph and the people who read where the paragraph came from will be the same people in many cases, but in a lot of cases, they won't be. Having all the caveats and the sources and everything in with the paragraph will mean that a lot of people won't read it.

MS. HUGHES: So, the reference can be in within 10 days or whatever this group has to review the final document and provide their comments back.

DR. AURAND: Sure.

DR. BUTMAN: I don't hear any major additions or changes to this list. Certainly, we can add things if we want to later on. Are there any other things which ought to be on there?

DR. TEAL: I think maybe all the biological species, the community structure and so forth, really ought to be separated into infauna and epifauna. The conclusions are different, and I think it would make it easier both for you to write and for people to read.

DR. HECKER: There is very little known about the infauna.

DR. AURAND: Are we going to go through and do this separately now for the biological questions or are we making a combined list for both of the two? Really, we are looking at a geology and geochemistry group and a biology group. Are we now working on a list for both groups?

DR. BUTMAN: I think this part so far, that canyons are special habitats, are both.

DR. AURAND: All right. In that case, does species diversity and density address the issue of the role of canyons in fisheries, or am I missing that somewhere?

DR. BUTMAN: That's a good point.

DR. TEAL: It says "untrawled" up there, but that ought to be expanded into the general role of fisheries.

DR. AURAND: Fisheries, nursery ground, refuge, all of that.

DR. TEAL: Yes, yes.

DR. AURAND: Did you have something to add?

DR. TEAL: Actually, there might be two paragraphs there. That is sufficiently important, it seems to me. One having to do with the fishing impact, the accumulation of ghost gear, the reasons for it and so forth; and, another one having to do with the importance as nursery grounds, refuge areas, because that issue in itself is very, very important to people who are interested in the whole issue of allowing or not allowing drilling.

DR. VALENTINE: Well, we have to also include the fact that this lobster and trawl fish issue is in.

DR. TEAL: That's what I say. It is not just that you don't trawl there, but that you do have traps.

MR. LANE: How are you going to integrate the discussion of impact producing agents or activities? Is that going to be done for each of the habitats or discussion topics, or is that going to be a separate section, saying that now we know this about the habitat and now that we know this about the flow regime, this is what it means in terms of OCS activities and their impact in these areas.

DR. TEAL: I was proposing, when I initially spoke, that that come separate. This is what we know about it now and then, from that knowledge, what can we say about the impacts?

DR. BUTMAN: This is a motherhood statement about canyons and why they are special and why we care about them. That was one thread that I saw in the discussion yesterday.

Then, primarily from a physical point of view, I tried to list four or five questions here which I think we need to answer before we address the hypotheses and I will just read them to you. Then, if you think it is reasonable, we can discuss them.

The first one, and you can state this either as a question or as a hypothesis. I am just sort of paraphrasing it here. The first one is: Particles enter the canyon from the shelf. That's important because we want to know, if there is drilling around the margin, that the particles from that drilling activity will enter the canyon. That is one question.

The second question or statement is: Fine grain sediments accumulate in canyons.

The third one is: What is the potential for accumulation in canyons?

A fourth one is: What are the unique characteristics of canyons that might make results from previous studies at OCS effects on drilling inapplicable? What is special about a canyon so that we cannot summarize from, say, the Georges Bank monitoring program, what is going on in canyons.

Finally, I had a list, from what I saw yesterday, of what were some of the limitations of the available data that we have. What things do we know? There were a number of comments made where we don't know certain things very well that we need to know more.

For each one of those questions, I have tried to make a list of sort of the highlights of the discussion yesterday. For example, particles entering the canyon from the shelf, I saw five things, five pieces of data which were presented yesterday, and on the right-hand side, I have a little table.

This is the answer, yes or no, they do or they don't, whether the evidence is actually direct evidence or whether it is indirect, whether we have to infer something or whether we actually can measure whether particles are coming from the shelf, and then what canyon it applies to. Do we know it applies to all of them or is it a specific canyon?

For example, I said particles enter the canyon from the shelf, and the data I showed of the flow regime, showing flow directly from the canyon, from the adjacent shelf into the canyon and some calculations. It says that yes, particles do enter the canyon from the shelf.

It is essentially an indirect calculation because it is an indirect measure, because I don't actually measure the particles entering the canyon, I infer it. That was primarily for Lydonia, but I think from what we know about the residual circulation on the continental margin that we can pretty much say that is true of all the submarine canyons along the southern flank of Georges Bank.

Actually, I had another column here which I couldn't fit on, which actually said this. But I said that. Mike showed some data showing direct measures of increased barium in the canyon axis and sediment traps during the course of drilling on Georges Bank.

That was in Lydonia Canyon. That is a hard, direct measure of particles which we know were ejected on the shelf and we actually found them in the canyon, so I say the answer there is yes.

Page talked about observations along especially the east rims of canyons showing ripples migrating to the west into the canyon. He showed those both in Oceanographer and Lydonia. It is a somewhat indirect measure but, again, the answer is yes. Actually, the answer is yes to all of these.

Dave Twitchell's high-resolution surveys in the head of the Lydonia Canyon again show this wedge of fine sands and silts with a major wedge in the head of the canyon but thinning out onto the shelf adjacent to the canyon so, again, that is sort of indirect evidence, but it does suggest that particles enter the canyon from the shelf.

Finally, Mike presented some accumulation rates in the head of Lydonia Canyon--only Lydonia Canyon. I think that's actually the only direct accumulation rates we have in any canyons, is at about 150 meters in the head of Lydonia Canyon.

Again, I call that indirect evidence. It is not absolutely certain that that material is coming from the shelf. It could actually be coming from the canyon walls, but I think in that case, it is pretty definite that the particles are coming from the shelf.

I think at least one major conclusion is that particles do at least cross over the rim and enter the canyon from shelf environments. Based on this data, from areas quite--we don't actually know the zone of influence, where the particles come from, but based on this data, it's at least 5 or 10 kilometers, the distance of the closest rig.

Is there a discussion on that? Is that an important question to address?

DR. VALENTINE: I would like to modify that a bit.

DR. BOTHNER: Okay.

DR. VALENTINE: Concerning the third line, the submersible observations at the rim, well, we had observations at the rim, on the wall and in the axis. We are pretty sure that the canyons are underlain by this Pleistocene silt and clay. At least, we have never seen any outcrops of unconsolidated coarse grain sediment in these parts of the canyons.

Given that and the fact that we observed coarse-grained sediment at the rim, on the walls and in the axis, I would say that is direct evidence that sediment is moving from the shelf into the canyon.

DR. BUTMAN: It's not just submersible observations at the rim, but it's the texture of the walls.

DR. VALENTINE: And the floor.

DR. BUTMAN: So, I shouldn't call it submersible observations. You are actually saying it's texture observations throughout the canyon.

DR. VALENTINE: Yes, submersible observations and direct sampling of the sediment. I mean, we can see that it is sand, but we have also sampled it and determined that it is sand. We know that it occurs nowhere else but on the shelf, so it has to come from the shelf.

DR. BUTMAN: Are you saying that is true in Lydonia, too, the walls of Lydonia are sandy?

DR. VALENTINE: I don't know as much about Lydonia, but I know that in Oceanographer, Gilbert, and Hydrographer, they all have the same sort of axial sediments. So, I feel confident that those three canyons are receiving sand from the shelf and, by analogy, the others are, too, although it may be mixed with finer grained material.

DR. BUTMAN: That's a good point. Maybe we should qualify this, fine versus sand, versus finest. What you just said was "sand," not silt and clays.

DR. VALENTINE: Right. You can't separate out the silt and clays from the material coming from the walls of the canyon itself through bio-erosion, from this fine grained stuff coming off the shelf.

DR. BUTMAN: Right. The reason I posed this question is, as I said, if there is activity around the rim, we want to know is that going into the canyon or is that somehow going to bypass the canyon? I think there is evidence at least some fraction of the material on the shelf is entering the canyon system.

The question is: What happens in the canyon? That's the next question, though.

DR. KRAEUTER: You've changed now. We're just talking about sands on these?

DR. BUTMAN: No, we are still talking about both.
DR. KRAEUTER: You're talking about both?
DR. BUTMAN: But I just thought that was an interesting and important qualification.
DR. KRAEUTER: I was thinking about what we had just above that, in canyons being unique. How can we compare this to the adjacent shelf? In other words, increased barium following drilling on the slope. Would we see it there, for instance, mud patch?
DR. BUTMAN: That's a good question.
DR. KRAEUTER: Here we are defining things that may be general processes on the whole area or are we just talking about things that are unique?
DR. BUTMAN: I guess in this case, I was saying, because the workshop is focusing on contaminant effects in canyons, at least, this question was posed directly towards canyons. But you are right. Some of that may actually be occurring on the slope.
DR. KRAEUTER: Then it becomes a question of how much more, how much less, which is much more difficult.
DR. BUTMAN: Although this mechanism is particularly applicable. We don't have the same information about transported particles from the shelf to the slope as we do from the shelf to the canyon. If you want to say particles enter the slope from the shelf, we could make another list of information, I think, that would address that and maybe we should do that.
DR. AURAND: Maybe you need to just re-word the statement. I think the question was whether or not there was any reason to think that canyons would not receive material. The answer is: No, there is no reason to think that they would not receive material. You said that at one point, too.
DR. BUTMAN: Right. In fact, there is reason to receive it.
DR. AURAND: So I think that is a reasonable statement to make someplace. I am not sure how you want to word it, but it is reasonable to say there is no evidence that they won't receive material from

adjacent areas; in fact, the evidence would support the conclusion that they will receive materials.

DR. TEAL: I think the evidence is stronger than that. The evidence is that they receive relatively more material from the shelf than the general slope.

DR. KRAEUTER: That is really what I was getting at. We are talking about something that you need a greater--

DR. BUTMAN: I'm not sure we do. Do we know that?

DR. TEAL: That's a good question. That's what I saw some of your geochemical studies as saying, that there was a relative increase in the material accumulating in the canyon.

DR. BOTHNER: Again, it is a question of which canyon you are looking at, but the small amount of data would suggest that the potential for accumulating contaminants is higher in Lydonia Canyon than it is on the slope.

I would love to analyze samples from Oceanographer to see if that holds true. In spite of the fact that we think that is an erosional area, I think there is still a potential for intense scavenging in Lydonia Canyon by the very small amount of fine grained material that exists in the bottom sediments, even within the axis, that will be very important in enhancing the pollutant load for those pollutants that are reactive to surfaces of particles.

DR. BUTMAN: I guess I was thinking that this is particles. Whether they actually increase the--so, all right. Is there any other information we heard discussed that should be added to that list which says that particles--either sand or fines--enter the canyon from the shelf?

Does everybody agree that that is true?

DR. RAY: I want to come back and follow up, ask something else, following up on what John just asked and that was, in the perception of the canyons in this area as to whether or not they are--you know, you are saying they are a primary mechanism for the transport of particulate off the shelf and onto the slope.

The perception in California along that coast is that those canyons are pretty actively picking up the long-shore transport of particulates as they conduit down off the slope, but here, you are not saying that. Is that what I have heard in the discussion here?

Are the canyons or are not the canyons, in your mind, a major conduit of material off onto the slopes?

DR. BUTMAN: I think all I am addressing here is this, that stuff comes in and is going like this (indicating), but we have not addressed what comes out.

You are saying that in addition to being a vacuum cleaner from the shelf, that they also--stuff accumulates and may move out into the deeper water in some other mechanism. I think that we really don't have much data.

We really haven't addressed the issue of what happens to the--

DR. RAY: I was asking it as a question. I wasn't stating it either way. I was just trying to get a clarification because, you know, John raised a point there and it was still hanging. I wasn't sure whether we had really defined, you know, the active role of these canyons as far as transporting stuff off the shelf.

What you are saying is we really don't know; is that what you are saying?

DR. BUTMAN: I'm saying we haven't addressed it yet this morning, yes.

DR. RAY: Okay.

DR. AYERS: If we do choose to address it, you know, why wouldn't--logically, you would think if they were conduits, I mean, why wouldn't they fill up over time if they were? You know, they've been there for eons and sediment has been going into them and yet, they are not full.

DR. TEAL: I had that problem yesterday, too. The geologists don't seem to have a very good idea of how fast stuff is accumulating and not accumulating. I still, I guess, see them probably as a conduit to deeper water. It isn't clear to me from what we saw in the data yesterday that you can support that idea.

DR. HECKER: At least the data from Baltimore Canyon seems to show that it is not just--you know, it is always the thought that you've got material going from the shelf to the canyons down the slope and you get this feeling of a sort of chute.

Basically, what Brad showed with regard to Baltimore Canyon is that you are affecting the whole water column, actually, because you are invecting material out at all areas due to the resuspension, particularly near the canyon head. Your resuspending then gets invected out.

DR. BUTMAN: Ken and Will showed that stuff.

DR. HECKER: This was some work done at Lamont with regard to some work I was doing in Baltimore Canyon. Their feeling was that it was not just going out to deeper water but, in fact, material was coming out of the canyon and then was being picked up by the currents and being carried along the slope.

In fact, rather than having material moving out straight this way, you've got material coming out into the water column and then it is going out along the slope, also, as well.

DR. BUTMAN: I think a reasonable hypothesis might be that canyons are leaky traps. That's what you're saying.

DR. HECKER: I am saying it is not that straightforward. You are affecting the water column and you are affecting the slope on the west side.

In fact, some of my data with regard to the distribution of filter feeders, particularly in Baltimore Canyon, where it is very--where the axis bends, you've got a much higher concentration of filter feeders on the west wall which would add into fine material coming right--being invected out there that way.

There is a bunch of evidence that seems to show that there is an accumulation out on the west wall, at least down in that canyon.

DR. TEAL: That supports the idea that it doesn't all just accumulate there.

DR. BUTMAN: That's right.

DR. HECKER: But it goes all over the place. What I am trying to say is that it is not straightforward that it just goes to the deep sea.

DR. TEAL: Right.

DR. BUTMAN: It may actually be a source of sediments to the slope downstream of the canyon mouth.

DR. KRAEUTER: Let me follow that up, because yesterday, I was puzzling about the source of the sediments and how things got up and down and where they were. The thing I was finally trying to mentally compare it to were the inlets along barrier islands where you have both an ebb and a flood tidal delta.

You can get the fines deposited at both ends, which all sorts of things happening in between. I don't know whether that is the proper way to look at this or what we are saying now. Everybody seems to be thinking everything is going down.

The way I see the evidence is I don't see it is that clear, but maybe my idea is not the appropriate one. But that is the way it seemed to me, that the fines are accumulating like in an ebb and a flood tidal delta.

DR. BUTMAN: I think what we would all like is a mass balance of sediments that come in and where do they go? How long do they stay there and where do they go? I guess my perspective is not quite ready to answer that question.

These are some building block questions that we have got to go to first things first, that particles are entering and then we have got to say what happens to them once they get there. Our long-term objective would be to make some kind of a mass balance, both in the size and the fine particles for each one of those.

DR. VALENTINE: I'd like to make one point here, that the California canyons are a much different geological environment than these. Their heads are closer to shore, are topographically high, an eroding coast. There is a lot more sediment supply. They are catching a lot more sediment. Mass movement is probably the main reason for the transport of the sediment to the deeper water.

Whereas here, we are far from shore, deep water, very little sediment relative to California is entering these canyons, so we have I think the deposition in the canyons of probably, I would say, most of the coarse grain material is staying in there, whereas you might have some fine grain transport out towards the slope. But it is a much different situation than California.

DR. BUTMAN: Is there evidence on the slope for mass movement of material from the canyons?

DR. VALENTINE: In the Gloria side-scan survey, there are very large mass movements feature off the Middle Atlantic, off Georges Bank, too, but who knows how old they are.

DR. BUTMAN: Right.

DR. VALENTINE: Those might be involving the whole slope and there are conduits from the canyons to deeper water and you can see effluvial geomorphology out there, but there is no knowledge on the age of these things or whether they are even active now.

DR. TEAL: It is true that you have to keep in mind that these canyons may have been more active conduits for mass motion than at present, so the fact that it has happened in the past, I think, can probably be documented, but how frequently it will happen under the present circumstances is really very much in question.

DR. BUTMAN: Maybe Page has a better hypothesis that the sands stay there and in some canyons, the fines either stay or move, at least for the east coast canyons.

DR. VALENTINE: We don't have good information on the sediment texture in the deeper parts of the canyon axis out on the slope.

DR. BUTMAN: That's right. That's a really good point.. I made that at the beginning. We really only have looked at the texture primarily in the axis. You've looked at the texture on the slopes, but for instance, that Lydonia stuff is primarily on the axis and it doesn't go much deeper than 500 to 1,000 meters.

DR. VALENTINE: There's a way you could tell if there is sand coming out of the canyons. You could sample in the deeper parts of the axis. Knowing the size of the material in the silt and clay that blankets the canyons and the slope, it is mostly silt and clay. There is some sand in there, but very fine grained.

If you find coarser sand in the deep axis, then you can make the assumption that it is coming from the canyons because there is no other place it could come from.

DR. BUTMAN: Because it is not on the slope.

DR. VALENTINE: But we don't have that data.

DR. BUTMAN: Right. We'll add that to the list of limitations. I'll do that right now.

Here is the next question or the next statement. The fine grained sediments accumulate in the canyons. I said fine grains, because we are interested more in pollutant accumulation rather than the coarser grained sediments.

Maybe we should say both fine grain and sands, but this list is really for--this evidence is really more for the finer sands and silts and clays rather than the coarser sands.

Again, I think the most direct evidence is in Lydonia Canyon and it was the measured accumulation rates which Mike showed from piston cores in the head at about 150 meters, which showed accumulation rates of 60 centimeters per 1,000 years. That was in two locations and that is fairly clear, direct evidence.

Page used the texture and bed forms to infer that in Oceanographer Canyon there is no accumulation of fine-grained sediments, at least along the axis, whereas in Lydonia Canyon, we used texture and the absence of bed forms to indicate that there probably is at least some net accumulation.

Again, the high resolution profiles at the head of Lydonia, which show a thick wedge of sediment in the same area of the measured accumulation again, is slightly indirect evidence but, again, I think fairly solid evidence that there is accumulation in the head of Lydonia, anyway.

I don't think we actually have that data in the head of Oceanographer, do we? Has there been any high resolution work in the head of Oceanographer showing any--

DR. VALENTINE: A couple of side-scan passes across the very narrow part, but I don't know if there were seismics.

DR. BUTMAN: I don't think there were any seismics. I'm not sure. In my talk, I suggested a convergence towards the head of Lydonia, which at least is consistent with the finer-grain sediments at the head and then, actually, I listed bed forms and texture again to take care of Oceanographer, but I put them up there as a yes and a no.

I think here, the issue is it really depends on what canyon you are in and what area of the canyon you are in. In some cases, there are and, again, I think we are probably talking about the axis here, not the walls. There may be fine-grain accumulation on the walls, but here, the answers vary depending on what canyon environment you are in or what canyon you are in.

Is there discussion on that?

DR. BOTHNER: That is probably not as controversial a list because you have just covered it in the previous question.

DR. BUTMAN: Good.

DR. KRAEUTER: Kraueter. I've got another question. That's fine-grain sediments accumulate in canyons; I think that is true. Do we have any evidence that they don't accumulate in canyons?

DR. BUTMAN: Yes. In Oceanographer, there is evidence from the bed forms and the texture in the axis that they don't.

DR. KRAEUTER: That is what that "no" really means.

DR. BUTMAN: It means "yes" in Lydonia and "no" in Oceanographer.

DR. KRAEUTER: But that's different. What I am saying is: That they don't accumulate is a separate question. I thought you were saying yes, we have evidence that they do accumulate in all those cases and that the "no" means no, we don't have evidence that they accumulate.

DR. BUTMAN: That's not evidence. That means yes, they do and no, they don't. The evidence where we list it under the canyons, in Oceanographer, we have evidence that they don't accumulate and in Lydonia, we have evidence that they do accumulate.

DR. KRAEUTER: That's fine. Fine.

DR. VALENTINE: It seems to me that that's direct evidence.

DR. BUTMAN: All right. Page, you are changing all your "indirects" to "directs."

DR. VALENTINE: Yours, too. You are saying a layer of fine grain sediment in Lydonia is obviously accumulating in there, even if it's coming from the canyon walls; it is still accumulating on the floor.

DR. BUTMAN: Okay. Maybe the "direct" and "indirect" isn't an important distinction.

DR. NEFF: Do we know where the fine-grain sediment is from, the shelf or around the heads of the canyons? Is it coming from the canyons or from another source? That relates to these questions, if the canyons can be a source of fine-grained sediments.

DR. TEAL: I gathered from all that you were saying yesterday is that is not known. We don't know where those fines up at the head of the canyons are coming from.

DR. BUTMAN: No. But you are saying the canyons may also--it is absurd that there are finer-grained sediments around the heads of canyons relative to the same depths on the existing shelf. Is that something caused by the canyon environment?

DR. NEFF: Right. Could the canyons be the source?

DR. BUTMAN: Right.

DR. KRAEUTER: Again, I think the term is "accumulate." Obviously, if we say yes, they have to be filling up. It depends really on the time scale of what we mean by accumulate. Just the fact that we've got a layer down there may mean that's a very temporary thing and it is going to be resuspended and gotten out of there or it is just a pass-through as a conduit or something.

We've got a time scale we are dealing with here and that creates some problems for me, trying to figure out what accumulate means. An accumulation, to me, means it is slowly filling up and I don't know whether that's true or not.

DR. BUTMAN: Argue with me on the evidence. I think here, this says they are slowly filling up. On the time scale of the last 10,000 years, in the head of Lydonia Canyon, I would argue that they are filling up.

DR. KRAEUTER: Change that to the last 3 or 4 thousand years. That's even more important, though, because sea level was near its normal level.

DR. BUTMAN: So, how about we say in the--do you want to put a time on that?

DR. TEAL: January 4, 2000 BC.

(Laughter)

DR. KRAEUTER: Then we can say something about rates. Do you know how deep the sediments are and things like that?

DR. BUTMAN: All right. No more discussion on that one in the interests of moving along.

The other question I asked is: What is the potential you could say contaminants accumulate in canyons? Is this different from--the subtle distinction between sediments accumulating is the resuspension issue that sediments can strip contaminants from the water column.

So, the evidence here is we have a lot of direct evidence of sediment resuspension. We have sediment trap measurements. We have more ray and light transmission observations, beam attenuation measurements. We have current measurements which show that they are stronger than the threshold for the existing sediments.

At least, in Lydonia, we know that there is indirect evidence that the process that can cause stripping is occurring. I guess you would probably call that "direct," also. We have also seen the currents are very strong in Oceanographer, but whether they actually accumulate there--and, thus, stripping--we don't really know.

As Mike just said, we need to analyze some additional sediments from an erosional canyon where the fines aren't accumulating to see whether there may be still some stripping and contaminants.

Then the two pieces of chemical and geochemical information from Lydonia that say that's actually occurring is both the lead-210 inventory and the lead-210 concentration per unit mass.

DR. BOTHNER: You might add the fact that plutonium shows the same, not just one isotope but two, that helps with this.

DR. BUTMAN: What is the--

DR. BOTHNER: 239 and 240.

DR. NEFF: Of course, there is another thing for stripping. First, you have to have particles in the water column if you are talking about absorption and then you have to have something in solution to absorb.

The question I would have is related to drilling. What soluble contaminants would you have in the bottom water and at the head of the canyon available to be absorbed onto the particles?

DR. AURAND: I think it is reasonable to do it the way he is approaching it, in that: Does the mechanism exist? Yes or no. Then, I assume, we go from that to: Given that the mechanism exists, is there any interaction with oil and gas activity that would allow this mechanism to play a role?

That is what you are now addressing, where it would come into play.

DR. NEFF: Right.

DR. BUTMAN: Are there other chemicals, other trace metals, radioisotopes which say that stripping is occurring.

DR. BOTHNER: There are a few other metals that show the same pattern so, yes, there is a slight--it is a slight indication there on the basis of other sediment reactive elements.

DR. BUTMAN: I think Jerry made a really good point that it is important to distinguish. We have established that a mechanism is there for natural--for isotopes and for metal which are in suspension now. What that means for drilling is a separate and another question.

DR. RAY: Jim Ray. In the case of metals, Mike, for resuspended sediments to act as a scavenger, what species would metals have to be coming by in the water to be scavenged out by suspended particulate, for it to actually be an active mechanism to actually attach onto those metal species as they come by? What form do they have to be in for that to be occurring?

DR. BOTHNER: Well, I guess they would have to be in an ionic state. They would have to be truly dissolved and the absorber could be something like manganese and iron oxides, hydroxides, which are being formed at the water/sediment interface continuously, given reducing conditions in the sediments below.

I envision that these surfaces are--you know, being generated at the sea floor, they are being resuspended and then they would grab whatever is in solution. As you know, all these metals are in solution at some low concentration all the time.

DR. RAY: Okay.

DR. BUTMAN: The issue, then, following what Jerry also said is that if dissolved constituents are in the water column from drilling, the question of whether they would get incorporated into sediments is a question of water mixing, not particle transplant, actually.

DR. BOTHNER: It's both.

DR. TEAL: It is both because the solubility of some of these things is so low that the accumulation by stripping could take a much longer time, at least in theory, speaking from ignorance, compared to the transit time or the dilution rate.

DR. BUTMAN: But the water--we talked about particles entering the canyon, but the water with dissolved contaminants discharged to the surface, there may be no mechanism for that ever to get into this zone where stripping would occur. That might just go off to the southwest with the mean flow or something like that.

But if they are absorbed in particles immediately in the surface zone and the particles are trapped in the canyon, that is another--but I didn't ask the question of surface waters from the shelf enter the deep water in the canyon carrying its contaminants with it, which dissolves it. Mike?

DR. BOTHNER: Well, this gets, of course, more complicated, but given that as the scenario here, that we are going to discharge some dissolved contaminants in the surface water, there, because of the normal biological recycling of particles and the incorporation of dissolved constituents into the final point, for example, that's been a well documented mechanism for getting dissolved constituents into the particle phase and then rocketing them to the bottom with fecal pellets as those bioplankton are being grazed upon.

I mean that is part of the equation, I think, in terms of getting material from the surface in the deeper waters. Then it becomes a question of: Do these particles that are generated nearby get into the canyon because now we have put it onto the particulate phase.

I think that is a fair description of what happens to dissolved constituents in the surface water.

DR. BUTMAN: All right, so there would be many ways to get there.

DR. AYERS: I guess if we are talking about dissolved constituents of drilling, is that what we are concerned about? I mean, there are some other sources, yes, but it is very, very difficult to ever measure any dissolved constituents from drilling.

I have tried to measure soluble chromium, for example. You can't measure dissolved chromium in the discharge immediately beneath the discharge column. It is in extremely low concentrations.

DR. TEAL: That comes up later when we talk about that. But the mechanisms exist if there were dissolved chromium, so then the question "is there any"; that's another question.

MR. LANE: We are also interested in other dissolved constituents like hydrocarbons, as well, if you want to talk about it in that context, too.

DR. KRAEUTER: We have another potential for accumulation once it is on the bottom, assuming you are out of these zones of extremely active resuspensions, the mixing, down into the sediments.

DR. BUTMAN: That's a good point. So, that is vertical mixing within sediments.

DR. RAY: I have one other question. In your lead-210 work, do you actually find any stratification in those sediment areas in the canyon that actually allow you to date, or do you have a jumbled pattern of lead-210 as far as trying to date it?

When you take a look at the core section of those sediments, are you getting stratification where you can actually date it or are you really getting a jumbled pattern of sediments as far as lead-210 is concerned?

DR. BOTHNER: Let's see. I would describe it as--I would say that the cores that we look at do not show stratification from the standpoint of textural variability. You don't find a sand layer on top of a clay layer or that sort of thing.

I also would say that there is not a jumbled pattern of lead-210 as a function of depth but, rather, a uniform, logarithmic, nearly logarithmic, decrease in lead-210 activity which suggests fairly uniform mixing; that is, uniformly decreasing mixing intensity as a function of depth. That is the way I interpret the lead-210 profile.

So, the end result is that you cannot use lead-210 independently to date these sediments because the mixing is so intense.

DR. VALENTINE: Page Valentine. I think we have indirect evidence that there is sediment resuspension, refined sediment resuspension, in Oceanographer. You only have Lydonia up there.

DR. BUTMAN: Actually, I agree. In your direct submersible observations, we see there definitely is resuspension; it is just not as--I guess the reason why I didn't put Oceanographer there was that the accumulation of fine-grained sediments, the stripping is there, potentially there, but whether it actually stays in Oceanographer is the question.

DR. VALENTINE: But I mean the mechanism is there to get the fines up to strip and get transported somewhere else and settled out.

DR. BUTMAN: That's right and also, for this vertical mixing, it may not actually have to accumulate, but it may be mixed down into the sediments or it might stay there.

Are there any biological processes which would--I don't know, filter feeders or something like that, that would enhance the potential for accumulation in the canyons?

DR. TEAL: Filter feeding.

DR. KRAEUTER: Filter feeding would certainly do it and it would give you a mechanism to pelletize anything that was stripped out, putting it down into the sediments and then the deposit feeders or whatever would mix it down into the sediment, so you have got a direct link to accumulation and retention.

You have got a dominance of filter feeders all the way along there, as Barbara has shown.

DR. HECKER: That's possible with the fine material, also, stripping the water column and with resuspension and I suspect resuspension is important in supporting some of the filter feeding populations.

As that gets continually wafted up and strips the water column, it keeps going like this (indicating) and keeps passing past those filter feeders and they keep taking it, you are going to get more and more accumulation.

DR. KRAEUTER: Right.

DR. BUTMAN: Dick.

DR. COOPER: Dick Cooper. There is another mechanism of vertical transport of contaminants down to the ocean floor that we haven't considered yet, I don't believe, and that's these krill that occur up to 1,000 animals per cubic meter--1,000 per cubic meter.

They go through extensive vertical migrations. They are feeding in the water column at night-time, transporting to the bottom, excreting waste down near the bottom. They are fed upon by your flounders and other organisms on the bottom. That could be a very significant mechanism of vertical transport down, too.

There is one other thing. These krill seem to be--we don't have a lot of data on this, but these krill seem to be mostly concentrated in these submarine canyon environments.

DR. BUTMAN: Do they eat sediments? Are they filter feeders?

DR. TEAL: They filter stuff out of the water.

DR. COOPER: They are feeding on particulate that, to some extent, probably scavenge some of these pollutants in the water column. This is a biological mechanism of vertical transport to the ocean floor.

DR. BUTMAN: What is--

DR. TEAL: It is the same one we were talking about before, but with the point here that the *Meganyctiphanes norvegicus* are concentrated in the canyons.

DR. HECKER: They are vertical migrators.

DR. COOPER: My reason for bringing it up is that they are concentrated primarily--they appear to be--in these canyon environments.

DR. BUTMAN: We just ran out of space underneath that, so we are going to end the discussion on this.

DR. BOTHNER: Can I just make one more point about that?

DR. BUTMAN: As long as you don't add something to the list.

DR. BOTHNER: No, I'm not going to add anything to the list, but I am going to support Dick's column because, in some of our sediment traps, as you recall, we found shrimp-like creatures that were somewhat decomposed and, therefore, not easily identified. In addition, we found absolute layers of fecal pellets.

DR. TEAL: That's where it's from.

DR. BOTHNER: Maybe.

DR. BUTMAN: This is an important list. I didn't mean to cut it off. Are there any other things that are special about canyons that make sediment/contaminant accumulation important?

DR. HECKER: My instincts are there is more biomass in the canyons because the filter feeders are added on top of the background fauna, so what you've got is--the addition is, I think, you've got higher biomass.

DR. TEAL: The filter feeders are just important because they are there and they are generally absent from the other slope environments, are they not?

DR. HECKER: Yes, they are absent from the slope environments to a large extent, but what I am saying is: Several of the common slope species are concentrated in canyons, say, *Ophiomusium lymani* is about twice as abundant in the canyon axis itself, deeper in the canyon axis itself, than out on the slope. The deep-sea eel is more common in the canyon.

The only thing I found that really isn't is the red crab and there, it is hard for me to tell what the difference is between--they do red crab fishing on the slope and they don't in the canyon and, depending on when red crab lines have been along or not, I have problems with that.

But, in general, I would say you have got increased biomass in general even though the scavengers in the canyon--

DR. TEAL: The important point there is that you've got filter feeders and you can say there are more of them, but I mean it's a lot more.

DR. HECKER: I am just saying there is a higher biomass in canyons. If you want to think in terms of gluten accumulation or how much biomass is there.

DR. TEAL: But the important accumulators are the filter feeders.

DR. HECKER: Yes.

DR. TEAL: The important thing is that there are a lot more filter feeders in the canyons, rather than there are more infauna.

DR. HECKER: I was going to go through a whole list--diverse biomass, diversity. I mean, he is getting all this time for all that physical stuff.

DR. COOPER: You people really need to keep some perspective here. The physical stuff doesn't amount to a goddamn hill of beans as it only relates to the biology.

DR. BUTMAN: Right. You're supposed to chair the afternoon session.

DR. AURAND: May I make one request for the poor court reporter here? If you have thrown out scientific names, at the break, which should be coming in a couple of minutes, do her a big favor and walk past and tell her how to spell them.

We have been through this with the scientific committee and they have pretty much learned. They say "worms," "crabs." But if you use scientific names, she needs to get the information, because there is no way in the world that they can spell this kind of stuff.

DR. BUTMAN: I just have two more.

DR. VALENTINE: Could I make just one more comment about the biomass?

DR. BUTMAN: Sure.

DR. VALENTINE: I think we have to determine what we mean. Are you just talking about benthic organisms, because all the schooling fishes are up on the shelf, I mean, a tremendous amount of biomass on Georges Bank. If you compared it to the canyons, it might be a lot more on the shelf than it is on the canyons.

But, if you are talking about benthic organisms--

DR. HECKER: I am talking about epifaunal benthic stuff now. That's all I'm talking about.

DR. VALENTINE: We don't want to make some statement that will be obviously incorrect because it is not qualified.

DR. BUTMAN: The second to the last one is--here we go with "unique" again. I wonder if there are other characteristics of canyons that make conclusions from previous studies of OCS effects of drilling inapplicable or not appropriate and--I'm really getting in trouble with this one.

The two things--there may be many things here, but the one thing which is very different in canyons from the open shelf is this confined extent. The canyon is only 3 kilometers wide or so.

We have heard a lot about the effects of drilling. At least identifiable barium is only maybe one or two kilometers from a drilling rig. In a canyon, if you go 1 or 2 kilometers, if you actually drill in the axis of the canyon, 1 or 2 kilometers from the drilling rig is the whole canyon or at least a much larger percentage of that area than it would be, of that environment, than it is, say, on the open flank.

In addition, the reason why or at least one reason why we only see barite within a few kilometers of a rig on the shelf is that materials carried horizontally is accumulating farther away but we just can't see those concentrations.

In a canyon, it can't get carried that far away because there are canyon walls and so, I think it is a reasonable hypothesis to say at least initially--especially if you are shunting material from--not depositing it directly at the surface, that initially all of the drilling muds and cuttings will be in the canyon axis within that radius.

What happens to it after that is another question, but there is not that initial dispersal mechanism which is operating on the shelf in the canyon. I just called that confined extent.

Second, it seemed like from Barbara's presentation that there are a number of special fauna in the canyon. I don't know what data there is on toxicity studies on those species versus the species on the shelf.

Those are the two things that occurred to me and there may be others, but at least the confined area extent is one thing about the physical modeling of where material is going. It is the biggest difference. It is going to make it tough to generalize from previous studies.

DR. RAY: In the discussions, you know, I think we ought to look at scenarios. One is a scenario where if your initial discharge was into the confines of the canyon, that is one case like you are talking about.

The other one I think needs to be a part of the discussion is a discharge at some distance away from the initial axis of a canyon, in other words, where the material would have to come across the shelf before entering that canyon.

In fact, in the future, even if we could drill in a canyon, there may be no drilling, per se, in the canyon.

DR. BUTMAN: In the axis.

DR. RAY: The question is--and the regulatory agency is faced with a decision to make by all these stipulations. The stipulation is: How close do we allow discharge? I think that's a more practical, real question, although we should address both, than just the question of whether or not we dump it right in the middle of the axis of the canyon. I think we need to consider both later today when we have our discussions. I think they are both important.

DR. BUTMAN: I agree. It also makes a big difference whether you dilute it and discharge it at the surface, even if you are discharging right at the canyon, whether you discharge it at the surface or shunt it to the bottom. It will make a big difference how you do that.

But my idea here was more, for example, are the results of the Georges Bank monitoring program applicable to canyons? Is there something special about those fauna that would make the "no effect" that you saw there applicable or different in the canyon.

DR. NEFF: From what we have heard, I think there is substantial evidence that these fauna are fairly well adapted to high suspended sediment points. There is a fair amount of suspended particles in the canyons naturally.

DR. BUTMAN: So, they might be unique fauna, but they also might be uniquely adapted--

DR. NEFF: To suspended sediments. Now, obviously, if you dump several tons on them, you're going to destroy them, but that's a different story altogether.

DR. COOPER: Dick Cooper. We haven't talked anything about temperature and I bring this up because it is common knowledge amongst off-shore lobster fishermen fishing this time of year, the coldest time of the year there is, in the heads of these canyons, that the lobster catches are very high out there.

It is common knowledge in their ways of thinking from what few measurements they have made that the bottom water temperatures in the heads of these canyons are 1, 2, 3 degrees higher, warmer, than in adjacent shelfs. That's why the lobsters of these canyons are active, feeding, shedding, molting, mating and trapping right through the coldest part of the year.

Do we know anything about the temperature regimes of these canyonheads? I've seen some data on this in past years either from USGS or from our fisheries and I can't remember what the source was.

DR. BUTMAN: All the measurements we've made have temperature. We haven't tried to directly compare the average water temperature in the canyon and the water temperature on the slope. I think that's a good point, that we need to have some good working hypotheses about it.

Barbara and I were talking about this last night. What are the mechanisms for higher biomass or higher diversity? Is it temperature? Is it suspended matter? What are the actual mechanisms that cause it to be higher temperatures? Temperature is another good variable to look at.

DR. TEAL: Not for higher biomass. For periodic activity and things like that or trapping success, I wouldn't buy it just because the temperature is higher there than it is next door.

DR. HECKER: No, we were talking about the patchiness with regard to the high degree of faunal patchiness. Some of it I can relate to geology; other things I cannot and I really strongly feel it is related to physical processes.

On Baltimore Canyon, the west wall, specific areas of the west wall correlate with where lenses of fine material are coming out. It was that sort of thing that Brad and I were talking about with regard to Lydonia.

DR. COOPER: Are you likely to have vertical turbulence in these canyonheads that would mix superficial water at depth and cause this increase in temperature?

DR. BUTMAN: We looked at that a little bit in Lydonia Canyon and there was the suggestion that the vertical mixing compared--if you look at the TS properties of the water and you try to look at over what water depth, over what thicknesses the water is actually vertically well mixed, it looks like it is more well mixed in the canyonheads than at comparable depths, I would say the mouth of the canyon.

There is actually pretty good evidence for vertical stirring, which goes in, you know, which matches the resuspension ideas, also, that those are much more energetic and that you do get enhanced vertical mixing.

The proportions of that, you know, how much shelf water you actually mix with slope water, we don't know, but there is some evidence which says there is initial mixing.

DR. COOPER: These fishermen are fishing and making their big catches winter after winter down to depths of about 200 meters, so it is fairly shallow.

DR. BUTMAN: The problem is that the 200 meters is the basic shelf/slope water front, also, at 150 to 200 meters. The biggest thing is just the front moving back and forth. Sometimes it hits the bottom at 100 meters, sometimes it is 200 meters.

What you really see is a zone of large variability of temperature. I'd say that is the distinguishing characteristic, not the mean temperature, because sometimes it's in shelf water and sometimes it's in slope water. That 200 meters, the zone between 100 and 200 is really where the toe of that front always hits, plus or minus.

DR. NEFF: Certainly, in the Georges Bank area, the bottom water temperature there is much less variable than the higher water on the bank.

DR. BUTMAN: Right.

DR. NEFF: Station 15 was the most variable, the lowest and the highest temperatures. The deeper you went, the less variable it was. Likely, the heads of the canyons are winter refuges for a lot of animals who know that lobsters make major migrations seasonally and quite possibly congregate there because it's a little warmer and nicer in the wintertime.

DR. BUTMAN: Before we have a coffee break, let me just put the last one up here. We had a lot of discussion about sedimentary environments of canyons are similar or not similar.

I was obviously getting tired this morning when I was writing this out, so this is not quite as detailed, but based both on the texture and on currents--and, again, the primary data is in Oceanographer and Lydonia, although, Page, I think we can expand that to a much wider range of canyons.

Really, they are not similar. It is very difficult to say all canyons are erosional, all canyons are depositional. I think, from the data base that we discussed yesterday, we really have two canyons that we know something about in detail, Lydonia and Oceanographer.

The last thing I had was just a list of the limitations of the available data. Do you want to do that now or do you want to take a short break?

DR. HECKER: Let's take a break.

DR. BUTMAN: All right.

(A brief recess was taken.)

DR. BUTMAN: The final thing I tried to do was list a few of the limitations of available data and we have actually covered a number of these already in the discussion this morning.

I thought it was important to try to list what we do know, what we did know and what we do know. This is a partial listing of things which we don't know and some sort of qualifications to things that we know.

First, we have made the point already this morning that much of the process work has been confined to depths really in the heads of the canyons, 500 to 1,000 meters and really only one or two canyons. I think that's probably all right because the focus here is for canyonheads.

In terms of a large-scale box model and a bigger picture of the canyons, I think that is an important limitation we have to keep in mind.

We talked also about the stripping of pollutants from the water column as indicated by the lead-210 observations. It seems like what that efficiency is is an important question which we don't know about.

Third, we have talked a lot about resuspension and accumulation and transport in a few places. I suggested this hypothesis of a leaky system where there is some local accumulation but there may be some transport of material. Even though there is some accumulation, there is also some transport out of the system.

Actually, John brought this up yesterday in our discussion, that the rates is really a critical issue. We have a good idea about some of the processes but not necessarily some of the rates. I think the rates are going to be critical in trying to assess what some of the implications are for drilling.

We also talked this morning about hypotheses for the species abundance and diversity that are actually seen in the canyon. Joe and Barbara talked about variable substrates. We talked about limited exploitation in terms of direct fishing or dragging. We had a little bit of a discussion about temperature, but there may be others.

I think in terms of predicting effects of drilling, additional hypotheses for why the species are the way they are in the canyons are probably needed. Again, we don't have--a lot of our discussion is based on measurements within the axis, especially the geochemical measurements.

As I tried to point out in the very beginning, the walls are a large area of the canyon and we just don't have very many measurements of the geochemistry or accumulation rates or any direct measurements of accumulation rates or stripping along the walls.

In fact, from our physical measurements, the bottom currents--I probably didn't say this in my talk. The currents are bottom-intensified in the canyon and are much weaker above the bottom and, by inference, also on the walls.

This stripping may only be occurring, or this heavy resuspension may only be occurring or primarily may be occurring in the axis. Again, Page brought up this morning that we don't have many textural measurements deeper than 1,000 meters to try to look at the ultimate fate of where materials are going in the canyon.

During the break, the rapporteur also suggested we might think of this kind of box model of the canyon that we have. These arrows sort of indicate exchange. We have the upper canyon, which we have been predominantly talking about; the lower canyon, which we haven't talked about very much and then the slope and rise.

In terms of rates for sediment transport or exchange, we really want to try to quantify. We've talked this morning, saying that this arrow definitely goes from the shelf to the upper canyon. We haven't talked very much about--and these are really question marks--what is the flux of the material from the upper canyon to the lower canyon, from the lower canyon to the slope.

Actually, I guess the mechanism we discussed in Baltimore Canyon is a hypothesis of transport from the upper canyon to the slope and rise, also, for the suspended matter transport out in the mid-water column.

So, that was my list from yesterday of questions of the data that we don't really have data to have, or those are problems with the existing data. I think when we discuss the hypotheses, it will come out even more strongly what we don't have. Are there additions to that list or any discussion on those?

DR. VALENTINE: I'd like to limit that 1,000 meters to about 750 meters.

DR. BUTMAN: Okay.

DR. VALENTINE: This one here (indicating).

DR. BUTMAN: Mike, you actually talked about additional measurements of radioisotopes in other canyons.

DR. BOTHMAN: There is a tremendous lack of information there.

DR. AYERS: I missed yesterday. Could someone elaborate on the stripping and the scavenging mechanism that we are talking about? Is that yours? Could you tell me more about what you mean by that?

DR. BOTHNER: It would amount to a 10-minute talk. In 25 words or less, the lead-210 and the plutonium that we found in box cores from Lydonia Canyon axis compared to the continental slope is a much higher inventory and a higher specific activity in the canyon axis.

Since both those isotopes are considered to be models for sediment reactive contaminants in seawater, we find these higher inventories and activities from a non-core source suggest there are mechanisms for concentrating those isotopes.

The resuspension that we find in the canyon axis is the likely mechanism to account for their higher inventories and activities.

DR. AYERS: Did you find them throughout the canyon or where did you find the higher levels, the higher concentrations, along the axis of the canyon?

DR. BOTHNER: Yes. We've got one core at 630 meters in the canyon axis compared to one core at 630 meters from the slope. There are additional cores that came from what I call the deep water monitoring program on the slope and rise in which these isotopes can be measured, and should be, but have not yet been done.

DR. AYERS: Okay.

DR. NEFF: Are there other tracers for organic flux in the sediments, especially protective of organic matter, because that is a major mechanism for stripping solubles out of the water, absorption onto organic particles. Usually these are organic coatings on clays or some things like that. I wonder if there are any tracers for that.

Are they mainly associated with the iron and manganese oxides or what phase is the lead associated with, the lead-210?

DR. BOTHNER: From the evidence that Roy Carpenter has generated from the canyons on the west coast, the iron manganese oxides are, in fact, the most probable surface that is responsible for bonding the lead-210 into the sediments.

DR. NEFF: That's true for the cold lead. I wasn't sure for the hot lead.

DR. BOTHNER: It's true for the hot lead, as you phrase it, yes.

DR. NEFF: It's chemically the same stuff.

DR. BOTHNER: I know, I know, but we are using these model metals.

DR. AYERS: Do we have any other data for--I mean I can see why you are saying that, but do you feel like--is there other data to support that maybe it's just--do we have enough to where you feel real comfortable with that, or do you feel like--I'm sure you would like to have more data, but how much of that other data exists?

DR. BOTHNER: I'm not aware of a lot of data. Well, let's see, the west coast information using lead-210 in the canyons versus slope confirms very much what we find here on the east coast.

DR. AYERS: Okay.

DR. BOTHNER: Actually, since their data was first, I think I confirmed them, versus the other way around.

DR. BUTMAN: On the northern canyons of the west coast, there are sort of fine-grained canyons, not the canyons off southern California?

DR. BOTHNER: Actually, the data does deal with the canyons off Washington and Oregon and they are even different from the ones off Lydonia in that they do not have tremendous resuspension at the bottom.

There, the accumulation is thought to come from a mid-water layer which kinds of drapes off the continental shelf. Again, there are differences that make generalizations from canyon system to canyon system difficult.

DR. AURAND: Did we finish the preceding item, which was whether or not existing data was applicable or are we back to that?

DR. BUTMAN: That's what we were talking about with the sedimentary environments.

DR. AURAND: The one before that, the one where we had no--we came to no conclusion.

DR. BUTMAN: You should lead the discussion on that one.

DR. AURAND: From MMS' point of view, certainly, while the canyons, because of their structure and all of that, are areas where you want to make sure you have a good interpretation of what is going on, I don't think there is anything in what we have heard that would indicate that the results are not applicable in terms of what kind of sediment accumulation you would have around rigs or how far out things would go or what kind of effects they would have.

I think they would be applicable and I would push the fact that there is a confined area extent to where there are--I almost said "unique"--special fauna does not indicate to me that you cannot use the results from other monitoring studies to infer what would occur in a canyonhead or near a canyonhead environment.

In fact, in the Toms Canyon case, it was near a canyonhead, so certainly that information should be applicable, so I would say that there is no evidence that the previous studies are not applicable. I think we need to come to some kind of conclusion on that.

DR. NEFF: There are some ongoing studies, at least on the west coast, on hard-bottom communities which are similar. They are not the same biologically, but they are similar in terms of ecological niches, to some extent.

DR. HECKER: But do you have as much trapping in those areas?

DR. NEFF: No, it's a different situation. I am just talking about the interaction between the critters.

DR. AYERS: "Inapplicable" is too strong a word; that's the problem. You have to think about. There are some uncertainties.

DR. BUTMAN: That's a bad word. I didn't know quite how to word it.

DR. TEAL: One extreme you're saying is that you can't generalize science. Results from one place never apply 1 foot over. We certainly don't agree with that, because we'd all be out of a job. Well, we wouldn't be here if we felt that.

It is a matter of degree, to the extent to which the things that you have written down there--I mean, it seems to me the extent to which we have to not just import the conclusion from a study done somewhere else, but take the information about the processes and the dilution and so forth and apply it to the special situation of canyonheads. Then, I think you can do that.

DR. BUTMAN: What I was really trying to think of, Don, is that--and I was thinking of it primarily from a physical side. The one thing I was thinking was that the zone of influence has always been taken as 1 or 2 kilometers. I think that that--

DR. AURAND: I'd like to change it. I think that is more reasonable.

DR. BUTMAN: Taking into account what Jim said this morning, I think that depending on where materials are discharged, that's been sort of a very basic tenet of all the studies we have heard. You never see anything past 1 kilometer in terms of chemistry.

I think that that is something we need to examine in the canyon system and just not say that it is--

DR. AURAND: You need to examine that conclusion in the light of the conditions which exist in this area and see what it means, but the fact that all of the previous studies have shown that zone is an important piece of information and it should not be rendered inapplicable.

DR. BUTMAN: Okay.

DR. TEAL: I personally feel we don't want that in there at all. I mean, of course, we use information from previous science and we apply it to the special situation and we all take that for granted.

We don't have to justify doing that and we don't have to justify the fact that, since this is a different sort of a physical and biological situation that the conclusions are going to be somewhat different.

DR. BUTMAN: I like that approach. Let's just take this out of here.

DR. BOTHNER: Brad, I am tempted to say one other thing to you. You just said that sometimes the chemical effects are limited to a kilometer or two.

In fact, we can measure some signals as much as 60 kilometers away from the drilling region on Georges Bank. It just depends on how hard you want to look at it. There is nobody I think that would make the case that--well, it is difficult to predict any effects, certainly, beyond a few kilometers.

The fact that you can find the chemistry a good deal farther than that has been documented.

DR. BUTMAN: Okay. I wanted to somehow convey--maybe it is the special characteristics of the canyon, but this is the first case where we have talked about a confined area. All the other studies have been done--on discharges--for the open environments.

DR. TEAL: All the ones in the open ocean, but certainly that's not true elsewhere, is it? We've got lots of confined areas and made a big point of it in some cases.

DR. BUTMAN: That's true.

DR. TEAL: In produced water discharges in coastal lagoons, the whole point there is that you've got a confined area and you have to treat that differently than you treat produced water discharge along the open coast. I still think that it is a given that we look at this in a special way because it is a special environment.

DR. BUTMAN: I think instead of having a lot of discussion on this list of limitations, that might be the last thing we do. That's a proposed list, but it might be more important to flesh that out after we discuss the specific problem.

DR. AURAND: Certainly, from our point of view, if that were to lead to the list of limitations that that would usually lead to, we would want to make sure that it was restricted to those limitations which appear to have bearing on the ability of the group to draw conclusions and recommendations.

That list may be shorter than this list or it may include some things we haven't thought of yet, so I think that is an appropriate thing to do.

DR. BUTMAN: Good. Well, that concludes my list.

DR. AURAND: If I read the intent of all of this properly, we need to go on now to the biology of it. Did we have a volunteer?

DR. MACIOLEK: I volunteered Barbara.

DR. AURAND: We need to do that before lunch and then come back after lunch and start to try to put this together. Is that where we ended up? That's my understanding of where we ended up.

DR. HECKER: I am not as well organized as Brad.

DR. AURAND: Few of us appear to be.

BIOLOGICAL PROCESSES--DR. BARBARA HECKER, DISCUSSION CHAIR

DR. HECKER: Basically, I tried to do the same thing that Brad did with regard to some of the biological characteristics. What I have come up with is the biological characteristics of canyon populations.

Do they have a higher biomass for the megafauna? Yes. For the infauna? Yes. Higher diversity for the megafauna? Yes. For the infauna? No. Do we feel that we may have finely mediated pollutant concentrations due to feeding strategies for the megafauna? Yes. For the infauna? No.

Nancy said that they looked for filter feeders, differences in filter feeding, differences in surficial fine deposit feeders. They did not find those differences in the canyon stations.

Then the question is: Were the canyonheads nurseries for commercial populations? Yes. For commercial species? Yes. Were there higher concentrations of commercial species in the canyonheads? I think the answer is yes. Dick?

DR. COOPER: Absolutely. We can document that, too.

DR. HECKER: Does anybody feel anything else should be on this list?

DR. TEAL: All of this is in relation to the slope, not to the bank.

DR. HECKER: All of this is in relation to the slope.

DR. COOPER: Slope/outer shelf.

DR. MACIOLEK: That's true of, say, 150 meters which would be the edge of the shelf. Barbara--

DR. AURAND: That would be an important statement to include when you get into the writing of this--relative to what?

DR. BUTMAN: Is that true of all canyons?

DR. MACIOLEK: I only have information on Lydonia.

DR. HECKER: Gil Rowe did some work in Hudson Canyon and I think he found higher concentrations but lower diversity. Also, there is some work on Hudson Canyon. I would have to check the paper, but it looks like along the whole axis is higher biomass, higher density but lower diversity.

DR. RAY: Barbara, Jim Ray. In your statement up there about finely mediated pollutant concentrators, what types of filter feeders are you putting into the category, all of them? Are you figuring all of those corals and everything else?

Is there any data to show that they are actually filters or is that just a hypothesis that because they are filter feeders, they are concentrating pollutants?

DR. HECKER: Okay, it's a hypothesis. I don't know of any measurement done on it, except that the filter feeders do fine particles and, okay, Mike has said that these fine particles do tend to contain higher concentrations of materials. It is an inference.

DR. RAY: In some filter feeders, some of the mollusks and stuff, there are a variety of mechanisms where the pollutants are retained. In other cases, you are filtering out of the water and then passing and excreting stuff right away.

I am familiar with it in the mollusks but I haven't seen anything in the corals and gorgonians to suggest that just because it is a filter feeding strategy, that they, in fact, are pollutant concentrators. That is why I raise--

DR. NEFF: I don't think there is any solid evidence.

DR. RAY: That's why I was wondering what the background of that statement was.

DR. TEAL: The idea was not entirely that they were concentrating within their bodies, in any case, but that it was a way of taking it out of the water, fine particles out of the water, and then depositing them rather than accumulating them.

DR. RAY: That's true with mollusks. I am not sure with some others.

DR. TEAL: In any case, the hypothesis was not just that they were concentrating within their bodies.

DR. NEFF: There is one small point I always get annoyed about, but the word "pollutant" has a very specific meaning and it is not correct in this context. A pollutant is a contaminant that is causing biological impact.

Whereas, what we are talking about is contaminants. We don't know that there is a biological impact there yet. It is a conclusionary word as opposed to a functional word, so it is really "contaminant" concentrations. That makes it consistent with the previous thing, too.

It sounds picky, but people pick up on words like "pollutants."

DR. TEAL: It's important.

DR. MACIOLEK: Barbara, perhaps we need a line item there under biological characteristics to reflect different species' composition.

You mention with the epifauna that there were species in the canyons that you did not see on the slope, so you get different species on the canyons as compared to the slope.

DR. HECKER: You get the addition. In general, you have the same species that you see on the slope; you see them also in the canyon but you get the addition of another suite of fauna on top of that.

DR. MACIOLEK: So, you don't want to say you have--

DR. HECKER: I don't want to say you have replacement, per se.

DR. MACIOLEK: You don't want to say that the species composition is different in the canyons.

DR. HECKER: Composition, depending on how you want to define composition. Percentage, yes. You have a shift of percentages.

DR. MACIOLEK: That's the same thing that I saw for the infauna, as well, the same species, slope and canyon, but percentage representation was different.

DR. COOPER: Barbara, didn't you mention a species or two yesterday that were distinctive of the deeper portions of the canyons or did I misinterpret you, that were not part of the slope?

DR. HECKER: Oh, yes, deep in the canyon, yes. You've got various starfish, the, the filter-feeders, you've got a variety of corals down there.

The problem with deeper with regard to the way we analyzed our data, which would be with the community analysis, is that because the brittle star, is so common, that the contribution of the other organisms is just sort of masked, but they are there.

DR. COOPER: Aren't there several species, based on your work, primarily, that are distinctive only to the canyon environments? Is that a true statement or is that not true?

DR. HECKER: Several species distinctive only to the canyon environment? Have I never, ever seen those corals on the slope? No, I cannot say that. However, I may see one or two in a whole slope area and I will have seen 100s in the canyon.

DR. COOPER: Relatively speaking, distinctive to the canyons.

DR. HECKER: Relatively speaking, distinctive to the canyons. Most of those corals, I would say about 90 percent of those corals, are distinctive to canyons. There are only several sea pens that are not distinctive to canyons.

DR. COOPER: I have a very strong feeling here that we are not writing these short 100 words here for some peer view science journal; it is for the general public. I think we want to be careful not to nitpick the total accuracy or all fired concern about whether there are exceptions.

If we do these studies in another 10 or 20 years, we will find an exception to virtually everything we have said here in the last day and a half. If there are some species that are--that are what, that are--I'm not sure what the word to use is here.

DR. VALENTINE: Not the "U" word.

DR. COOPER: There are species that are relatively distinctive--

DR. HECKER: Yes. Can I qualify that even more? In the North Atlantic.

DR. COOPER: Primarily found within canyons. Then I think we ought to make that point.

DR. HECKER: Yes, in the North Atlantic. The Mid-Atlantic shows a different pattern, so if we want to put our qualifiers in, let's put all of them in.

DR. COOPER: The tile fish is a classic example. Virtually all of the tile fish populations that I have seen or heard anything about are strictly canyon populations, but there are one or two very small isolated fisheries, for whatever reason, on the outer shelf/upper slope environment where these tile fish occur.

But, that wouldn't stop me, just as one participant in this workshop, from making the statement that these tile fish are primarily unique to canyon environments.

DR. VALENTINE: Couldn't you say that these species are concentrated in the canyons? I mean, after all, if the upper slope at comparable depths had the same habitat diversity, they would be all over the upper slope, but they are not because the habitats are different, so they are concentrated in the canyons, although they can live on the upper slope if they find a boulder or something to attach to.

DR. NEFF: That's the obvious thing, the substrate availability.

DR. TEAL: Some of the corals, for example, are soft strip [words unclear] substrates.

DR. HECKER: Same with the sponges.

DR. TEAL: It's the same with the sponges. It seems to me it's fair to say if, for every 1,000 of them, 999 are in canyons, then you can say they live in the canyons.

DR. HECKER: I recall some species canyon indicators. That does not mean that they are nowhere else. It's just when you've got them, you've got a good indication--

DR. KRAEUTER: Would it be useful, under your megafauna, to separate those forms which are truly attached to corals and things, to require a hard or firm substrate as opposed to the things that you were describing, the fish, lobsters, crabs?

They can be substantially different in the way they go about things and stuff, so some of your--it's a splitting of the categories, to a certain extent, but when I think about fish, lobsters, crabs, mobile organisms, I think about them as substantially different in response to all sorts of things than those that are fixed.

DR. HECKER: Sessile.

DR. KRAEUTER: Sessile.

DR. HECKER: But do you want a substrate distinction, because the sessiles are not just a single substrate? How fine do you want it broken down? That is the question?

DR. KRAEUTER: I don't know. Up at the top, on the very top, you've got megafauna versus infauna. I was thinking megafauna really is two categories, that's what we are talking about here, the sessile and mobile.

DR. COOPER: The sessile and the mobile. A number of your major mobile species of commercial importance--

DR. TEAL: If you are making a table, it isn't useful to break it down so much that nobody can find any information on it.

DR. KRAEUTER: No.

DR. HECKER: I was trying to give an overview.

DR. TEAL: I think that's appropriate. In the paragraph when you write it up, you make the distinctions that you were just making.

DR. KRAEUTER: All right. Fine.

DR. NEFF: For which commercial species are the canyons nursery grounds? It might be good to give some examples.

DR. COOPER: Tile fish, lobsters, white hake.

DR. NEFF: To what extent for lobsters? When do they enter the canyons?

DR. COOPER: The canyon environments, the pueblo village communities specifically are the only off-shore areas where we've seen juvenile lobsters, 1 and 2 year olds, 3 year olds.

DR. MACIOLEK: How about goose fish, Dick?

DR. NEFF: They presumably migrate there.

DR. COOPER: No, there's shedding, mating and egg release right up around the rims in the upper portions of the canyon.

DR. NEFF: Yes, but then they are planktonic for a fairly extended period of time.

DR. COOPER: That's right. These lobsters are settling out over a wide range of areas. The ones that settle into the proper substrates of these canyonhead environments are the ones that survive.

DR. NEFF: We don't know if others migrate in.

DR. COOPER: We know a lot about the migratory behavior. The lobsters that we call off-shore deep water population lobsters migrate in shore either up on the Georges Bank or Cape Cod Bay, Long Island Sound or the southern coast of New Jersey for completed reproductive cycle, spring, early spring to mid-summer, and then they go back off shore.

Lobsters from the so-called endemic in-shore populations are quite distinctive. From all appearances, they do not migrate off shore.

DR. NEFF: I was just curious whether the lobster has to settle in the canyon to be a resident of the canyon, so to speak, or if the youngsters can migrate in at an early stage.

DR. COOPER: The answer to that would have to be no, they don't have to, because as extensive as this on-shore/off-shore migration is, so also is the along-the-outer-edge-of-the-shelf migration.

You get migrations in the course of a year or two of 100s of miles from Hudson Canyon up to Veach and Corsair down to Atlantis.

DR. TEAL: It isn't likely that a lobster that size is going to be able to wander around very much.

DR. COOPER: No, these lobsters don't begin their migratory behavior until they are about 65 or 70 millimeters.

DR. NEFF: In the case of the blue crab, that's when they do migrate.

DR. COOPER: Yes. The lobster population would no longer be, if they were out in the open at their small size.

DR. BUTMAN: I also wondered when you say both higher biomass and higher concentrations of commercial species, I wonder what that means in terms of the whole picture, because canyons are a small area.

If you took the low concentrations of the slope area times the slope area and high concentrations of the canyons times the small canyon areas, do you have roughly equal biomasses or does the canyon still have, in terms of the total biomass along the Georges Bank continental slope, of the total biomass, what percentage is the canyons?

You may not have the data to address that, but I have always wondered.

DR. HECKER: I would say that you still have--I mean, the slope accounts for probably the majority of the biomass, but I am talking on a per unit area.

DR. BUTMAN: In all species?

DR. HECKER: No, not all species.

DR. TEAL: What about commercial species? You said that canyon heads were an important nursery area. Now, is the slope also a nursery area for white hake?

DR. COOPER: No.

DR. TEAL: Not at all?

DR. COOPER: No. That also is true for the lobsters.

DR. TEAL: Okay.

DR. BRODY: Are the canyon stock necessary to stock other areas?

DR. COOPER: We believe very strongly that these submarine canyons, especially for lobsters, probably more so for lobsters than any other species, are very important nursery grounds. They are sort of home grounds for these lobsters to move laterally up and down the coast.

DR. TEAL: You are saying, also, for white hake that if the canyons, suppose that somebody did away with all the canyons, they all filled up and then the white hake population would drop down to--

DR. COOPER: I would think you would have a big drop, a tremendous drop, in white hake, certainly in lobsters, the so-called lobster population; your tile fish population would virtually disappear if that scenario were to--

DR. TEAL: Yes, I mean, that's a way of saying that, in your opinion or backed up by whatever data you know about.

DR. BUTMAN: Are there some commercial species which are only caught in canyons?

DR. COOPER: Tile fish, 99 percent, I would say.

DR. BUTMAN: The red crab fisheries and the lobster fisheries are all along the--

DR. COOPER: Lobsters and crabs are all along the outer shelf, upper slope.

DR. BUTMAN: Is it bigger in the canyons?

DR. COOPER: Bigger in the canyons for lobsters; certainly, of course, we are talking about a total area of these two physiographic regions that are very different. The red crab fishery is primarily a continental slope fishery, as I understand it. Lobsters are primarily a canyon fishery.

These two species, the lobster we believe is the highly dominant of these two and occupies these prime habitats down to about 1,200 feet and, at about that depth is where the red crab population takes over.

DR. VALENTINE: The red crab population might be limited more by temperature. I mean, they are a deeper water organism. The slope provides more of that area than the canyons do.

DR. COOPER: Who is to say whether the red crab, if you did away with your lobster population from 500 feet down to 1,200 feet, would the red crabs move down into that area? I have no idea. My suspicion is that they would. They are probably out there because they are out-competed by the much more aggressive lobster.

The lobster is not going down into the deeper range because of the temperature regime. They have a very strongly preferred temperature regime of about 9 to about 11 1/2 degrees.

DR. VALENTINE: But on the slope, you see, the red crabs don't go up to shallower depths on the slope where there are no lobsters. I mean, they are limited to--

DR. HECKER: Where there are fewer lobsters, I think the red crab may be competing with the jonah crab.

DR. COOPER: The lobster is a very cosmopolitan species. It occurs. You name a habitat and you will find lobsters there, almost. The lobster population extends all the way from at least Corsair Canyon down to almost Cape Hatteras outer shelf, upper slope environments.

DR. AURAND: So this paragraph is tough to write.

DR. HECKER: It is just you can put so many qualifiers in, but you can qualify any sentence into meaning nothing, then. I think we can make some statements and then you can always say that for these commercial fisheries, however, at least 1 out of every 999 tile fish is caught on the slope.

DR. COOPER: We can come up with as best anybody can come with today, a paragraph on that. I've got one paragraph already structured here for the special nature of submarine canyons.

DR. HECKER: Your next assignment, Dick.

DR. BRODY: I would like to ask one other question about the lobsters and nurseries. Do you think if lobsters were not fished in the canyons, the populations would go up elsewhere?

DR. COOPER: I think so. The canyons are very intensively fished. They have been for the last approximately 20 years plus.

DR. HECKER: No, not all species.

DR. COOPER: The average size of an off-shore lobster, say, 15 years ago was probably about 6 to 8 pounds with 30 or 40 pounders occasionally seen. The average size now of this off-shore population is just about what it is in the Gulf of Maine, 1 1/4 pounds, which is a very strong indication that it has been fished very intensively.

The one thing that the deep water population has going for it is that those lobsters, through their migratory behavior, which the in-shore lobsters do not perform, maintain themselves at a temperature regime always of about 9 degrees centigrade or warmer, so they are active; they are feeding; they are mating; they are shedding; they are growing throughout the year.

Their growth rate is almost twice that of the in-shore population, so they are growing. A lobster off-shore reaches sexual maturity at about the age of 3 or 4; for in-shore, it takes about 7 or 8 years. That says something about the size structure that you see off-shore.

DR. NEFF: That would imply we should probably be protecting those resources for our fishermen.

DR. COOPER: There are a lot of fishery managers, Jerry, that would say that is being done automatically through the economics of supply and demand.

DR. NEFF: The demand for lobsters is almost infinite, as long as you can ship them, people will buy them.

DR. BRODY: That is a real argument in favor of special exclusion for placing rigs along the canyonheads. A special exclusion could create a sanctuary.

DR. TEAL: As long as you don't drill there.

DR. NEFF: In fact, with any discharges from a rig, there is likely to be a substantial fishery impact.

DR. VALENTINE: Would it be appropriate in this action to have any list of why we think the canyons are harboring this special or unusual fauna? We have--we are defining the nature of the fauna in the canyons, but I do not think we have addressed or listed why it is different from comparable depths on the slope or the shelf.

DR. HECKER: We can do a hypothesis on that.

DR. COOPER: I think, quite clearly, this is a wide range of substrate or habitat and I am sure Barbara has mentioned several times, and I am sure she is right, but there are other reasons, as well.

If you are looking at the submarine canyonhead environments, where your commercial species primarily occur, you could say it is partly a result of a temperature regime, but the primary factor here of the numbers of species and the much higher biomass of about 8 or 10 different species that you saw on the videotape yesterday is heavily a function of these low relief three dimensional habitats that provide shelter or a compacted substrate that is conducive toward burrowing or excavation.

DR. VALENTINE: One of the reasons I bring that up is that, if that is true, it is the diverse habitats that cause this phenomenon to happen, then there are some canyons--most probably some canyons--which are called canyons which really don't fall into this category, some of these shallow--what I think of as (inaudible) of the slope.

I don't know if they have been set aside, things like Dog Body, Welker or some of these smaller canyons possibly don't meet the criteria that we have listed here, for biological diversity, biomass, heterogeneity, et cetera. They are really just (inaudible) of the slope.

If that is true, then they are really not part of the argument here. If we are not going to exclude areas of the slope for drilling because of the reasons we are discussing here today, then some of these canyons might fall into that category.

DR. HECKER: How would you characterize Heezen Canyon, Page?

DR. VALENTINE: Well, I've never been in Heezen Canyon, so I really don't know. From what you said yesterday, it is more like the canyons we are talking about now.

DR. HECKER: Deeper, because I only know the canyon deeper, but I think there is flushing going on. I had very well defined ripples at 1,600 meters.

DR. VALENTINE: But I'm talking about much smaller canyons.

DR. HECKER: Than even Heezen?

DR. VALENTINE: Oh, yes.

DR. HECKER: Heezen doesn't really indent the slope.

DR. VALENTINE: Indenting the shelf.

DR. HECKER: Indenting the shelf, yes.

DR. COOPER: File Bottom and Heel Tapper are really more like gulleys.

DR. VALENTINE: File Bottom does not invade the shelf bank, but there are 11 or 12 canyons, which I showed--Heel Tapper being one of them--which does, but it is really just slope environment.

If somebody is setting aside canyon areas and this is a named canyon and it is set aside, when really it is not like what we think of as canyons--Oceanographer, Lydonia, that sort of thing--we don't have enough information on it.

Nobody has studied these smaller ones to any extent that I know of, so we really don't know much about them.

DR. MACIOLEK: We have what we have the "gully/non-gully" comparison, but there weren't any names.

DR. VALENTINE: Those are probably not areas that are considered "canyons."

DR. MACIOLEK: No, they wouldn't be at all.

DR. MACIOLEK: Your point goes back to Brad's question about to how many canyons can we apply that characterization and it also comes back to Brad's list of limitations on the data because we haven't studied all of the major canyons.

DR. VALENTINE: I made one dive in this Heel Tapper Canyon for the purpose of comparing sediment texture and sedimentary processes and I just had one dive, so I went up the so-called axis of this canyon, which is really not very well defined. It looks just like the slope. That's just one observation.

So, I mean here is an area that we probably should address, I should think.

DR. COOPER: I think Page has got an excellent point here and we probably should have jumped on this sooner. But I think that we've had enough experience, even though some of these canyons may have received only several dives, you can tell a lot, especially in the smaller canyons from just two or three or four dives.

I think what we ought to do here, Barb, is name the canyons for which our description, we feel, applies. I would say that, certainly, Corsair and Lydonia and Oceanographer, Welker, Hydrographer, Veatch, Atlantis, Block, Hudson.

DR. HECKER: You will give me that list.

DR. VALENTINE: Corsair is not in American jurisdiction, so I don't think we have to consider that one.

DR. AURAND: Would it be easier to name the ones we don't think apply?

DR. COOPER: The list is not that long. Again, some of what I am saying here is based on one or two dives, but these are dives that were made in the axis and across the rims of canyons.

We know from those few dives, I would have no problems about putting them into this classification or characterization or not.

DR. BUTMAN: I think the very first thing we discussed this morning, when we made a list of what we think makes canyons special, and I think you just apply them when a particular area comes up. You apply those, whether there is resuspension or whatever.

I'm not sure we have to make a list of the canyons if we just made a list of the properties which you think are special to canyons, the characteristics you think are special to canyons. You can just apply that list to whatever area you want to apply that to.

DR. TEAL: You are the people here, sitting in this room, who have the ability to apply that list. It seems to me appropriate that you at least--you don't have to try to make something all inclusive, because you haven't been to them all, so you can't.

DR. BUTMAN: Sure.

DR. TEAL: But, at least, you could say that there are those canyons that this whole discussion applies to.

DR. BUTMAN: I agree.

DR. COOPER: That's appropriate.

DR. VALENTINE: Somebody reading this document in Washington will see the word "canyon" and he will look on the map and see all the canyons, and he will think that all of them fit.

DR. HECKER: I don't know which ones in the Georges Bank canyons this may apply to, but I do know that off New Jersey, say, something like Hendrickson Canyon looks exactly the same in the shallower depths and is very, very different from the slope deeper. It would depend on where we have looked in the canyon.

I would be uncomfortable with some of the classifications--some.

DR. COOPER: I think there has also been enough done in Norfolk Canyon, Barb, to be able to use those characterizations.

DR. HECKER: Oh, yes, Norfolk, no problem.

DR. KRAEUTER: Maybe what we need to do is go out and see where the fishing gear is to find out which ones meet some of these criteria by going out and looking for lobster traps.

DR. HECKER: Are you kidding? Whenever I'm out there, the fishermen ask me where to go. They go, "Where are the tile fish? Where is this? Where are the red crab traps?" I'm not kidding. The guy that runs the red crabs--

DR. COOPER: I know the difference between deep-sea lobster gear, tile-fish gear and sword fish.

DR. HECKER: Red crab traps are 5 miles long; that much I know, the lines. I've tangled enough of them to know.

All right. If we characterize the--let's say if we've now identified the populations that characterize canyons, I also think that there are quite a few questions left with regard to what are the biological properties of these organisms. I guess one of the things I want to raise with regard to--I usually think about megafauna.

Your canyon indicators are largely sessile. They are filter feeders. They have very, very restricted habitats, in a sense. The question I am raising here is: Let's just say that we are going to have an impact. Say, we have development in a canyon.

Let's just take it at faith. I want to sort of bring out a worst case scenario in terms of questioning some of this. If we want to take at face value that we will have an impact and say, we might wipe out a population in one canyon, what effect is that going to have over a longer term?

Now, I am bringing up the concept of stock populations. You don't have high enough densities of a lot of these animals out on the slope; you may or may not in the canyons, depending on which canyon you are talking about and which population.

I guess one of my concerns is the concept of a stock population, in that even though you may be wiping out only one one-thousandths of a continental margin area, are you wiping out 50 percent of a possible stock population?

These are questions I feel are hard to answer. I don't think we know enough biologically about these populations. The other thing is you are dealing with sessile populations. Why do we only find some of the soft substrate organisms in the canyons themselves?

Why don't we find them on the slope? Are they not settling there or are they settling there and not surviving? Here, my concern with regard to settling cues would be in the canyons. Are we going to change the physical characteristics of the substrate?

Are we going to change the chemical characteristics of the substrate? What is important in terms of settling, in terms of a settling cue, at least from shallow water, coral work, that I'm aware of--here, we are going way back--it would be the chemical cues are very, very important as are physical cues.

Then, there are also the cues with regard to whether there are adults there and chemical cues, also, saying, "Hey, this is a good place to settle down." I think that is a question that is not answered with regard to physical characteristics.

I do know that right in the immediate vicinity of the drill site, you are going to change the physical characteristics of the sea floor, the immediate vicinity. I don't know how far out that goes. I don't know what level of change may be acceptable or may not be acceptable.

Another question is: What concentration would alter -- you know, what concentration of a difference, what percent of difference would alter characteristics? I think John said to me 10 percent; right? A 10 percent increase in something may alter.

DR. COOPER: That's a pretty safe statement.

DR. TEAL: I think it very frequently takes something like a 10 percent change in physical or chemical characteristics to alter the environment.

DR. HECKER: Will you be able to give me a reference for that, John?

DR. TEAL: Since we don't know anything about these organisms and you don't know what their sensitivities are, I'm sort of talking off the top of my head. It may be nonsense, but if you said a 1 percent change, then you could be fairly certain that if you changed, for example, the concentration of barium in the sediment by one percent, you would be fairly certain that that wasn't going to have any effect on the organism. I think that's a fairly conservative statement.

DR. AYERS: You can't measure that.

DR. TEAL: That makes it even more conservative than I thought it was.

(Laughter)

DR. NEFF: There is the natural seasonal variability in things like sedimentation of the soil to twenty percent over the years.

DR. TEAL: Right.

DR. COOPER: Given a commercial-drilling operation in the head of Lydonia Canyon or on the rim or something, what is likely to be the biggest, most serious impact? Is it going to be the mercury, lead, and zinc that may get down into the lobster or shrimp populations?

Is it going to be suffocation 2 miles away from the rig? What are we talking about here?

DR. TEAL: It's not suffocation 2 miles away from the rig, no.

DR. AYERS: Can I just respond to that? I've done a lot of looking and I guess, in my experience, the impact you see is in the immediate vicinity of the well site. I'm saying it could range anywhere from 100 meters maybe to 7 or 8 hundred meters. They are burial effects, physical burial effects.

Jerry can certainly expand on this, because we have asked him to look at this a lot, but as far as any kind of metal uptake, significant amount of bio-accumulation, bio-magnification, to our knowledge, doesn't exist. We don't see any significant accumulation.

Most of the metals that are present in drilling discharges are highly unsoluble metals, and that includes barium sulfate because the sulfate ion is in the sea water. Even if you get some way to release the barium, because you have a sulfate ion there, you are still back immediately to the barium sulfate.

The other metals are generally sulfides, mercuric sulfide, cadmium; where there is some release from cadmium, more so than mercury, again, it is small. As a matter of fact, it is almost impossible to get any kind of mercury to dissolve.

I think the organic materials that are generated from the mud are at very low concentrations. You can't detect those in sediments. We are unable to detect those in sediments. Battelle did some work where they might be able to detect some, but again, the only tracer we've been able to use very efficiently at all is barium. That's because it is used in high concentrations.

You will discharge quantities of solids that you will discharge from a Mid-Atlantic well or a North Atlantic well like we're talking about here have been in the range of 2 to 3 thousand tons. It is predominantly--about one-half of that would be drill solids and one-half of that would be principally ferrite and clay, with small amounts of lignins, and that would be the principal ingredient.

Those are the kinds of things that we are looking at. I think it is highly important that we focus in a little bit on the quantities of materials that are discharged. It sounds like a lot, but when you consider them up against the natural sedimentation rate, these numbers are very, very small. It's almost immeasurable.

We were able to see, only measuring very close to the well site, we were able to get stuff caught in sediment traps. The highest levels we saw were maybe 10 percent of what was the natural sedimentation rate. When you got 3,000 meters or more away, it was down to less than 1 percent, even 0.1 of a percent, less than 10 percent in the canyon itself.

Then again, we are still looking at insoluble barium sulfate which is ubiquitous and is definitely not unique. It is everywhere. It is in the canyon sediments. It is in the shelf sediments. It's on the slope. It's everywhere.

Is barium sulfate a bad actor? Certainly not in our experience. I've never seen any indication that it is a problem. So, I have probably just given you a lot more information than you really asked for, but those are the kinds of things we do out there.

DR. COOPER: What this means to me is that any measurable impact to the commercial species, which are mostly mobile in the heads of these canyons, from growing activities, commercial growing activities, through suffocation or through contamination, direct injection into the food chain from various trace metals and so forth, is very unlikely, very small.

MS. HUGHES: Dick, are you talking about exploratory activities?

DR. COOPER: I'm talking about--

MS. HUGHES: All the way through to development and production?

DR. COOPER: Yes.

MS. HUGHES: Are you concerned about formation--

DR. COOPER: That was my next question. I'm looking at this in two or three phases, if you can bear with me for just a couple of minutes.

If there is some basis for my own gut feeling that that is the case there, now you go to, say, a large oil spill where you get a lot of hydrocarbons dumped in the water. In these high energy environments, what is the likelihood of this stuff getting down to the bottom at 530 feet?

DR. AYERS: I'd rather have somebody else address that. I think Jerry or--

DR. NEFF: We certainly know, in oil spill situations, that some of the petroleum does get to the bottom, mostly in shallow waters. We have been hunting for years for mechanisms for that and we can hypothesize various things like the fecal pellet production.

The mass transfer for that is not very efficient. We can't get an awful lot down that deep by any mechanism we know. I think there have been very few indications of sedimentation hydrocarbons in deep water. I don't know of any documented cases, but in shallow water at 10 to 20 meters, there is the absorption of suspended particulates and so forth.

In either this case or the oil spill case, you're going to have hydrocarbons in surface waters; how you get them down to where the critters are is a tough call. I don't know how you would do it. So, if there is a potential for a problem there, I don't see it.

DR. BUTMAN: The discussion this afternoon will try to finish the general characteristics of the canyon. It seems like we're getting off into the specific effects.

DR. AURAND: I think it is only relevant to the discussion of the thresholds. You need to probably color how much time you want to spend on the thresholds with some concept of what this means relative to thresholds. Other than that, I think you are probably right.

I guess what I am trying to say is it may not matter whether you pick 10 percent or 1 percent. Certainly, you don't have to argue over 8 percent or 9 percent, you know, for the thresholds, because we are not talking that kind of level of accuracy, if you will.

MR. LANE: The transport matter under discussion is important; whether you say it is going to be 10 percent, 1 percent or 90 percent, you will have to be able to define mechanisms for the transport of this material into close contact with the organism and something that keeps it in contact long enough for adverse effects.

DR. COOPER: I have just one follow-on. I probably agree with Brad, but bear with me for just one moment.

Most of your commercial species in the heads of these canyons are partially or highly mobile. Many of them, because of the temperature regime, are very fast growing and mature sexually at a very young age.

If the very worst were to happen and, say, the worst case scenario here was an oil spill where you had globs of oil in these pueblo village communities. Assuming that that form of pollution carried through and was washed through this area very fast, you may or may not have some short-term impact, but I suspect that those canyon heads, in terms of tile fish, lobsters, hakes and so forth, will probably bounce back fairly fast, in a matter of 2, 3 or 4 years.

DR. NEFF: Again, the critical question is: How the heck do you get the oil down there? I don't think you can get enough down there even under the worst scenario.

DR. TEAL: That's for this afternoon's discussion, really. I have an idea for a worst case that is different.

(Simultaneous discussion.)

DR. AURAND: Again, we can talk about some of the discussion that went on on Monday at the Fisheries Workshop, because this same topic came up in terms of modeling oil spills. There was little conclusion there of the mechanism.

MR. LANE: Formation waters or something you wanted to talk about, also?

DR. COOPER: I'll hold my peace for the moment.

DR. HECKER: Dick, here is a question for you, largely. Are the differences at different life stages and do we have to be worried about seasonal--I mean, what seasonal worries should we have with regard to discharges?

This goes with regard to, I think, the water column transfer, also, with regard to the water column transfer and with regard to the mobile fauna.

DR. COOPER: I would say that probably the worst time of the year for a discharge or any physical or chemical impact on these canyon environs would be the wintertime. In the summertime, a number of these species have moved out up on the Georges Bank and towards the coast.

DR. HECKER: Where are the concentrations of the materials the highest, or have they not been measured?

DR. COOPER: I don't know. The only time anybody has really studied them has been in July, August, and September. I don't know anything about the wintertime positions out there.

DR. HECKER: I guess the other question in here which, as is stated down there, is also: Are your commercial species like lobster and that more sensitive in the juvenile stages, since we call these areas nurseries?

DR. COOPER: Sensitive to--?

DR. HECKER: To anything? To anything that might happen with regard to them?

DR. COOPER: Anything that is going to force them out of their nursery shelters at a young age where they probably would be very quickly subjected to much higher rates of predation.

DR. NEFF: Certainly, the embryo-larval stages are more sensitive to chemical toxicity or whatever; the early juvenile stages in the cases of lobster are more sensitive to physical alteration of their environment. There is lots of documentation of that. If you change the substrate, they have more trouble digging their little burrows and things like that.

By the time the lobster gets to the juvenile, he is as tough as an adult as far as the chemical--

DR. COOPER: One thing that is very true of lobsters and it never ceases to amaze me here in the 30 years I've been studying this, larval lobsters, at the time of settling at the bottom as little miniaturized adults, stage IV, stage V, will thrive in the most filthy, polluted water you could possibly imagine--New York Harbor.

The lobster, in addition to being very cosmopolitan in terms of distribution of habitat, are very resilient to stressful situations.

DR. HECKER: Any information on the hake with regard to that or the tile fish?

DR. COOPER: I don't really know.

DR. NEFF: It is generally true of fish that once they pass into juveniles, they are fairly tough. Then you get into reproduction and so on.

DR. COOPER: One thing about tile fish, they apparently have the behavior when stressed, they dive into these grottoes and just hold on for dear life waiting for this unpleasant environment to pass. There have been some very large scale die-offs of tile fish because of warm winds coming in, in areas with a large jump in temperature.

The tile fish, unlike lobsters and hakes, will not emigrate from the area; instead, they stay there. That is a fairly characteristic part of their behavior.

DR. BUTMAN: I was just going to ask you something. If lobsters do well in polluted environments, do they do better in unpolluted environments?

DR. NEFF: By what standard?

DR. BUTMAN: You are implying that there is no impact of this on lobsters and--

DR. COOPER: No, no.

DR. BUTMAN: Okay.

DR. KRAEUTER: You also need to be very careful about what you say in terms of pollution and sewage pollution. If someone starts spreading pesticides in an area, you are going to have a much different response in a lobster. I don't know that you should eat one.

DR. COOPER: Protect the lobster, so no one will eat him. Pollute the lobster enough so that nobody will eat him, that's good for the lobsters.

(Laughter)

DR. HECKER: I guess what I have come up with, largely in what little we do know about some of these organisms, and that goes back to what Dick was trying to distinguish between, the sessile filter feeders and the more mobile fauna, is that we feel in commercial species, since they have a rapid growth and are mobile, you would expect short-term effects, if there were any.

DR. COOPER: You could kill off all your lobsters, for example, at the head of one canyon and, within a year or two, there would be a tremendous amount of immigration from adjacent canyons in that area.

DR. NEFF: That's very important, because if you had isolated populations, you could destroy it if they intermingle readily.

DR. HECKER: I'm worried with regard to the sessile species. My instinct is that it is going to be longer term. It may also be further reaching because of the distribution.

DR. NEFF: It really depends on their recruitment strategy. We don't even know if the recruiters living there now came from the population that's living there. They may be from somewhere else.

DR. AYERS: I'm not a biologist, but in the Mid-Atlantic study, the brittle stars are the ones that--you know, we came back one year later. We did a pre-drilling study and then one year later. The brittle stars are the ones that, within 90 meters of the well site, have not come back. They were still depressed.

The rest of the community is pretty well back to normal, but I think someone said that they had--one of the biologists said that they had a 7-year life span or something, and it apparently took them more time to grow and come back.

DR. HECKER: How many sessile filter feeders did you have there, you know, for a comparison?

DR. AYERS: I don't know. We had polychaetes, you know. I'm not talking about in a canyon. I'm talking about a drilling discharge study we did on a shelf in the Mid-Atlantic.

DR. HECKER: Yes.

DR. NEFF: It could have been a textural [word unclear] brittle star. They can migrate.

DR. AYERS: I think there is no question you are going to see some burial effects. There is just a lot of material and most of it settles out quickly. Most of it--90 percent of it--is going to be in the immediate vicinity of the well site. You are going to have piles of these.

DR. HECKER: What I'm talking about here is not just--I guess what I'm saying, again, as I started, worst case scenario, we are going to have an impact.

What I am asking largely is the question: If we wipe out a local population--I am not asking are we going to wipe out the local population; I am saying if we wipe out the local population--would we consider it to be short-term versus the long term?

I think for the reasons I mentioned the settling cues and the stock populations, I would be rather concerned about the sessile.

As we talk about the quote-unquote "unique" canyon--

DR. AYERS: Before we get away from that, when you say you are concerned about the ones in that immediate area, if you had something there that was highly significant--

DR. HECKER: If you wipe out the population in one canyon, are you just wiping out the population in that canyon or is that canyon serving as the stock population for a canyon further down the way?

I feel that there is transport out of the canyon and transport along the slope and possibly into the next canyon. Is this the way these organisms are moving around? I mean, what explains some of the patchiness of some of these organisms?

Is it concentrations by currents in a specific area in terms of recruitment that these conditions are also good for them? I think there are still too many unanswered questions as to exactly what is causing the faunal patchiness and how this translates in terms of their strategy. Here, I am talking about specifically sessile canyon populations.

DR. AYERS: How fast would they recover?

DR. HECKER: How fast would they recover, yes. My instincts are--and I don't think anybody here would argue with me--if we wiped out a population, it would have a longer term impact on non-commercial than on mobile species. Mobile species can walk out; these sessile ones cannot.

DR. AYERS: Don't we have some information in the literature on dredge sites or something where we've done some damage and looked at recovery rates?

DR. HECKER: The information is that the highly mobile fauna recovers faster.

DR. AYERS: 10 years? 5 years? 2 years?

DR. HECKER: I will say it all depends on the depth and it depends on the lifestyle of the animal. I mean, I can tell you about some infauna at 3,000 meters in the Bahamas that after 12 years had not recovered.

DR. AYERS: The deeper it is, the longer it takes.

DR. HECKER: Generally. Unless you hit a petroleum well and you get seepage, and you get chemosynthetic organisms moving in, okay, in general, I would say things move slow, and that is in general. There are some people that will argue, but in general, things move slow.

I think, also, another "in general" is the deeper you go, the more sensitive the organisms--maybe, and that is very "in general."

DR. KRAEUTER: But that may not be true. One thing I keep puzzling about, and we haven't really touched on it here. We are talking around it. Obviously, we have a high concentration of filter feeders in these that we don't have on a slope. That implies, obviously, there is something going on with the food, to me, as opposed to recruitment mechanisms and things like that.

DR. HECKER: What I am saying is: Aren't the two together, because the currents that are bringing the food in may also be bringing the larvae in.

DR. KRAEUTER: But it also implies that with the higher density of the infauna and everything else, that there is a greater source of organic material coming into these areas from somewhere, or some reason that is causing this. That may be more important than the other things.

I'm just trying to make analogy to the shallow waters that we know, and when you get these concentrations, it is often of some import, for some reason that's causing this. That may be more important than looking at the patchiness of distribution. We could go down the list and spend all day making up reasons for patchy distributions and probably never get there.

Again, the rate of import of organic material, the concentration of it, or something may be very important and that gets back to mechanisms and rates again. We've got a greater biomass to support it, and there has got to be a good reason for that.

DR. HECKER: The patchiness was with regard to the concept of: If you are destroying this area, are you wiping out half of the stock population or are you wiping out one 1/1000 of the stock population?

DR. NEFF: You can't answer that.

DR. HECKER: I think you can answer it. I think you are wiping out a much higher percentage of the stock population by wiping out a population you can't eat, the sessile, a much higher percentage, even though it only covers sessile. The concept of the stock population is going to contribute to a long-term problem.

DR. COOPER: Barbara, Joe Uzman (phonetic) here several years back determined what percentage of the Georges Bank continental slope, upper continental slope was occupied by the so-called Georges Bank Canyon. I think he came up with an estimate of about 20 percent, which surprised me at the time.

I sat down with a chart and looked at it and it probably was pretty close. Of the upper continental slope, 20 percent in the area of Georges Bank are occupied by submarine canyons, which is a--

DR. TEAL: Not so unique after all.

DR. HECKER: Wait a minute. Wait a minute.

DR. COOPER: That was, in fact, a uniquely high percentage.

DR. HECKER: There is a question. Now, all canyons are not considered unique canyons. We have got to subtract from that 20 percent, take that into account, and then maybe not the whole canyon area. What did he consider the canyon area? I guess that's the question. From the rim down?

DR. COOPER: I think everything on the chart was called the canyon, whether they were bottom gulleys or not.

DR. BUTMAN: Was that lineal extent or just what actually intercepted the slope? I bet it is even more than that if you take the length of--

DR. COOPER: I'm not sure.

DR. TEAL: In any case, you are all agreeing that it occupies a substantial fraction of the entire upper slope.

DR. BUTMAN: If it didn't, we wouldn't need this meeting.

DR. TEAL: No, no, because this meeting could be about Flower Garden Reef in Texas, which is one lone spot on the coast.

DR. HECKER: Brad, if we only have one canyon out there, I doubt if we'd even consider the concept of drilling anywhere near it.

DR. BUTMAN: That's what I was saying.

DR. HECKER: Yes. I want to just raise a couple of other little things. John Teal told me that I was going to object.

Actually, the very first question that I wanted to raise is: What is an impact? Let me play the devil's advocate. An impact is anything that changes the existing condition to something else, if you want to consider that an impact. Do you want to consider an impact anything that alters the environment deleteriously? Then you get into the value judgment of what is deleterious.

DR. AYERS: I think it has to be measurable.

DR. HECKER: I have always felt the critters respond to cues whether we can measure them or not. They are responding to their own cues.

DR. TEAL: His question is still good. If you can't measure it, you say you can measure it in the response of the animal, and that is still a measure.

DR. HECKER: Yes. Okay. What constitutes an impact?

DR. NEFF: The key question is whether you consider an impact a chemical change or just a strict impact, a biological change? Historically, we have been able to use chemistry as our guide and say, "Okay, we've got a new chemical here. There is an impact."

Basically, I am asking you to define what you mean by impact. If we could detect an increment in barium sediment of a 10 percent increase, is that an impact?

DR. HECKER: Do we see a change in the animals and is that change in the animals significant from background noise?

DR. NEFF: That is what I was going to ask you.

MR. VILD: Aren't you really concerned with adversity here? An impact, if it is any change, it could be a change for the positive, like you said with your chemosynthetic organisms after an oil spill. For them, it's a beneficial impact. Maybe for nobody else, but for them, it is.

I think if we are talking about adversity, and I think that's where we are headed on this, because of--here we go into semantics again, but because of the way "impact" is normally defined, for better or for worse, we should have "adverse impact."

DR. TEAL: No, that's the thing that I disagree with. If we say "adverse"--

DR. HECKER: "Adverse" is a value judgment. It is a value judgment.

MR. VILD: Okay. Then, let's talk about everything. Let's talk about something that may prove to be beneficial as a result of all this.

DR. HECKER: Take, for instance, artificial reefs. You increase the--

MR. VILD: That's my point exactly. That is an impact. If we want to talk about that, fine, but if we want to restrict what we are talking about now to adversity, to negative impact, we have to have that qualifier in there.

DR. TEAL: I don't think we do.

DR. HECKER: The question is: What is a negative impact?

DR. BOTHNER: I'd rather see you use the words "biological impact."

DR. HECKER: That is why I am asking this. What do we consider constitutes an impact?

DR. COOPER: If I can interject here, I can tell you what a recent study panel considers biological impact as regards sewage sludge dumping off the Mid-Atlantic, as regards lobsters and red crabs, shell disease, a significant increase in shell disease caused by two different causative organisms; a lethargic non-active sort of state of recently caught lobsters; females carrying a noticeably low load of eggs at a time of year when they should be much more fully egged out.

DR. AYERS: What we are really doing is describing a measurable biological impact, a measurable biological change.

DR. COOPER: You are talking about some quasi-measurable, quasi-quantifiable aspect of this product that you are bringing up out of the ocean depths that is going to have a very serious impact on its salability and human concern for the quality of an edible product.

DR. NEFF: That is where you need chemistry to link the activity to the impact. We don't have that evidence of the impacts of dumping that is causing these problems.

DR. COOPER: You haven't seen the reports yet.

DR. NEFF: I've been involved in some of the work that has been out there.

DR. COOPER: Okay.

DR. KRAEUTER: Given that same scenario, then you could certainly look for lesions in tile fish.

DR. COOPER: Fin rot, lesions on tile fish, that's right.

DR. KRAEUTER: Yes.

DR. COOPER: We know, even though this has not been quantitatively assessed up to now, at least, in many of these so-called pristine areas, it is a rarity to go out there and catch a tile fish that has fin rot or lesions on it. It is a rarity to find a shell disease or black-gilled or poorly egged out female lobsters.

When those kinds of things start showing up, it is very evident to the commercial fishermen. They are the ones that first let out the cry of concern. It is a fairly easy-to-define impact, in quotes, if you get out there in time to get out there and measure the chemistry.

DR. VALENTINE: Couldn't we broadly define it as a reduction in productivity or usefulness of the organism?

DR. COOPER: That, Page, is extremely difficult. There are such tremendous fluctuations in catch rates from commercial fishermen, that your impact would have to be extreme and so obvious that a get together of people like us would all be academic.

DR. VALENTINE: If you say productivity and/or usefulness, I mean, they consider red crabs with these black lesions not useful; tile fish with lesions are not useful. They can't sell them.

DR. COOPER: They are not marketable.

DR. VALENTINE: Lobsters with high loads of copper or whatever are not useful.

MS. HUGHES: I think one of the difficulties with using what Dick has described as a measure of impact, if we are proposing it as a measure of impact, say, in activities around the submarine canyons, I mean, it is still inexact, but it appears as though things like shell disease and black gill and all are associated more with a much more long term and chronic problem--years versus months, or many years versus months--than what we might be talking about with regard to exploratory operations and--I don't know, it's possible--with regard to development and production, if it were to occur.

DR. HECKER: One of the reasons I was asking the question is: How are we going to define impact? Are we going to not allow any impact? I mean, is a 1 percent population reduction unacceptable? Is a 10 percent population reduction?

DR. COOPER: You can't even measure that.

DR. HECKER: Yes.

DR. AYERS: You can't even measure chemical changes, let alone biological changes.

DR. COOPER: I disagree with Pat here. I don't think this is this long term--fin rot, lesions, shell disease, black gill conditions are things that can easily set in in a matter of a number of months, up to a year, maybe a year and a half.

I don't think we are talking about long-term chronic build-ups that require a number of years to manifest themselves.

DR. NEFF: I don't think there is any rational, though, for assuming that these are the types of things you might see from a long-term development of drilling. I mean, it's a very different scenario. Using it is a good example, but let's not mislead people to think, "Well, gee, that may be happening in the canyons if they drill out there."

DR. COOPER: I have no idea whether those would be the effects from something like this.

DR. NEFF: It is common where you dump a lot of sewage sludge and that is the major linkage right now.

DR. COOPER: That was strictly an example of the kind of thing that--quality of product. The obvious thing is to try to measure a drop in catch. There are many other things that relate to that where you would never, in 100 years, show any cause-effect relationship unless it was a massive drop-off.

DR. VALENTINE: There are two points here. One is defining what we think is an impact. The second thing is: How much of it can we accept? I don't think we ought to mix the two.

DR. NEFF: I think we ought to first go out and see if we can detect a biological change that is clearly attributable to the activity, usually through the chemistry link. Then the next step is a judgment. What level of change of that kind is acceptable or unacceptable?

DR. COOPER: I gave some thought to this on my way coming up here the other day. To me, the only real, sure-fire way to demonstrate any impact, regardless of how you want to measure it, is to set up one or several site specific stations prior to a production platform going into effect on a small scale; it doesn't have to be a big program to do it.

Do it _in situ_ with this high tech diving technique we have, so you're going down and monitoring one given rock pile, several site-specific pueblo village communities where you can monitor individuals and populations in those specific environments.

DR. AURAND: We are doing that on the west coast. The drawbacks are, of course, in the soft bottom areas, you can't, but we are doing that in the hard bottom areas. Unless you get into a solution where that would be allowed so that you can actually monitor it, then you are kind of caught in a Catch-22, you know.

That's the information you actually want, but you only get the information once you actually drill.

DR. NEFF: You don't cause an impact until it's done.

DR. AURAND: I think reasonably, what you have to do today, you probably have to leave it, for now, with words like "a measurable response" and wait until you talk about what the mechanisms and the possible consequences are before you try to define what you would use as an impact agent.

I mean, if you pick one now, it might be that you would come back after lunch and say, "Well, there is no way we could do that." The whole discussion may be irrelevant until you start to talk about the mechanisms that could cause the impact.

We are probably better with Jerry's more general definition up until that point.

MR. LANE: Should we add the one that he just introduced, "directly attributable to OCS activities"?

DR. NEFF: We have to have some linkage there even if it's only circumstantial.

DR. AURAND: Presumably, if the discussion focuses on how the area would change, there would be a discussion of how OCS oil and gas activities could cause that and, at that point, we will define what that is, and then that would lead, perhaps, to some criteria.

In any effect, I think it is important to realize that we are trying to do that, including the submersibles, in looking at the same rocks on the west coast, but we aren't very far along with that now.

DR. NEFF: It's up to the drillers to drill and the permitters to permit.

DR. HECKER: Okay. Then I guess another thing with regard to determining impacts or something, Dick, your feelings with regard to commercial species is that it doesn't really matter if we've got just exploratory versus developmental drilling, right?

DR. COOPER: Perhaps. I don't really know the difference between the two. I know some obvious differences. I don't have a good feel for what the volume of output of cuttings is between an exploratory hole and a commercial hole. Obviously, a commercial hole is one that has the potential of an oil spill.

DR. NEFF: You've got cumulative chemical signals, physical signals, and that could cause cumulative impact.

DR. COOPER: So, the potential impact from a commercial platform is much greater than exploratory?

DR. NEFF: The potential for an impact, because there is more stuff discharged.

DR. HECKER: My concern is just for the sheer number, the number of wells allowed. We might be able to measure something 300 meters away, 3,000 meters away or something, and consider the small size of a canyon, put one well in there, okay, and I get nervous about it. If you tell me you're going to put four in there and--

DR. NEFF: They would all be on the same platform. It's one platform. They drill a hole bunch of wells.

DR. TEAL: One platform with one pipe coming down and a little pile of cuttings down there, that's exploratory; then, if it works, then you've got 60 pipes coming down and 60 piles of stuff, all in the same place but you've got 60 times as much stuff--well, probably not 60 times, but 40 times as much stuff.

DR. HECKER: All exactly in the same hole or different holes?

DR. TEAL: It's different holes, but all under this same platform. The going in different directions takes place after they are under the surface.

DR. AYERS: Just a comment on the development drilling for people who aren't familiar with it, it's multiple drilling from a single platform with a single discharge point where normally, you know, depending on the depth of a well, you may take anywhere from a month to a couple of months to drill a single well. It's just one after the other.

Your total drilling of an area from that single platform, the actual drilling and discharging may occur over a 4- or 5-year period, just depending on the number of wells, but that's the length of time the drilling and discharging would go on.

For different locations, I guess the most extensive one we've seen here is a 26-well discharge--discharges from 26 wells from a single platform. The one general comment I'll make based on what we've seen from the sediment chemistry is that you don't see 26 times as much stuff from the bottom as you would from one well.

It is not a directly linear increase. You do see--

DR. TEAL: I was thinking more of the volume of cuttings.

DR. AYERS: Obviously, the volume of cuttings would be large. The signal that you see from it, you know, you do see more barium and you can follow a gradient further with the other associated trace metals beyond barium. Again, that signal falls back off to background still fairly close in.

The barium signal, though, does stay elevated fairly far out to a couple thousand meters, but again, I bring it up to make a point that, you know, you don't see 26 times as much of everything out there just because you had 26 wells.

Anyway, that's a general scenario of a development drilling type of operation. The discharge volumes, over a period of time and on a daily basis, are the same as an exploration. The difference is you've got one well after another discharging in the same location.

We might drill something like five or six wells a year at that one spot and we'd be doing pretty good.

DR. RAY: Depending on the depth of the wells.

DR. VALENTINE: What happens after the platform is totally--after all the wells are drilled and it is in full production? Are there any different activities that cause pollution, like tanker--spillages from pipelines or tankers?

DR. RAY: Once the drilling phase is done, depending on how that platform is produced, you really have two options. One is to pipe to shore from platform; the other one is that you lighter off onto a tanker for transportation to shore from there.

A third scenario is the production ship, like Exxon did out in California but that's almost a one-up type of operation to do that, so most likely, you have a pipeline operation.

You have the possibility of pipeline spills if you had a pipeline rupture. The key thing from the platform, other than domestic discharge and deck drainage, which are two key ones in produced water. If the platform is a production platform that does the production work right there, then you have the discharge of produced water, which is usually done right at the surface or shunted just below the surface.

In some locations, the oil and produced water and everything is transported ashore. The production separation of the oil from the water is done on shore. Then sometimes, it is discharged back to the coastal zone; in some places in California, the produced water goes back off-shore for discharge; in other places, it's reinjected. There are a lot of different scenarios.

In the case of if it is a gas area, predominantly, which there is some thought that this particular area we are talking about might be more prone to gas than it is to oil, and I have heard that suggested, then you would have little or no produced water; whereas, with oil production, you tend to have most of your produced water.

DR. VALENTINE: Roughly, what percentage is water?

DR. RAY: It depends on where the formations are being produced from and the age of a well. Usually, 10 percent or less of the volume would be produced water, really early in the life of the well.

In some of the old wells that we've got down in the Gulf of Mexico, you can have 70 to 80 to 90 percent of the total volume produced will be water. It shifts during the life of a production of a field.

DR. AYERS: There is an economic limit on how much water you can take. The water/oil ratio is controlled by economics, crude price, what it costs to get rid of the water. If it gets too high, they stop.

DR. HECKER: Can we continue this after we've given this the last 15 minutes?

DR. AURAND: I think there is one thing you need to think about. If we are going to do the new arrangement that we talked about, something needs to get written down very quickly.
I think people need to decide who is going to right what down. Then we need to break for lunch and you need to come back and write something that everybody can see.

Then we need to go on to a discussion of what the mechanisms of impacts are and whether or not we can reach any conclusions relative to this topic. Otherwise, we are going to lose a lot of people--physically, not mentally, necessarily, but people will be leaving.

So, I think for both, both of you need to--before you let everybody out of this room--find out who is helping you write these various paragraphs.

DR. HECKER: Can we tell who is helping us?

DR. AURAND: Far be it from me to restrict you from doing that.

DR. HECKER: Dick, John, Nancy, Jim, if everybody writes a paragraph, I'll look over it. Fred? Fred is head of the biological session. I think we'll let him take over now. I've assembled your team for you.

DR. AURAND: Brad, you need to do the same thing, point people out and, no, MMS is not writing this.

DR. BUTMAN: I guess I don't see it is appropriate to write something about the discussion that we had.

DR. AURAND: No, you are going to write the introductory material. You are trying to write about this is what you think is special about the biological communities or the characteristics that we think are important, and the same thing for the physical environment.

The up-front introductory material, that's what you are trying to write.

DR. BUTMAN: These are the 100-word paragraphs.

DR. TEAL: The people who gave presentations yesterday also need to write their 100-word paragraphs.

DR. BOTHNER: Sometimes, they overlap, don't they? I mean, the topics that Brad listed is sort of an abstract of the abstracts, but it is the layman's summary of what we need to present in the introductory paragraphs.

DR. AURAND: I think it would be an excellent idea to put the charts back up and put the names next to the various sections.

DR. BUTMAN: Page, you write a paragraph on the topographic environments, the first thing I had on that list about why canyons are unique--sort of the topography and what is important about those.

I'll write a paragraph about the currents. What else was on there--topography, currents.

DR. HECKER: John, would you like to write anything on impacts and percentages we might accept, the level?

DR. BUTMAN: Page is going to write something on number one here.

DR. MILLER: I'd like to refer to your agenda. If you will go to--what you have done is jumped to the third day, which is tomorrow, what we were going to do tomorrow.

During the final day of the workshop, the panel members are to meet, prepare written drafts of their conclusions, focusing on the workshop hypotheses discussed during the Roundtable Session and to summarize their comments developed on issues considered during the panel sessions.

We have changed the agenda here, but that still does not relieve the responsibilities of accomplishing the end goal, which is to prepare this document, and that is what we are focusing on. I would strongly urge you to take your panel members, take the topics that you had outlined, assign responsibility for that material to be prepared and written, and get your drafts in.

The rapporteurs are here to help you do the typing on that. They have been taking notes, but it is your responsibility to put this together and to come to some conclusions and recommendations, as to what are the problems as you've been discussing here, or the options.

This is the way this is wired in. The narrative section will form a consolidated summary synthesis of the conclusions and recommendations of the scientific panel members. That is what we have been trying to do.

In essence, you have collapsed some of tomorrow's session into today's by trying to--

DR. HECKER: But then to ask us to turn it around in an hour, I think is--

DR. MILLER: This was one of the dangers of doing this.

DR. AURAND: That is also one of the dangers of accelerating the schedule.

DR. MILLER: That's right. That's exactly right.

DR. AURAND: If I was to read this as a decision maker, I would want to see something where you told me what canyons were like. I would want to see something where you said something about what it was you were analyzing, not in detail, just we're going to look at what is in cuttings, whatever.

I would want to have a discussion of what the impacts you thought might happen were, your conclusions about what those impacts might or might not do, any need that you identify through this process that restricted your ability to reach consensus on conclusions.

If there are any majority dissenting opinions from the majority, then they should be appended, as well. I think those are reasonable things to put in there. Whether you want to go right down that list and write the five things, and include it as a second part of this or whether you want to change that list or whatever, the question was: What do you expect to get out of it? I'd love to see that, personally. Jim?

MR. LANE: What we were really trying to accomplish, and we originally scheduled three days to do it, was to write the proceedings. That is a good outline of the proceedings we would expect to go into this. This is why we also brought so much support on site in terms of personnel and very highly qualified rapporteurs.

DR. AURAND: There is a rational for doing that, too, and then I think we need to break for lunch and decide when we are going to come back.

When you try to write the proceedings independently, we would send a copy of it to everybody and ultimately, we would not have the interchange over the edited document that you can have if you see something as a group that you can discuss. What we are looking for is the discussion and consensus, and that's the important part.

The only way we know how to do that is to put you all in a room with the people who are writing and let them produce it as fast as they can so that you can see where this is all going.

Is it reasonable to get back together at 2:00 o'clock?

DR. MILLER: At 2 o'clock, because some of them will be leaving.

DR. BUTMAN: I think all we have discussed that we could write a paragraph on is in terms of what we've done this morning. I think in terms of possible impacts, we haven't had that discussion yet.

DR. AURAND: I know we have not. This is in total, the whole thing. You should have some opinions on 1-A and 2, but we haven't done those yet and 2 and 3 are the real meat of this afternoon.

DR. MILLER: That's got to be written and documented, Don, before they leave.

DR. AURAND: If they are going to pull out if they have a plane reservation, that may or may not be so. That's all I can tell you. We really had scheduled 3 days to do this. We expected to reach about the bottom of number three some time late this afternoon and type it overnight and give it back to you tomorrow, not try to give it to you at 2:00 o'clock this afternoon.

But, because of the press for a number of people to leave, we are trying to accelerate that process. For those of you who are still here tomorrow, we can work it out. I mean, we are here until tomorrow. We can flesh out the document tomorrow.

MR. LANE: By doing that, we've lost some people who have to leave. What we think we have lost that we really would miss is the opportunity for group discussion and consensus on the recommendations and conclusions.

What would bother me the most is the fact that the next opportunity you will get to see this document is individually, in your separate offices. Of course, you can talk on the phone and such, but it does not replace a group meeting or a group consensus in producing the final document.

DR. AURAND: So, as much as you can get down on paper that you can look at before you separate, that much better off we are in terms of doing something with this.

DR. RAY: You mentioned this morning at about 11:00 o'clock or so that you were going to make an official decision as to whether or not you were going to cut it a day short or not. Have you made a decision?

DR. AURAND: I think we will try. What we ended up with was the fact that we would try to squeeze as much into today as we could possibly squeeze in. Those of us who are here tomorrow will try to finish up.

DR. VALENTINE: There will be plenty to discuss tomorrow.

DR. MILLER: Who will be here tomorrow? May I see a show of hands, please?

DR. AURAND: I don't think we should count MMS, by the way. Seven.

MR. LANE: You've lost the people who (inaudible) produce the volumes and materials or concentrations, someone with toxicology.

DR. AYERS: That is probably the principal contribution we will make.

DR. BOTHNER: Maybe I ought not to bother to write the characteristics immediately, but rather, take advantage of that expertise while you are here, and get that down onto the tape recorder, at the very least.

DR. AURAND: Let's start talking about how we--are there any contracting officers here--the way we modify this thing. We could send copies. We could arrange for the characteristics sections to be sent all around for everybody to look at.

We could, I suppose, follow through with that and, in cases where there were disagreements, work out some way to correspond with you to get rid of the disagreements if different people had different concepts. Then, we could just come back at 2:00 o'clock and start right on into the impacts, the discussion of the impacts, at least.

DR. MILLER: The only problem we are facing, Don, is the time frames on the contract.

DR. AURAND: What the government has put together, the government can change.

MR. LANE: To me, the greater loss is the opportunity for the group to agree and interact on these.

DR. AURAND: Is that a statement that we should adjourn and come back together again? I would be willing to pursue with the contracting office any changes in the structure that you thought were appropriate to facilitate the purpose of the meeting.

I can't tell you now that we would agree to anything or what would be accepted. I also think Pat has got the right idea, that there has got to be a limit to this or we could talk forever.

MS. HUGHES: I know I have upset a lot of people here by proposing to put an end to this and all of that sort of thing, but I appreciate it and I think it is valuable to put everything together. I guess I would say that there is nothing to stop everyone from continuing to discuss and get through this list.

The rapporteurs are working on paper. We are taping the whole thing verbatim. I think people have pretty good enough memories that the things we have talked about yesterday and this morning, right up until now, will continue in people's heads. Perhaps, we can get down to the conclusions today or perhaps not.

I guess I'm struggling with this. There seems to be this incredible difficulty here and I don't get it.

DR. AURAND: We wanted you to see the written conclusions. We wanted you to see, as a group, what the rapporteurs and the group drew for conclusions, so that you could decide whether or not that was, in fact, what you said, as a group, not as individuals.

MR. LANE: Let me ask it another way. Do you think that you can verbally go through this list? Do you think all the discussions that will feed into this list can be accomplished today, not writing a thing, not putting a thing down?

MS. HUGHES: I don't know. I honestly don't know.

DR. RAY: I think we can make a good dent in it. I think Brad and Barbara sort of already picked people and started to make some assignments to write the 100 words. You've got a lot of the people with the toxicology discipline and some of the others here.

If we got back after--you know, make our assignments to write the 100 words, get back and get into a discussion where we have the interaction to go through the possible impact, the characteristics of the discharges, so we can work toward a conclusion.

DR. AURAND: Then just write them on the sheets of paper and give the sheets of paper to the rapporteurs?

DR. RAY: At least that, so we can have the interchange here with the expertise and the ideas, and then we will just have to struggle to get the writing done after we get that, so you don't lose the people. If you don't have the people here to have the discussions, then you've got a real problem.

DR. AURAND: Having had a course once where he who holds the marker controls the meeting, I will be happy to do that and record it for you, if that's what you want to do. We can rip those sheets off and then have the rapporteurs try to put it together into sentences. That's fine. But, we want people to see the sentences before we walk away from here so that there are no misunderstandings about what the consensus is.

DR. BOTHNER: I vote that's the path we take.

DR. AURAND: All right. Let's reassemble at 2:00 o'clock and try.

(Whereupon, the luncheon recess was taken.)

AFTERNOON SESSION

(2:10 p.m.)

DR. AURAND: So, dealing with the concept of impacting agents and conclusions, Jerry, can we go ahead and write down what you would think maybe would be the main points? You can just talk and I will write.

DR. NEFF: Obviously, the impact-causing agents, let's assume the scenario of a drilling rig, an exploratory rig about a kilometer from one of the canyons, which I think is the most reasonable scenario.

The two areas of concern are drilling muds and drill cuttings. In that situation, you would get, presumably finer fractions of the mud into the canyonhead or into the canyon, depending on where. That is your major impact-causing agency associated with any exploration activity.

There is a remote possibility of oil spills, but the likelihood of an oil spill during an exploration is not terribly greater than the likelihood of an oil spill at any other time, I guess. There's not a lot of oil.

DR. AURAND: Actually, it's less, according to the statistics.

DR. AYERS: How about biological impacts, say, a kilometer away?

DR. NEFF: That's what I was getting to next. If you have mud and cuttings presumably falling through the water column and diluting, fractionating, so basically what you have is mud solids and some cuttings accumulating on the bottom of the canyon, that could affect local fauna through burial or chemical toxicity.

Available data, which is fairly substantial, would indicate that the chemical toxicity problem is probably very minor. There is just not much left in the solid fraction by the time it has gotten away that could cause any problems.

In terms of metal, bio-accumulation, contamination, tainting, whatever you want to call it, is obviously the other concern. Of the metals in drilling, probably only chromium, lead, and zinc are potentially toxic if they were accumulated by the animal. To my knowledge, barium to tissues is not toxic when ingested. That would be it, if the animals bio-accumulated it and you then ate the lobster or whatever.

That is pretty much where it stands in terms of potential impacts on the environment. As I said, the available evidence shows that the bio-accumulation probably is not a serious problem. Very little of the metal is in a form that can be readily bio-accumulated. It is mostly the sulfides.

The chromium actually is mostly reduced to chromium^{-3} which absorbs very strongly. My understanding is you get chromium sulfide, by anyway, it is quite immobile. There is no mechanism in a reducing environment to oxidize chromium^{-3} even though manganese potentially could. It's chromium-carbonate is probably the form it exists in sediments, even though it is introduced as Cr^{-3}.

So, that is basically where we stand. If you go through all the metals, you could say what form they might be present in, but virtually, they are all in insoluble forms, so there is very limited bio-availability, bio-accumulation.

DR. COOPER: Jerry, would a production rig have a number of anchors, 10- or 12-ton anchors in radiating directions?

DR. NEFF: If it was a semi-submersible exploration rig, it would; a production rig probably would be sitting on the bottom. It would be a structure. You've probably seen pictures of the one in the Gulf of Mexico. It's a great big superstructure sitting right on the bottom.

There are all kinds of new designs. Some of them have guidelines, but if it is in the exploration stages, you have all the anchors. Obviously, a key consideration is you don't want to put those anchors down in the canyon walls. You would probably do some damage on these vertical cliffs.

DR. VILD: Did you say the chromium is not available?

DR. NEFF: That's right. Of all the metals in drilling, the chromium is the most mobile because it is introduced as an organic complex, whereas all the other ones are in the mineral phases, you know. They are solid metal.

But when you put it in water, the chromium is introduced as six sometimes. It is a question of what form it is in, but anyway, the six tends to be reduced to three because of the organic material in the water.

Even though the geochemists will tell you that six is the stable phase, you can't get the three back to six very easily because of the slow kinetics and the fact that the three binds very tightly to particles, so basically, what settles on the bottom is three and that is quite insoluble.

DR. RAY: Certainly, I think it is worth knowing that even at high levels in the laboratory where we test for chromium effects, you still can't come up with a good correlation between chromium uptake and levels and toxicity. So far, that's been the case.

DR. NEFF: It is a tricky metal to work with, because of the multiple variances.

DR. RAY: Jerry, do you also want to comment on the produced water question?

DR. NEFF: On the produced water situation, assuming that it was discharged from the platform, and that is only one of several options available, the produced water is usually a saline brine, at least as concentrated as seawater. The salts are the same salts as in seawater--sodium, chloride, iron, magnesium, and calcium.

The main potentially impact-causing agents are hydrocarbons--low molecular weight hydrocarbons--aromatics, benzene, and several metals. Strangely enough, it is almost the same suite of metals, but they are in a more mobile form. We don't know what the species are, but they are potentially ionic metals.

The ones most likely to be high are zinc, copper, sometimes lead, so those are probably the bad actors. You rarely get high levels of cadmium and mercury. High is several-fold higher than seawater. The benchmark is the seawater, because that is what you are dumping stuff into--the seawater.

My criterion for what metals are elevated is a thousand-fold above ambient level seawater. Those are the kinds of metals--barium, too.

For some reason, some produced waters have very high concentrations of barium, 100s of parts per million, presumably because there is not enough sulfide.

MR. LANE: I take it the hypersaline conditions are diluted so rapidly, that that is not considered a major issue?

DR. NEFF: To my knowledge, except in coastal wetlands, there has never been a measurable increase in salinity, even within a couple of meters of a discharge. I know we looked for it in the Gulf of Mexico in 9 feet of water, and we couldn't see any.

DR. COOPER: Jerry, there are active squid and swordfish fisheries in the water columns, especially in these canyonhead areas. Has your experience in the past ever related to the water column type fisheries and the waters and the slight amounts of hydrocarbons that may come off, in terms of any interaction there?

DR. NEFF: I have never seen, and I don't know if anything has ever been done on the water column. Hydrocarbon concentration--there actually are other compounds in produced water, other organic, but we don't know exactly what they are. They appear to be long chain fatty acids and things like this.

DR. AYERS: The water solubles, yes.

DR. NEFF: We've just finished some studies and they are not toxic.

DR. COOPER: Would you expect, a kilometer or two or three downstream from the rig, that the typical water medium is so diluted that--

DR. NEFF: Basically, let's take a worst case scenario. In the Gulf of Mexico, in one place 9 feet of water and the other place, 30 feet of water, so things can get down to the sediments quickly.

In 35 feet of water, we saw hydrocarbons in the sediment out to about 20 meters and, at 100 meters, we barely could see any signal. These were platforms that had been discharging produced water for 10s of years.

In 9 feet of water, we could see hydrocarbon sediments in the waters, out to about 100 meters and then, by 300 meters, we were down to background again.

In terms of metals, again, a concern is you are introducing--

A PARTICIPANT: 100 meters.

DR. BUTMAN: What kind of volume was that?

DR. NEFF: That was a couple thousand barrels a day, a thousand a day, so it wasn't a big discharge. I mean, you can get up to a hundred thousand in some of these, but this was a fairly small discharge.

As I say, in-shore, since the mid-1940s, it had been discharging from this one location; off-shore, it was 5 years at that location, multiple well.

In terms of the metals, we looked for metals and we had no elevations above background in any metals; however, there was a very high background of barium in these locations and we have no idea whether that was because--if you've been to the Gulf of Mexico, especially along the Louisiana coast, there are thousands of platforms.

It could be a general area-wide thing, but we are talking about background levels of barium of a thousand parts per million or higher. There were no gradients around the platform. That was the only metal that was elevated above what we predicted would be there; whereas, all the other metals were basically normal for the general Gulf of Mexico in shore sediment.

MR. LANE: One time there was concern expressed in the Gulf because of produced water and the radionuclides contained in produced waters. Is there any literature to support that as a legitimate concern?

DR. NEFF: Well, radium-226 and -224 are the primary radioisotopes in produced water. They are typical of geologic waters of all kinds. Hydrothermal water also has elevated levels. They come from natural radiodecay in fossil deposits. There is a relationship between the salinity and the radium.

The maximum I have seen is about a thousand-fold above the normal radium concentration in sea water, and this is work done by Reed in the

Gulf of Mexico, so you can get up to a thousand-fold increment in radium isotopes in the produced water.

Again, there have been a few attempts to monitor this in terms of where it goes, and radium is fairly mobile in seawater. It doesn't seem to attach to particles too readily and there is very little signal beyond the discharge, so I don't know where it goes. Presumably, it is diluted out into the surface water.

DR. AYERS: In the process of doing surveys, among other things, we've looked at produced water, but when we talk about radionuclides, we are looking at radium-226, lead-210--

DR. NEFF: And -228.

DR. AYERS: Yes. The concern there is really again more of a marsh/on-shore, where the receiving body is small enough for the discharge. If the receiving body is larger than the discharge, then it disperses too fast for anything to happen.

Also, again, even with the radionuclides in the marsh areas, you are still concerned principally not so much with external exposure, but we need to know more about those things are accumulating, bio-accumulating.

DR. NEFF: Radium, being an alkaline metal, tends to be bone-seeking, so it is possible that bivalves and so forth and fish could accumulate it, but I've never seen any evidence that that occurs. That's where you would expect to see it.

DR. AYERS: One of the things that ought to be under consideration here, EPA is, again, working on the off-shore guidelines for mud and produced water. Even that should be done by the time anybody would ever--you know, it's only taken them about 15 years or something, but they are still going to finish before anybody would ever put a platform in.

There is a lot more information that is being developed about produced water. Certainly, there is nothing that we know today that would make us feel like we've got any emergency situation or anything alarming.

DR. COOPER: Given the discovery of significant reservoirs of oil and/or gas, what number of production platforms might be had in the off-shelf upper slope environment in the next 10 or 20 years?

DR. NEFF: That would really depend on the geology of the reservoirs, because if they were large reservoirs, you could have very few platforms, but if they are spotted all over, you are going to have to have lots of them.

DR. COOPER: What's your best guess?

DR. AYERS: If you have anything at all worth developing, you are not going to have too many platforms. It's too expensive. You are going to be 100 miles off-shore or something, and you're talking about tremendous cost. I would really think people are going to have to be looking down the road before they'll do that.

When we were drilling out there, we were thinking $100 a barrel by 1990; that's when the action was; that's what people were looking at. It's expensive to bring it ashore. There are tough drilling conditions, you know.

DR. RAY: If you ended up having multiple platforms out there, you'd probably have several miles between platforms. Because of the large number of wells you'd drill from a single platform, you'd be able to cover a couple of leases from one platform, so your spacing, if you had a number of platforms, would probably have several miles between then if you are in that situation.

DR. COOPER: We are certainly not looking at a field anything like the Gulf of Mexico, more like 10, 15, 20 at the most?

DR. NEFF: Probably, the best one would by Hibernia, which is just north. That's what, a couple platforms now?

DR. AYERS: How many platforms are in the North Sea?

DR. NEFF: Not all that many.

DR. AYERS: Is it 30, 25?

DR. BRODY: I would have said 20.

A PARTICIPANT: At the site you referred to in the Gulf, Jerry, what is the background of that?

DR. NEFF: In the sediments? They are fairly low, even though it is in the delta area, maybe polycyclic aromatics of a few tenths of a part per million, so the signal to noise ratio, if we were seeing an accumulation there, we could detect it very easily.

MR. LANE: On the physical smothering effects and anchoring, since we are talking about benthic communities, and to tie it back to the existing drilling stipulations, technological constraints, is there any problem with a 200 meter set-back from the canyon rims, in terms of exploratory drilling?

Would the physical smothering be unlikely outside of 200 meters from an exploratory or development production?

DR. NEFF: That's the experience so far, as far as I know, that if you get much beyond a few hundred meters, you don't get a pile accumulating on the bottom. That's what it takes. We are talking several centimeters to really effectively smother major benthic communities.

DR. AYERS: You could give yourself a little more leeway, 500 meters.

MR. LANE: Even 500, from an oil producing technology standpoint, wouldn't be a problem for exploration or drilling?

DR. AYERS: I don't think so. If you're going to build a platform, you know, and go to all that trouble, you want to be able to move it around a little bit to support that 500 meters.

MR. LANE: Anchoring. How far from the platforms, drill ships and semisubmersibles do the anchors usually go laterally away?

DR. AYERS: Would you say about a mile?

DR. RAY: Typically about a mile, chain and/or cable. I think it would be more or less, depending on the bottom type you are dealing with.

MR. LANE: A mile in diameter or a mile radius?

DR. RAY: Radius.

DR. COOPER: So, a given platform could have physical cables and anchors out in a circular area roughly of about 2 miles in diameter or something?

DR. RAY: Yes.

MR. LANE: Presumably, you are not going to dangle an anchor over the edge of a canyon rim. I would presume not.

DR. COOPER: The concern on the part of the fishermen is much more in terms of lost area for fishing gear than it is the effect one 12-ton anchor is going to have on a little piece of the canyon floor.

MR. LANE: Drag the distance. I mean, that was a big issue in Southern California and dragging of those anchors over significant distances was a big issue.

DR. RAY: Probably the study we did on Tanner Bank is one of the few times anybody has ever surveyed anchors. We surveyed seven out of the eight anchors on Tanner Bank. What happens is a lot different than what people perceive.

Anchor-handling procedures generally are to take the anchor out by boat and drop it straight down and then they pull tension on it, unless you happen to have a really unconsolidated bottom where you get a lot of drag. They will tend to bite fairly quickly and once they bite, they tension those things up to a couple hundred thousand pounds tension, and that sucker doesn't move.

In the last 100 or a 150 feet or so of the chain before the chain or the cable comes off the bottom, that's the point where it will work and it will have a small swath, but the rest of that thing absolutely does not move. The actual zone of messed up bottom is a pretty narrow path.

We used a submersible out on the west coast to do that, and we ran a hell of a lot of miles of nothing of video and camera transects along these, documenting what happened. This was up on a rock reef. This was on a big reef where a bunch of these chains ran across. There was a lot impact and damage than people had predicted before, not having studied the behavior of these chains.

DR. COOPER: These anchors are marked with a surface buoy?

DR. RAY: Yes, they are marked with a surface buoy. When they are ready to retrieve them, the boat comes up, pulls up the surface buoy, pulls it straight up and then it is winched in.

DR. COOPER: Trawlers would know to stay away from them.

DR. RAY: Supposedly so, because they've got 1000-gallon or 2000-gallon drum-type things, which are the buoys for those things.

DR. COOPER: We found one of those little 12-pound gems in the center of the south central portion of Georges Bank once and raised hell with some of our fishing community over the loss. They wouldn't admit to it, at least initially.

Some people's fear was that here you have a heavily laden scrod dragger about ready to bring in his last catch and head for New Bedford. Heavy seas come up and they hang up on something like that, and they could very easily flip them over.

DR. BUTMAN: Is there any operational consideration? From a current measure, there is probably substantial shear in the water column, like the top is going one way and the bottom is going the other way at fairly moderate frequencies like a few hours, and the currents could be up to 50 or 70 centimeters to a knot.

Are there operational considerations to having a drill in that kind of a shear environment with such strong flows?

DR. RAY: There are, but those aren't strong enough. They had some trouble over in Brazil, off-shore Brazil, and I think even in the South Atlantic in some places, they've had to be careful. They have had to maybe sometimes pull up or do something, take some emergency precaution, when something like this occurred. These were real strong currents we're talking about.

DR. BUTMAN: Does it need to get to 3 or 4 knots or something like that?

DR. RAY: We were studying that when we were doing our deep water drilling in the 6,000 feet of water, we were constantly monitoring and also doing studies for the warm core rings to look for unusual currents and currents of different depths. Apparently, the parameters that exist there don't seem to cause a problem.

Not only that, but we are drilling deep wells off the Gulf of Mexico now and it's pretty routine.

DR. AYERS: The only place I've known where people had any trouble with it and had to do something like pull up or something was in Brazil, off-shore Brazil, and I think in the South Atlantic.

DR. BUTMAN: They also had some trouble in the Andaman Sea with those big internal waves there. I remember reading something about that a long time ago.

DR. AYERS: Where?

DR. BUTMAN: In the Andaman Sea in the Pacific.

MR. LANE: Would Gulf Stream type velocities or warm core ring type peripheral velocities cause you to disconnect?

DR. AYERS: It didn't. We were exposed to those things.

DR. NEFF: There were some major winter storms in '81 and '82 that caused some problems.

DR. AYERS: They had to stop drilling or something. It would keep us from using drill ships.

MR. LANE: On the anchoring pattern, does it have to be radial?

DR. RAY: I'm sure they've got some on, you know--

MR. LANE: Trying to get 2 to 5 hundred feet from the rim of a canyon, how one would anchor an exploration platform or drill ship or semisubmersible without extending your anchor chain over a 2-mile swath or a 1-mile swath.

DR. RAY: I'm sure there is some flexibility, but I don't know that much about it.

DR. AURAND: Oil in the water column. Blow-outs are rare, but would you want to run through very quickly what you think the situation would be for contamination?

DR. NEFF: The general feeling is the major impact would be to surface waters, possibly eggs and larvae of fish, and that can include cod and haddock and so forth. As I say, I don't know of a mechanism to get enough oil down on the bottom to cause any serious long-term impacts.

MR. LANE: Larval stages of the species at the surface?

DR. NEFF: Yes. With cod fish, for instance, the eggs are on the bottom, they rise to the surface and then gradually sink back down again, so there is a period where they are up in the top meter or two. Basically, the fishery modelers, that's basically their hypothesis, that that is the only significant impact on the fishery business.

DR. AURAND: We went through this Monday. Basically, what they modelled is a plume going to the surface with some cone shape to it, but primarily a straight plume and then spreading in the surface.

DR. NEFF: If you have a blow-out introducing oil at the bottom, obviously, you have a slightly different picture.

DR. AURAND: Even in that case, it still is basically a vertical plume going to the surface.

DR. NEFF: In the Ixtoc case, they didn't get much oil involved. I can't imagine an Ixtoc happening on Georges Bank. Hopefully, we've learned something.

MR. LANE: How about a gas spill?

DR. NEFF: A lot of bubbles. It would soon be very volatile and be gone.

DR. AURAND: Last question for either one of them before they leave. We are going to hold this for reference as we talk. While we were coming back from lunch, Jim took the opportunity to write down what we perceived to be the list of impact agents that you would want to talk about.

While I put all of this together, Jim, maybe you want to run through what those are.

MR. LANE: I think we just added some.

DR. AURAND: Add or subtract.

MR. LANE: Maybe I'll just run through them quickly. Anybody who wants to make some additions or corrections may do that. We have only one toxicity expert and one oil drilling technology left, Dr. Ray, so we had better pick his brains in a similar fashion, before he has to leave.

Operational discharges, drilling muds, and additives. What I had originally thought of was simply putting down the volume of these materials that were released, what the toxic fractions were and whether we could postulate any mechanism that would bring enough of this stuff down into the canyon to have a deleterious biological effect.

Is that what we agreed to was the definition?

A PARTICIPANT: A biological effect.

MR. LANE: Directly related to OCS development, pile cuttings, produced water, operational discharges of hydrocarbons. It's not 50 parts per million anymore from the oil and water separated but 48. Deck drainage, sewage discharges, accidental spills of oil, accidental releases by blow-out of gas and gas condensates, produced water with the same idea, the volume of these materials and their known toxicities, whether or not we can come up with a mechanism to get that toxic fraction down in contact with the communities.

Space-use conflicts--the distribution of space, interference of the gear for fishing. We also talked about anchor chains and anchoring. We talked also about physical smothering.

I guess the question on the floor now is: Does anyone have any additional issues, impact-producing agents, that they want to consider in evaluating the impacts to the biological communities in these canyons?

DR. GRASSLE: What are those things written sideways?

MR. LANE: Volume and toxicity over here. We just got as much as we could from Jerry on each of these. I don't know if it would help to go through each of these or not.

DR. BOTHNER: How about one addition? Noise. I know there is data on that from the west coast and I don't know a thing about it, but if there are some of you who can comment, it would be interesting to hear.

DR. HECKER: Fish are very sensitive to noise. It stresses out the poor little rock fish. What if they can't mate or something because they are freaked out? All I've heard is the noise perturbs them.

DR. TEAL: That was seismic.

DR. HECKER: Out and out seismic. Okay.

DR. AURAND: In fact, MMS is the one who funded the study on that, lucky us. What we found was not so much that there was no recovery in terms of activity; what we found was a significant impact on catchability. So, what that means to the fish--I mean, if you ever beat on an aquarium--

DR. HECKER: They didn't get any?

DR. AURAND: Not if you were trying to prove to the fishermen we weren't causing a big problem, but in any case, there was no significant difference in aggregation of the rock fish plumes after whatever period of time they tested it for, I don't remember, but there was a significant impact on catchability.

We are now trying to figure out how to study that, which is turning out to be a real treat. It wasn't so much on aggregation as it was a continuous--

DR. TEAL: As I remember it, the way they did that study, they steamed around rock fish aggregation with this seismic thing.

DR. AURAND: Continually beating at them.

DR. TEAL: Yes. It wasn't as though they were doing a seismic line. They really beat on them.

DR. AURAND: It was sort of a worst case trial.

DR. TEAL: It was much worse than the worst case.

DR. AURAND: We did.

MS. HUGHES: The other part of that was that there was some measure of scattering.

DR. AURAND: Yes, but they recovered.

MS. HUGHES: But they ran out of money and didn't get back to actually take a look. After a period of time had gone by, did they actually come back to aggregate? As I remember the presentation--

DR. AURAND: I'm not sure that's right. The part we couldn't follow up on was whether they would recover from being able to catch them, because there was no significant difference on the aggregation, if I remember it correctly, and I haven't read all of the report.

DR. TEAL: It wasn't a very well-designed study.

MS. HUGHES: Right. I guess the gist of it is, it's not--

DR. AURAND: Well, we never expected to see an impact.

MS. HUGHES: We shouldn't reference it as necessarily something that provides us with a--

DR. TEAL: What it demonstrated was that if you really tried to disturb the rock fish with a seismic rate, you can.

DR. AURAND: Yes, it does demonstrate that, which is fine with some groups and not necessarily fine with us. We are left with the problem of what would happen in a realistic condition as opposed to a worst case analysis.

In fact, the design of the study was based improperly, perhaps, on the assumption that we could not do that, that we could not disturb them significantly and, lo and behold, we were able to.

DR. HECKER: Dick, do you think the tile fish would hide in their burrows if you've got drilling going on, if you've got vibration going through the sea floor? Do you think they would hide in their burrows or would they come out?

DR. TEAL: I think, in general, the noise effects, my own personal feeling is any animal that can't get used to that degree of noise is going to go extinct, anyway.

(Laughter)

DR. TEAL: But the ordinary activities, it's--

DR. KRAEUTER: There are ships going through the area. If you postulate that, then you had better cut out shipping, because the shipping noise is tremendous from just the trawling and the general boats moving back and forth through the area. This is a constant noise, which is also the constant drumming, and things get used to that.

MR. LANE: Certainly, geophysical profiling is a normal part of OCS exploration and development activities. Would it be fair to say that it is a concern as far as extensive use of geophysical profiling in an area, the possible noise disturbance to fish, or are we saying that it's not really important?

DR. TEAL: I didn't say anything about geophysical seismic profiling.

MS. HUGHES: Are you talking about noise from drilling?

DR. TEAL: Ordinary drilling production activities.

DR. BOTHNER: Has that been studied, the noise due to drilling?

DR. TEAL: We did studies on whales. Are gray whales seventeen kilometers away upset? The humpbacks stop singing.

DR. HECKER: Wait a minute. Whales are attracted to noises. Whales are very much attracted to noises.

DR. TEAL: These are some of the things that have been brought up; that's all I'm saying.

MS. HUGHES: Well, Dick and Barbara, too, in your work on tile fish, is it mostly the lights that--of your activities in the canyons--

DR. COOPER: The lights.

MS. HUGHES: Is it mostly the lights?

DR. COOPER: Yes, most of these animals are living, say, 500 to 2 or 3 thousand feet. They are living in a world, from our point of view, of virtual darkness. All of a sudden, this great big light flashing, vibration.

DR. TEAL: Vibration is right up close, too. Part of their defense is to dive into their hole when a shark comes by. When something 10 times as big as a shark comes by, they'd be very stupid if they didn't dive into their hole.

DR. HECKER: So, they are going to be sensitive to vibrations. How do they know a shark is going along if they can't see?

DR. TEAL: I didn't say they couldn't see. I said that an *Alvin* coming along is like a hell of a big fish or at least a hell of a big disturbance coming through the water close by.

DR. COOPER: We had a tile fish try to mate with us one time. Sometimes, it's attracted to lights.

(Laughter)

MR. LANE: Would it be fair to say that operational noise from platforms produces a short-term startle response on the part of some commercial fish species, tile fish?

DR. RAY: Some of our remote camera work around some of our deep well heads in around 6,000 feet of water, even when they were dangling the drill bit right over the entry gear and banging around there and everything else, there were a lot of deep sea fish just floating around and having a good old time, attracted, I think, by the physical structure, the light and everything else.

DR. KRAEUTER: If you put down anchor lines, you are going to get a different habitat and you are going to get things that may not normally be there. The evidence suggests that any time you put anything down there, you are creating an artificial reef; whether that's positive or negative, I don't know.

DR. HECKER: You might increase predation pressure on the infauna in the area, so it might be negative to the infauna, but it might be positive to the epifauna.

DR. TEAL: The stuff that grows on the rig itself falls off onto the bottom and thereby changes the environment immediately under the rig, but we're not talking about that now. Our rigs we have already defined as being 500 or 1,000 meters away from the head of the canyon.

MS. HUGHES: They are going to recommend no drilling in the canyon head?

DR. TEAL: Nothing closer than 500 meters to the head of the canyon.

DR. AURAND: Actually, Bob alluded to it and we had discussed it earlier in the afternoon, that the oil companies, at least speaking for Bob's oil company, they don't have any trouble with the 500 meter offset.

They don't really want to drill in the canyonhead if it is going to cause a lot of controversy. You heard him say it, a 500 meter offset, fine.

MS. HUGHES: I was only asking for clarification. Then, is it fair to assume that in this report, that collectively, it has been already decided that--

DR. TEAL: I am suggesting that we decide right now. If you say that cuttings and dense stuff from the muds, the cuttings, from the drilling operations themselves, can get up to a few hundred meters away. There isn't any indication that cuttings can get as far as 500 meters away. I think 300 meters was mentioned as being the most, and I'm not sure that was even for cuttings. That was for muds.

So, if we say 500 meters, then we can be pretty sure that nothing will go into the canyon.

DR. HECKER: For very fine material?

DR. TEAL: No, the heavy stuff, the stuff that accumulates on the bottom. I'm not talking about stuff that gets mixed up into the water. What you could say is that we recommend that nothing be closer than 500 meters from the canyon, so that we can consider impacts of a more important class of things.

What are these things that you are really worried about? You would be really worried if they proposed to drill right at the head of the canyon, but let's just count that out. That is certainly going to be unacceptable.

DR. VALENTINE: We haven't even discussed that. We haven't discussed what the probable impact would be of drilling in a canyon from what we know about the sedimentary environment in the canyon.

I don't think we--I'm not for it or against it. I don't see how we can exclude talking about that.

DR. TEAL: I think we're wasting our time if we talk about it, because he and she are going to see to it that their governments would hang this--

DR. VALENTINE: That doesn't preclude a discussion on it.

MS. HUGHES: That has no meaning here.

DR. AURAND: If I can, I think Bob's comment to me was that it is a given that you will bury the organisms that are in the immediate vicinity of the rig; that is an uncontestable fact. If they are down there and you drop this stuff on them, they are buried.

As far as he is concerned, if there was to be the conclusion of the group that that was not something that you wanted to do, you didn't want to bury half a kilometer of the things that live in the head of the canyon, then the easy solution is to say, "Don't put it there." That is where the comment about being offset by 500 meters came from.

I do not think that that is the same as saying that fines would not ultimately be transported into the canyonhead, but it is a comment that you wouldn't have massive burial in the canyon.

MS. HUGHES: I only raised it because it was my impression that we were trying to work up to some of those conclusions and that, certainly, in this morning's discussion and even comments that, for example, Dick made yesterday that as far as he is concerned, the biggest impact would be a massive spill.

I am very pleased and I think it would be wonderful if this group recommended that. It's just that it came as a surprise to hear it.

DR. AURAND: Minerals Management would not allow the burial of a live bottom area, if you want to use the analogy. We do not allow the burial of a live bottom area in the Gulf of Mexico.

I suspect--although I am not in the operational side of the house--that we would not issue a permit allowing them to bury a special biological habitat here, either, unless there was no other option. That's the one kicker.

If you are in a lease block where there is no option and you can show that it's not unique for that lease block and that you are not going to be taking a lot of the habitat, then you might get permission to do that or to have special stipulations, but we wouldn't probably allow that in the first place.

MR. VILD: I think we really need some sort of classification about where exactly we are going on it, because does that mean that there is still a possibility that the Interior Department would try to lease those blocks with the idea of having a stipulation that would prohibit discharges, that is, prohibit the burial of those live bottom communities?

Or, are we talking about the sort of situation, like what John was alluding to just a few seconds ago, that there is really no way in hell that those areas will ever be offered? Well, there is no way in hell that they will ever be leased, because if they are offered, the governors of New England or the Mid-Atlantic or wherever the canyons are located are going to very vigorously oppose it.

DR. AURAND: That is not exactly what I heard him say. What I heard him say was that if we came up with a recommendation that we were going to allow you to put a rig right in the middle of a canyonhead, that you would be opposed to do that. You would be.

My comment is: I don't think the Department of Interior would allow anybody to put a rig right in the middle of a canyonhead, anyway. Remember, I speak for Studies. I base it only on what goes on in the Gulf of Mexico. There are live bottom stipulations in the Gulf of Mexico, and we don't allow them to do that.

DR. VALENTINE: A canyonhead, though, you are saying, not necessarily a canyon block?

DR. AURAND: Yes, that is correct. I am not saying that we would exclude a canyon block. In fact, the Department of the Interior--if my understanding of the situation is correct--would not be excluding canyon blocks now had there not been Congressional action and the history to get us into a place where we have moratorium around canyonheads. What we would be applying would be what we wrote as stipulations.

MR. VILD: But even in a canyonhead area, are we talking about the possibility of a lease block being leased on a canyonhead, but there being a special stipulation prohibiting on-site discharge so that you don't get into this business about varying the--

DR. AURAND: I'm not willing to--I can't speak for the Department of Interior, but I think it is reasonable to consider the possibility of mitigation in these situations for the burial. Now, I am just speaking for burial.

One, the bottom line is you don't put it there. You can do that by a couple of different mechanisms. One is to not put the platform there in the first place and have an offset. The other one would be to not discharge it.

I think the Department of the Interior would prefer the offset, as opposed to the no discharge. Jim, do you want to say anything about that?

DR. RAY: My only comment on that is that that situation would depend on whether there is exploration or not. If an operator feels that an area from an exploration standpoint and the only way to drill is to drill a straight hole, and their option to drill or not to drill was to drill a straight hole and haul it all, then, you know, it might be worth their while if they think it's an important enough prospect to look at. They'll say, "Fine, no discharge. We'll haul it."

If they were going into a development scenario, then there may be a different choice they'd have to make. Then, in the development scenario, then they've got much more flexibility as far as the offset.

As I mentioned yesterday, they can kick out up to a couple of miles now with these offset wells. They have the option of not having to be directly over their target in order still to develop the formation.

DR. AURAND: I think the important point for you all to discuss in terms of the impacts is whether or not the loss of a circle 600 meters in diameter at the head of a canyon is something that you are concerned about.

If the answer is you are concerned about it, then there are two possible solutions.

DR. GRASSLE: Of course, there is another way of thinking about it. If what you are saying that there is no compelling reason for the industry to actually put a platform on the head of the canyon, and there is a potential for concern, then there is no reason to do it.

DR. RAY: That's what I said. In the development stage, you know--

DR. GRASSLE: It seems to me that that should decide the issue.

DR. RAY: On the development stage, as I say, they have a lot more flexibility as to where they actually drill the hole. In some of the exploration phase, there are certain constraints on them as far as drilling a straight hole.

DR. GRASSLE: In other words, we could adopt what John said.

DR. VALENTINE: What I want to know is: Is MMS saying that no blocks that are totally within what we call a canyon will be leased? Is that what you said?

DR. AURAND: I don't have any control over those kinds of statements at all.

DR. VALENTINE: We are assuming that--

DR. AURAND: All I said was that if you should wish to make a recommendation about offsets or back from the head of the canyon, that the industry representative said he didn't see any problem with a 500 meter offset. The existing stipulation says 200 meters.

To me, it sounds like you could recommend that you not dump stuff in the heads of canyons.

DR. VALENTINE: What I am saying is that if there are blocks that are totally within canyons--

DR. AURAND: Are there any like that?

DR. VALENTINE: Well, we don't know that. I mean, we don't have a map here, I guess. But the thing I'm hearing is that MMS is saying, "Well, we wouldn't let them lease those, anyway," and industry is saying, "Well, we wouldn't want to drill right in the canyon, anyway." That's kind of hearsay.

DR. AURAND: I'm not considering the option. Anything I said did not relate to the situation where the entire block was covered by the bounds of the canyonhead. In that case, I don't know what they would do.

I have no authority nor information to speak for the leasing side of the house, in any case, okay? So, anything I said to you would be my own personal opinion. All I'm saying is that right now, there would be an offset back 200 meters if there were to be a lease in a block which contained a canyonhead.

Bob said that he didn't see any problem with a 500 meter offset in terms of drilling the well. Now, what would happen in a situation where the entire block was inside the canyonhead, I have no idea. I just don't know.

DR. VALENTINE: The situation I am trying to avoid is that after this document is completed and we've only considered the impacts on canyons from drilling at a minimum of 500 meters to the canyon rim, then somebody is going to say, "Well, why didn't they consider drilling in the canyon?"

DR. AURAND: I think you can take care of that by just making the statement, if it were the consensus of--

DR. VALENTINE: Somehow, we have to cover this; otherwise, it is going to look like we didn't consider it.

DR. AURAND: I think John has said you could dismiss that in one sentence. It is not acceptable to put the stuff right at the head of the canyon.

DR. VALENTINE: It's easy to make that statement, but don't you have to justify that statement? It's not acceptable because.

DR. RAY: I think Page's point is a good point, but I would say address that scenario as a secondary thing. The primary scenario is, the highest probability is, that there is much more of a probability that there will be a platform somewhere in the area from which the deposit of materials on the bottom sooner or later, by bottom transport, would find their way to a canyon.

So, there is much more possibility of that than just a single situation where you would want to drill a hole right in the middle of the canyon. Address the bigger question as to whether or not there is a potential impact from the materials being transported over some distance, starting with the minimal point, into those canyons.

There is a much higher probability of something happening there; then, as a secondary, if you want to say yes, we've asked the question and given a response to it. Take the case of, okay, you're going to have a well right at the head of the canyon, even though it might not ever be politically feasible. You can cover it and the fact we've addressed it.

The other scenario is the much more likely scenario.

DR. AURAND: Please, I don't even think I should have gotten into that, but you are here to try to determine what would be acceptable to you all, as a group, from the scientific point of view.

All I am trying to say is that if you want to say that you ought to have an offset of 200 meters, fine.

DR. HECKER: 500.

DR. AURAND: But I think Page's comment is well taken. Somebody could come back to you at some time and say: Why 500?

DR. KRAEUTER: We should have another meeting.

DR. AURAND: You should probably write a sentence or two as to why you came to that conclusion, but I don't think there is anything wrong with that as a way to deal with this, rather than having to argue over--if you remove whatever the radius of that circle would be, in the event of production, what is the impact? Just say: We don't want to deal with that; we think it is important.

DR. GRASSLE: I think part of it could be that there is no compelling interest in doing that.

DR. AURAND: I'm not sure you should say interest. I think you should say there seems to be no compelling reason to get any closer to the edge than that. All indications are that industry can stay at least that far away without a real adverse impact.

MR. LANE: The way the stipulation was prepared way back in 1984 was that all of the blocks directly in the canyons were not leasable and the set-back was 200 meters. The assumption was that if they stayed 200 meters away, they could still produce from a prospect directly underneath the canyon, if need be, as long as they stayed 200 meters away from the outer rim.

DR. GRASSLE: I think we should write that down.

DR. AURAND: You guys have got to give me the words. I'm not going to make up the words for you.

DR. TEAL: The conclusion is that no rig should be closer than 500 meters from the rim of the canyon.

DR. GRASSLE: Part of the reason for that is that the industry doesn't see any particular advantage--there is no compelling reason to do it.

DR. COOPER: You are going to have to define what you mean by "rim," because some of these canyons have got very gradually sloping rims.

A PARTICIPANT: You could say 500 meters from the boundary, which is at the rim.

DR. TEAL: Which is at the 200 meter point.

MS. HUGHES: The language in the stipulation now came in as defined by NOAA.

DR. AURAND: Do you want me to put down here "as defined by NOAA"?

DR. COOPER: Those are definitions we made working about 8 or 10 years ago.

DR. BUTMAN: I'm not sure that, based on the stuff that Page showed on the western rims, that that would not preclude cuttings from getting into the canyonhead.

DR. GRASSLE: Part of the reason for that is that there is no major negative impact on the drilling operations.

DR. HECKER: I wanted to raise the issue of should we differentiate between the east and west rims?

DR. TEAL: No eventual impact, maybe, but if the stuff that's coming in is coming in the same way that the natural stuff comes in, why would that impact be any different than the natural impact?

DR. BUTMAN: By doing that, I guess you are saying you don't want the pile in the canyon axis; you don't care about stuff later.

DR. HECKER: No, I guess what I was thinking was do you relax that requirement on the west wall?

DR. TEAL: It's much easier to make the regulation uniform.

DR. KRAEUTER: Under "B", you might put the reasoning behind why we don't want the pile.

DR. AURAND: This would exclude massive accumulation of muds and cuttings. Muds and cuttings, or cuttings?

DR. KRAEUTER: Cuttings and muds, too.

DR. HECKER: Heavier material, I would think.

DR. TEAL: Heavy material; that's fine.

DR. HECKER: This would exclude the accumulation of heavy material.

DR. AURAND: Does anyone want to say anything about what "heavy" means? Somebody will ask sooner or later.

(Simultaneous discussion.)

A PARTICIPANT: Just put "mud and cuttings" in parentheses.

DR. AURAND: Can we put in parentheses there "cuttings and muds" or just "cuttings"? Jim?

DR. RAY: I think the rapidly settle-able materials, that takes in both cuttings and the settle-able fraction of the mud. When the mud partitions, part of the fine particulates go off into the water column but some of it goes straight to the bottom and those settle-ables become settle-able solids.

DR. AURAND: If anybody cares to go ahead, there is a plume model in the NAS muds and cuttings report and it, in fact, shows two transport mechanisms. There is a long-lived surface plume which is the ultimate mechanism for the transported distance and then there is another plume that goes straight down into the bottom.

What we are really talking about here is the plume that goes to the bottom. There may, in fact, be some words in there to explain the difference between them.

DR. GRASSLE: It might even be accidental discharge or whatever, if somebody drops a wrench.

DR. AURAND: This would exclude the accumulation of rapidly settling material in the canyons. Then, "C", John, I would assume that means direct burial effects are not significant?

If you are not going to have the pile, then you don't have to be too concerned about direct burial effects in the canyon? Yes? No?

MR. VILD: In the canyonheads.

DR. AURAND: Well, wherever you put the thing.

MR. VILD: We are trying to preclude direct and immediate burial.

DR. KRAEUTER: Burial and smothering, maybe.

DR. TEAL: You want to pile the sediment up slower than the animals can dig out.

DR. KRAEUTER: Right.

DR. AURAND: Which takes care of this one; in the near field, it takes care of those (indicating).

If you have the set-back, does it do anything else for you? Are there any other concerns which--

DR. TEAL: Yes, there are a number of other things. It gives added dilution to anything that is going to be suspended in the water before it reaches the canyonhead, so that if the flow is down-canyon at the head at the time whatever this is comes along, it will be more dispersed than it would have been if the rig were closer.

DR. RAY: Just for the record, I want to throw in a word on the industry comment about the set-back, because I know what will come. We are in the same position that you are. We are here kind of as semi-official industry representatives, but we know better than to presume to speak for our own companies, more or less for the whole industry.

Obviously, there are going to be people who will say, "Hey, those guys can't say that for us." So, I'll say our comments that generally, my opinion is--without having asked my company or any of the other companies--that I don't think it is unreasonable to make a recommendation of that 500 meter set-back.

I guess the other general comment as to whether or not to even lease canyon blocks, just as a general principle, you know, the industry likes to say you should, for the most part, consider leasing entire areas, but then come in with your biological stipulations or exclusions, you know, where necessary.

There may be a case where the canyon would cover the whole block and you might decide to exclude something like that. But where it is a partial thing, they say it ought to be considered and then you come in with special restrictions to do that.

But, that is the general thrust of the industry. They like to come back to the stipulations afterwards and at least have them considered, then they should not be totally excluded.

DR. GRASSLE: We could add another word, "little" instead of "no," and that might help.

DR. RAY: Yes.

DR. AURAND: I've added a note here that says that all companies may or may not agree with that.

DR. RAY: I just wanted to add that.

DR. TEAL: Add another little note that we have heard them boast that they can off-set drilling.

DR. RAY: That's no problem, John, and what I told you about the difference between exploration and development and what we can and can't do is all accurate. For obvious reasons, I know that as soon as I make an absolute statement, I'll get clobbered by everybody. I may be looking for a job, too; I don't know.

DR. KRAEUTER: We are not talking about blocks now. We are talking about a specific thing and how the blocks fall is irrelevant.

DR. AURAND: All right, so we've got added dilution. Given the added dilution, what can we do with some of these concerns? Maybe we should take metals. Is there any consensus on anything we can say about metal input in this situation?

DR. RAY: Metal input to where?

DR. AURAND: Metal input to the canyon.

DR. RAY: I can tell you what we have seen with the variety of studies we have done. Once you get outside several hundred meters, we find it virtually impossible to pick up elevated signals of all the trace metals you're looking for outside of barium.

All the other ones that are associated with the drilling fluids, we can't find them measurably above background. I guess in the case where you have one or two percent fines out of your total sediment distribution, you may be able to pick up signals, like some of the stuff you did, Mike.

In general, we have not been able to pick up elevations of these other trace metals any distance from these platforms.

DR. BOTHNER: I'll respond to that. We couldn't even find elevated levels of trace metals except right under the rig by looking at the fine fraction on Georges Bank, so I agree with you, I think, that metals from the drilling operations are going to be very weak signals, and not a serious concern.

DR. BUTMAN: Mike, I don't think you can generalize from what you found on Georges Bank in terms of where here there is a much larger fine fraction. I don't think you can say that is going to be the same, that you wouldn't find an increase in metals in a much stronger deposition environment.

DR. BOTHNER: That may be, but in Georges Bank, of course, there's much fewer fine fractions, so on the other side of the coin that I agree with, that you just mentioned, is the fact that on Georges Bank, there is a much greater concentration factor and we didn't see it.

I guess if Jim says that in all the studies so far, he doesn't see a problem with metals, I just would say that Georges Bank would confirm that. I don't automatically think that in a canyon area, you would enhance the problem from the solids that are introduced from the drilling.

DR. RAY: With all the other fines coming into the depositional area like that, your signal would be even more obscured, trying to pick up metals from the fines associated with just the drilling discharge, moving with all the other materials coming into that depositional area.

DR. BUTMAN: I would say the reason why you don't see it in Georges Bank and some of the other areas is because the fines have been transported away. They are not settling out and they are being transported away.

Here, this may be a case where the trace metals--all the trace metals--the worst case scenario would be that all the trace metals that are deposited would accumulate there. I think the reason why you don't see it on Georges Bank or somewhere else is that they are diluted tremendously as they are carried away.

In this case, the potential for dilution may not be as large.

DR. TEAL: All the evidence we have for concentration comes from his work and it shows that a two-fold concentration may be up to two-fold of plutonium?

DR. BOTHNER: Two-and-a-half.

DR. TEAL: So, that's the kind of factor that we might expect, based on whatever evidence we have.

DR. GRASSLE: That's an exploration rather than a drilling.

DR. TEAL: No, this is from plutonium and lead.

DR. GRASSLE: I'm talking about what Mike said.

DR. BOTHNER: John is referring to the lead-210 and the plutonium in the canyons.

DR. GRASSLE: Oh, I see.

DR. TEAL: I'm saying that compared to the fines on the slope, just what is in the general area, he finds about two to two-and-a-half times as much of this particle reactive substance in the canyons, so you might expect, let's say, two-and-a-half times as much of the heavy metals from the drilling activities, also a concentration factor of two and a half.

DR. BUTMAN: I wouldn't say that. That's given the same dilute source as these sediments--this area concentrates them by a factor of two and a half.

DR. TEAL: Yes. The stuff that is spread out into the water, I would say its first approximation would be a pretty dilute source. Let's say because it's only 500 meters away, it's 5 times.

The question, then, is: Is five times what you find in the other drill rigs monitoring the studies enough to be of any significance? That's the question I want to ask; however, whatever concentration factor you put on it--and it seems to me we ought to do something based on that, and set some limits around it.

I say, okay, let's use two times the concentration factor we find, because you are saying this source is more concentrated than the source that he's looking at, which is spread all over the whole area.

DR. BOTHNER: Let me say something about that. First of all, the plutonium and lead-210 data suggests something about the fate of dissolved metals that may be introduced from whatever source. It doesn't say anything about the particulates.

The other thing I want to get at is that, as I think about it, I love these tape recorders because it gets your first thoughts, but you are sometimes wrong. I am going to reverse myself and say that I agree with Brad, that what we are talking about here, in addition to the dissolved phase, is the solids which, in fact, may end up more in the canyon.

Therefore, whatever concentration the original drilling might, in solid phase, occur at is the concern to begin with; however that is diluted when it gets into the canyon is the issue that you are asking and we don't know the answer to that yet.

The chances are that it's greater than what you would predict on the basis of the Georges Bank monitoring data, so I defer to you on that, Brad. That's an important thing to bring out.

DR. TEAL: But I meant to be trying to make a distinction between the particulate stuff which, according to Jerry, is very unavailable to the organisms. It's in very insoluble forms, and whatever is soluble that comes out from the rig which is available, can be absorbed onto particles and can be leached off the particles, then, in the reverse reactions and, therefore, is available to be taken up by organisms, readily.

The kind of an argument I was trying to make, and I'm not clear now whether you are rejecting that or not, to say that if you said, say, five times a concentration in the canyons of these soluble materials, would that represent a level which was of concern to us?

I am trying to get at a way to address the question.

DR. BUTMAN: I see what you are trying to do. I guess I don't feel comfortable here with your two times or five times. I don't think we have the--

DR. TEAL: The two and a half comes from his data.

DR. BUTMAN: Right, but that two and a half is saying that--I see the availability of material to be scavenged in the canyon to be much larger than it would be on Georges Bank, the availability of dissolved constituent to be much larger than it is on Georges Bank.

There is the same amount of material, but on Georges Bank it gets distributed over a much larger area because the circulation is not confined. To get to your number, I would rather take the total number of pounds of dissolved constituents and distribute it over a reasonable area of the canyon, say, a few kilometers of the canyon axis to see what those concentrations are, since all of the dissolved constituents end up there.

I think on Georges Bank, all of the dissolved constituents are being spread over hundreds and hundreds of kilometers.

DR. GRASSLE: That assumes complete scavenging by particles, right?

DR. BUTMAN: That's right, but I think it is a safer scenario.

DR. TEAL: Then how about a different scenario? How about looking at a rig that is in an area that has fine sediments? There have been rig monitorings done in areas with fine sediments.

DR. BUTMAN: On resuspending sediments.

DR. RAY: One of the examples, when we were talking a few minutes ago, as far as the distribution of these other metals, we found it very interesting in shallow water as compared to deep water, less than 100 feet versus about 300 feet.

When we took a look at our key tracers, especially the barium, there was a factor of about 10 more of the barium in the near zone, in the immediate area around the rig than there was in shallow water, and it was because of resuspension of transport and the depth of the water column.

You get out into deeper water and that elevation of barium goes--you know, let's take within a 1,000-meter radius or something, there is a factor of about 10 more of the barium within that zone; because of the resuspension transport, there is less going on out there. There is not the wave action, so it is there.

Even in a case of a quieter environment, where you have more material settled down and stay in the area, when you look at the other metals outside of barium, you don't have to go more than a couple hundred meters from the platform and pretty soon, you are back down to ambient levels. You cannot measure those above background for the different trace metals that we are dealing with.

DR. GRASSLE: When you say deep water, how deep?

DR. RAY: That was right around 300. It's a little bit shallower than--

DR. GRASSLE: In the Gulf?

DR. RAY: In the Gulf, yes. I guess the key thing in this discussion we are having is that, you know, if these materials get to the bottom and then go through the normal dispersement process on the bottom into the canyon as the fine materials, once that stuff is on the bottom, those fine materials are just like most of the rest of the fine materials out there.

Whatever is of the right particle size, they are all going to move into the canyon together and the question is, you know, if the signal is already getting back down to background, the canyon doesn't have the ability to reconcentrate the materials other than some of the discussion we've been having on the scavenging, with some amplification from that.

In general, I don't see how you have, with all of the other fine materials that are also moving into the canyon off the shelf, I don't see how you are going to be able to measure very much.

DR. BUTMAN: Let me try to answer John's question in a different way. If you had one unit of dissolved contaminants ejected into the water on the surface, some fraction of that is absorbed onto particulates. Some fraction then gets transported away along with the ambient flow and some fraction settles to the bottom.

What I am saying is that in the canyon, because of the topographic constriction of the wall, that fraction which is initially carried away in open shelf environments with the ambient flow could potentially also end up in the canyon, and we don't know the distribution between what, in a completely tranquil environment, what gets carried away and what comes to the bottom.

I think the reason why--I hypothesize that one of the reasons why we don't see very many contaminants right around the base of the rig in all of the existing studies is that much of it has been just diluted and carried away a long way from the rig along with fine grained sediments in the upper part of the water, and that may not happen in the canyon.

DR. TEAL: So, let's take that as a scenario and say, then, what area of the--how big, how much of the upper canyon do you want to concentrate it in? Let's take a value. I mean, is it a square kilometer? Is it 5 square kilometers? Surely, we are talking about something more than a few square meters, but let's just see what the number turns out to be.

DR. BUTMAN: Take 5 square kilometers and see what the number turns out to be.

DR. AURAND: You want a number for a representative metal concentration in a mud?

DR. TEAL: Soluble metal.

DR. AURAND: Soluble.

DR. TEAL: Only a small fraction is soluble.

DR. AURAND: Yes. What I've got is just--well, let me keep looking. What I've got is a number for a total concentration.

DR. TEAL: We can look at the total. If the total turns out to be small, then the soluble can't be worse.

DR. AURAND: This is milligrams per kilogram, two values. The source is either the shale shaker, and in that case it is 70 percent solids, or the fluid discharge, which is 21 percent solids. The numbers were 44 mg/kg for chromium. Zinc and chromium were the highest, 44 mg/kg chromium at the shale shaker and 191 in the fluid discharge; for zinc, it was 80 mg/kg in the shale shaker and 50 mg/kg in the fluid discharge.

DR. BUTMAN: What is the total kilograms of discharge for a typical well?

DR. TEAL: Mg/kg of what?

DR. AURAND: The NAS didn't do a great job with labeling their tables, but it looks to be the material either in the fluid discharge, the pipe discharge of muds and cuttings or the materials collected on the shale shaker. In the one case, you would have the liquid discharge of mud and, in the other case, you would have the discharge for cuttings.

DR. TEAL: Let's assume that it is the higher number and it represents stuff that is discharged.

DR. AURAND: Well, they both would be discharged.

DR. TEAL: Yes.

DR. AURAND: But, this is where they are separated, because they are trying to recycle the mud, so they run it through the shale shaker to get the big stuff out. The big chunks have the one concentration and the fluid mud has the other concentration.

DR. TEAL: The mud would have the higher concentration?

DR. AURAND: In the case of the chromium; in the case of the zinc, it was reversed.

DR. BUTMAN: I'm sorry. One was the shale shaker and what was the other one?

DR. AURAND: For chromium?

DR. BUTMAN: No, what was the other machine?

DR. AURAND: Oh, that's just the fluid discharge.

DR. BUTMAN: In 20 percent of the solids, you've got roughly 200 mg/kg.

DR. TEAL: Yes, 200 mg/kg.

DR. BUTMAN: What is the total discharge of muds?

DR. AURAND: Jim?

DR. RAY: It ranges one to two barrels per foot drilled. You figure an average well, it's probably 10 to 15 thousand barrels.

DR. TEAL: How many kilograms of mud?

DR. RAY: Well, let's get real arbitrary and figure about a--

DR. AURAND: Let me see here.

DR. RAY: If I was going to say an average weight for mud, it would be around 13 or 14 pounds per barrel, 42 gallons to a barrel. Who has got a calculator to figure it out?

DR. BUTMAN: 14 pounds per gallon, do you mean?

DR. RAY: 14 pounds per gallon.

DR. TEAL: It's somewhere around 500.

DR. RAY: 500 pounds per barrel; 200 kilograms per barrel.

DR. TEAL: 2,000 barrels, was it?

DR. BUTMAN: Two barrels per foot of well.

DR. RAY: One to two barrels per foot of well is a ballpark as to how you predict the total amount of mud that is going to be discharged.

DR. BUTMAN: So, it's a 10,000 foot well?

MS. HUGHES: The average well depth on Georges is about 18,000 feet.

DR. TEAL: That's an exploratory well.

(Simultaneous discussion.)

DR. TEAL: 3,000 tons?

DR. RAY: The concentrations--the mud concentration varies tremendously between early in the hole and late in the hole.

DR. TEAL: That comes to a little under a kilogram per square meter.

DR. AURAND: All right. Let's see. Table 10, the average discharges of particulate solids, barium, and chromium from OCS wells. Georges Bank, eight exploratory wells, total solids in tons, 1,220--drilling fluid solids only, does not include cuttings.

DR. TEAL: What was the number again?

DR. AURAND: 1,220 drilling fluid solids only.

DR. TEAL: That's tons, so that's a million kilograms, a little bit less than we figured, so that comes to $1x10^6$ and $5x10^6$.

DR. BUTMAN: Let's just use one column. That's a kilogram per square meter. That's of mud, right?

DR. TEAL: Yes.

DR. BUTMAN: But now there's 200 parts per million chromium in that.

DR. TEAL: Yes.

MR. LANE: It's in insoluble form.

DR. BUTMAN: So you mix it down 1 centimeter, trying to get a concentration.

DR. TEAL: So, mix it into a centimeter and--

DR. BOTHNER: So it's .2 possible meter increase? A meter times a meter.

DR. TEAL: I think that's right.

DR. BOTHNER: You are putting 200 milligrams in 10^4 cubic centimeters.

DR. TEAL: So, it's .02.

MR. LANE: 102 parts per million.

DR. TEAL: That's putting it all in and concentrating it in one square kilometer. The soluble part of that is only a tiny fraction of the total. We are turning out to have a very small number, even if we concentrate it all right in the first square kilometer at the head of the canyon.

DR. BOTHNER: Even if you are off by a factor of a hundred, you aren't going to do any harm that I can see.

DR. TEAL: And we are likely to be up by a factor of at least a thousand.

DR. BUTMAN: Are you sure that's right? 200 milligrams and 10^4--

DR. AURAND: I will be bold here and say that perhaps we can work with John to confirm the calculation. I don't know that you want to hang your hat on this, but I think somebody from the Atlantic Region can take the assumptions and calculate it and get it in there.

DR. BUTMAN: We just defined that backwards. It's 2 percent, isn't it, instead of .02 percent?

DR. BOTHNER: It's 200 milligrams per kilogram; isn't that what you ended up with?

DR. BUTMAN: 10^4 cubic centimeters.

DR. TEAL: Yes, for 10^4 cubic centimeter.

DR. BUTMAN: Right.

DR. BOTHNER: 10^4 cubic centimeters, that's .02 milligrams per centimeter squared.

DR. BUTMAN: Right.

DR. BOTHNER: Let's give it a density of about--so, ppm is micrograms; that's something I didn't think of before, so that's going to be 20 micrograms per centimeter cubed and that's got a density of 1, a little bit more, of course, but that's a good ballpark, so now we are up to a 20 micrograms per gram order of magnitude increase. That compares with background values on the order of 100, 80, 60.

DR. BUTMAN: It's 20 parts per million.

DR. BOTHNER: It's a roughly 20 parts per million increase. Now you have to go like this, because it could be a third to a fourth of the ambient.

DR. TEAL: Fine. A reasonable consideration, having done this, which we know is worse than the real situation, seems to me that the conclusion is that the metals from this source are not going to be a problem even in this canyonhead situation where we worry about concentration of the materials.

DR. BUTMAN: Okay.
DR. TEAL: I think that is a reasonable conclusion from this little exercise.
DR. BUTMAN: I would say two things: that those are much larger numbers than people have seen on the shelf and if you drilled 100 wells like that, then you could start saying it could be important.
I don't know what the toxicity levels are, but you are starting to be--if it's 20 parts per million--
DR. TEAL: Jerry said there was no measurable--no indication of barium toxicity even right under the rigs.
DR. BUTMAN: This isn't barium. This is chromium.
DR. TEAL: Jerry has never seen any effects of chromium or barium.
DR. BUTMAN: He's also never seen an increase. He's never seen any increase, has he? Has he seen no effects in organisms that were exposed to chromium?
DR. TEAL: They layered the mud on top of the sediment and put the animals into it.
DR. RAY: In the laboratory work, the maximum accumulation factor we have been able to see with chromium has been about a 5- to 7-fold increase in the organisms, and that's the maximum we've ever seen.
In the work that we have done and that EPA has done, they have not been able to come up with a good correlation between chromium and the toxic effects measured in the organism. The only component of the drilling fluids that they've ever been able to come up with as a good correlation between toxicity and the material has been hydrocarbon levels.
If you have a lot of diesel in the mud, you find a good correlation, you know, about .8 or .9 but with the chromium experiments that were done, they couldn't get above a factor of about .2 or .3, somewhere in that range was the best correlation they could come up with between chromium and toxicity from the work that's been done.
A lot of the bioaccumulation studies, the bioaccumulation factors were lower, maybe one- or two-fold. I think the maximum I have ever

seen was seven-fold, and that's pretty high exposures in laboratory experiments to get that.

DR. GRASSLE: So, we should try to re-do the calculation now?

DR. TEAL: Yes, of course, but Jerry said yesterday in experiments with lobsters, unclean and heavily contaminated sediments, where they had layered the drill mud on top of the sediments, no chromium accumulation, but barium accumulated on contaminated sediment. I wrote that down when he was talking yesterday.

MR. VILD: If some animals are accumulating chromium, then that means it is available in some form.

DR. TEAL: He said no chromium accumulation.

MR. VILD: Yes, I know, but you just said, Jim, in the experiments that you were just referring to that there was as much as a seven-fold increase of chromium in the animals.

DR. RAY: With high levels of exposure, there has been, in some of the laboratory stuff, they've been able to pick up some increase. In the field stuff, there has been hardly any significant uptake of any of the metals, but it is in the laboratory stuff where we've been able to induce some of that.

MR. VILD: Then I guess that goes along with what Jerry was saying about--I guess it's the hexavalent species of chromium that's taken up by organisms. Jerry was saying that just about as soon as any hexavalent chromium hits the sea water, it binds up with organic material and is immediately reduced to the trivalent form, which apparently is not available. That would corroborate what you are saying and what he is saying, too, that in the laboratory, you do see the accumulation but not in the field, so it's not a problem.

DR. COOPER: We measured chromium and the surficial sediments in the tissues and organs of lobsters, crabs, and scallops in Georges Bank just downstream of block 310 at the head of Lydonia.

The sediments ranged from 1 up to about 11 micrograms per gram of weight; in the animals, it was all the way from nondetectable to 1.5. We were finding those levels in animals that seemed to be very healthy and vigorous.

DR. RAY: That's another thing. It's an interesting debate that biologists always have and that is, a lot of times an organism will selectively uptake or if they are in a high background level, can actually uptake metals and it acts like a bioaccumulation factor, but that by itself does not indicate whether or not it is a harmful situation.

A lot of times, the multiplication factor can go up several-fold in the organism depending on the metal, and it is still handling it without a problem. So, because you've got a bioaccumulation factor itself does not necessarily mean you are having damage.

This has been one of the big debates in a lot of the criteria the Corps of Engineers uses for dredge materials.

DR. AURAND: I didn't write fast enough the last time. Does anybody want me to write this down? You've got to say something about metals.

DR. TEAL: I still think the dilution of the metals is sufficient that it is unlikely to be a problem, at least--

DR. BUTMAN: Those concentrations we just characterized as 20 parts per million are 20 times what he said. He just said one part per millon on Georges Bank, right?

DR. COOPER: About 1 1/2 ppm. The surficial sediments went up about 11.

DR. BOTHNER: That's for bulk sediments, right, Dick?

DR. COOPER: The surficial sediments.

DR. BOTHNER: That's bulk, not by fraction.

DR. COOPER: Bulk.

DR. BUTMAN: You said the fraction in Lydonia was 50 or a 100?

DR. BOTHNER: Using the bulk patches as an indicator, the background of the mud patch, in both samples, is on the order of 50 or 60 parts per million, so a 20 ppm increase is a significant fraction of what is there; that is a fact. It goes up by a third. I mean, the geochemists can measure that.

DR. TEAL: I think there are all kinds of very conservative assumptions in our calculations. We assumed it was all going to settle out and stay in the upper square kilometer.

DR. AURAND: Would it be fair to say that dilution of the metals indicates that they are unlikely to be a biological problem, but increases probably will be detectable?

DR. TEAL: No, I don't think we can say that. I think it is conceivable that it is acceptable.

DR. RAY: At 500 meters away, I wouldn't want to bet that you'd be able to measure chromium down in that canyon over 500 meters away. I wouldn't want to bet on it.

DR. BOTHNER: The other thing I think it is important to point out, as far as the biological effects of an increase of chromium in its "benign" state everyone things it may be in is probably not a concern.

You can find other places on Georges Bank--I'd be willing to bet if we went through this data--where you find a natural level that's a good deal higher than the 60 ppm I just quoted. That kind of gives you some perspective.

DR. BUTMAN: You can put John down for references on biological effects.

DR. AURAND: Since most is in insoluble form, the potential for impact is even less?

DR. VALENTINE: And unavailable to organisms.

DR. AURAND: Since most is in insoluble form, it is unavailable to organisms.

DR. RAY: It has limited availability.

DR. AURAND: They used chromium for their calculations. "Limited biological availability" is that what we said?

MS. HUGHES: Limited availability.

DR. AURAND: So, we dealt with muds and cuttings; is that correct? We did the metals. Is there any other?

DR. VALENTINE: Can we make the statement that barium is not a problem at all? I don't think we have made that statement, but it seems from the evidence that it is not a problem.

DR. TEAL: Jerry said he had never seen an effect of barium.

DR. BOTHNER: From the experience in medicine, one would certainly expect there not to be one.

DR. TEAL: That is why I am crazy as I am.

DR. BOTHNER: A barium cocktail, which I hope you have never had, is 500,000 parts per million barium.

DR. AURAND: It makes you move quickly for the next few hours. So, barium accumulation is not biologically significant.

DR. HECKER: I do not think I am comfortable with that statement.

DR. AURAND: That is what I am waiting to hear.

MR. LANE: Let's say "biological impact."

DR. AURAND: Somebody tell me what to write next. Barium accumulation is--I think it is reasonable to assume that something should be said about Barium.

DR. BOTHNER: Barium accumulation is expected, but no adverse toxic effects are anticipated?

DR. GRASSLE: Because of its low toxicity, barium is unlikely to have a major impact.

DR. TEAL: Yes. It seems to me we could quote Jerry and say he's never seen any toxic effects of barium.

DR. RAY: I think that recent work that I was talking about yesterday, which also tends to support the theory that the material that is taken up as insoluble barium sulfates.

DR. GRASSLE: One of the reasons for the hedge is that deep-sea organisms haven't been looked at.

DR. AURAND: It is unlikely--what did you say?

DR. GRASSLE: That it will have a measurable impact.

DR. RAY: One of the interesting things is that the higher levels of barium sulfate are in the deep ocean. The deeper you go, the higher the levels.

DR. HECKER: We have got to get the qualifiers in there. I am unhappy with these concise statements from the standpoint of the truth.

DR. TEAL: I think the statement is "unlikely."

DR. HECKER: That's a qualifier, but he had "no," and I objected to that.

DR. TEAL: I agree with that.

DR. AURAND: Mea culpa.

DR. HECKER: Yes. I will even go with "very unlikely," just as long as it is not an absolute.

DR. TEAL: We absolutely guarantee that there will never be--

DR. HECKER: Yes.

DR. AURAND: Is there anything else with muds and cuttings?

DR. BUTMAN: Actually, Barbara brought up one other issue about not directly smothering, but changing settling patterns. Is that--

DR. TEAL: That's all very close to the rig, where you would get enough of the stuff accumulating. Even if it gets into the canyon, we were assuming that it would be mixed, the sediment would be stirred up and so forth and there would only be a small fraction of the sediment that's there.

DR. AURAND: I guess maybe a more general question would be: Do you want to say something about that small fraction which will be transported more than 500 meters? Some will, clearly. Other than the fact that you've addressed--

DR. TEAL: We assumed it all would.

DR. AURAND: I know, and then you then talked about the potential impact of the metals and the barium. You did not talk about the potential physical impacts that would result from that small transport.

DR. HECKER: I guess the question there is if you do have concentration in the canyon, here, I might be worried about Lydonia Canyon, the depositional area of accumulations, would the chemistry of the sediment as well as the texture prevent settlement?

How sensitive are the larvae to sediment, to chemical sediment? I don't know.

DR. TEAL: I don't know, either. We were talking about 200 parts per million.

DR. BUTMAN: 20 parts.

DR. TEAL: We are getting to the point of 200 parts per thousand, so if you put it up into an area of 10,000 times, it would be the same relative concentration and now we are talking about the whole canyon.

We are talking about larvae, but sediment, putting all the sediments into the--what I'm talking about, anyway, is putting all the sediments out into the canyon and trying to decide whether that could have an effect on the larval perception.

It seems to me that another way of approaching it would be to don't let any of it accumulate.

DR. HECKER: But if it all ends up in the depositional part of the canyon, it converges--

DR. TEAL: How big is the depositional part of the canyon? That's the question now.

DR. HECKER: Mike can tell us. You told me all about that hole there, the silt hole in the axis that had all those sea pens in it.

DR. BOTHNER: Barbara, you remember the stuff from 5 years ago so well.

DR. HECKER: There is a cliff at the landward edge of it, okay? We came down that cliff and then you've got that very fine material. How big is it? How wide was the canyon? What sort of area are we talking about?

DR. BOTHNER: Was this the morning dive or the afternoon dive?

DR. HECKER: It was dive 1037, dear.

DR. BOTHNER: I don't remember that. The answer to that is to just look at a topographic map. From the dive description, I couldn't tell you.

DR. HECKER: You've got the depositional area of the very fine-grained sediment in the axis, say, from 300 to 450 meters? Do you remember the grain size?

DR. BOTHNER: I remember mostly from Brad's chart yesterday that showed this.

DR. HECKER: Approximately how large an area are we talking about, Brad?

DR. BUTMAN: I think it is reasonable to use a few square kilometers. It was probably 3 or 4 kilometers long and a 1/2 kilometer wide or something. Using 2, 3 or 4 kilometers is probably all right. I think it was about 1,000 cubic meters. Spread that over--

DR. TEAL: $2x10^6$?

DR. BUTMAN: 1 square kilometer is 0.1 centimeter. If you spread it over 10 square kilometers, it is 0.01 centimeter.

DR. TEAL: Mix it into the top centimeters, then.

DR. BUTMAN: Mixing it into the top centimeter is another--well, I don't know for sediment.

DR. TEAL: Well, it's coming down on currents. The process that is bringing it down there is the same process that is stirring up the sediments. The process that is bringing it down there is the same process that's stirring up the sediments, isn't it?

DR. KRAEUTER: Not only that, but the fine grained sediments there, you've got infauna certainly in the top centimeter.

DR. BECKERT: It is also coming in over several years, not all at once.

DR. TEAL: That's correct. The mixing you showed down to 10 centimeters or the upper 10 centimeters.

DR. BOTHNER: If you run a calculation on those mixing coefficients, you can make the assumption that within a year, an average particle goes down an "X" number of centimeters on the average. So, there is a lot of reworking going on into this dilution.

DR. TEAL: Don't you think that could have an effect on the settling of those organisms? That's the thing. We're talking about mixing.

DR. BUTMAN: I would say if you put 0.1 centimeter on the surface--

DR. TEAL: And left it there, yes, I think that would have an effect, I agree with you.

DR. BUTMAN: Okay.

DR. TEAL: But we are talking about putting it in over a long period that has mixing episodes and also biological mixing.

DR. BUTMAN: Right.

DR. TEAL: It seems to me it would be unreasonable to assume that it wasn't at least going to be mixed in the top couple of centimeters.

DR. GRASSLE: Well, over a period of time.

DR. TEAL: But it is being put in over a period of time, too.

DR. GRASSLE: At a lower depth.

DR. TEAL: At the depth we are talking about, it's not terribly deep.

DR. VALENTINE: We are not talking about a tranquil environment here. This stuff is being resuspended every day.

DR. HECKER: I guess I'm concerned, also, about gross sediment. Again, what I know about the larvae of corals is that they are exceptionally sensitive to textural and chemical.

DR. TEAL: But we are talking now about an environment which is very changeable, apparently, because it gets stirred up and mixed every day. These corals, I would expect to be more tolerant of changes just for that reason. We have heard so much about that.

DR. HECKER: Yes, you hear so much about it, but when you are actually down there and looking at it, they look pretty damned tranquil. That silty area looks tranquil.

DR. BUTMAN: You're not down there for very long.

DR. TEAL: One time, everything was pretty clear and then it began to pick up and more and more stuff was flying around.

DR. HECKER: I'm talking about the silty axis at the head of Lydonia. They are patchy. Also, you are talking about along the rim, the walls, you've got differences in the axis. In Lydonia Canyon, you go down to 600 meters and you've got material flying past you like Dick was talking about. Go to 900 meters and your sub is thrown all over the place. It depends.

DR. TEAL: But we are trying to put it into this area of where the deposition occurs.

DR. BOTHNER: In that area, there is frequent resuspension.

DR. VALENTINE: Brad's data shows current speeds over years, months and years, and we can determine from that whether this stuff will be resuspended and it probably will be.

DR. BUTMAN: The only caveat to that is that those are in the axis. A lot of Barbara's observations are on the walls and I think the resuspension is probably, because of the focusing of the current energy, there is probably less resuspension on the walls than in the axis.

DR. TEAL: Then the area is going to be more spread out, too.

DR. BUTMAN: Right.

DR. TEAL: The area becomes greater then.

DR. BUTMAN: I think it is fair to say you have to stretch to get a physical effect on sediment with those concentrations that we are talking about.

DR. TEAL: I still think it is fair to say that a reasonable expectation is the effect of the sediment itself on the properties--the mud discharges on the properties of the sediment, I wouldn't expect to have any.

DR. HECKER: It is probably unlikely, yes.

DR. TEAL: That is a double qualifier, Barbara.

DR. HECKER: I like double qualifiers, yes.

DR. AURAND: Is that "almost unique"?

DR. HECKER: It is probably unlikely that the fine materials from the drilling activity would alter the physical characteristics of the axis enough to present a problem to settlement of sessile larvae, sessile critters.

DR. AURAND: She won. She got it.

DR. HECKER: Did you get that?

DR. AURAND: She did.

DR. HECKER: It is probably unlikely that the fine materials from drilling would alter the physical characteristics of the axis such as to preclude settlement by larvae of the sessile organisms.

DR. AURAND: Does it have to be restricted to sessile organisms?

DR. HECKER: Those are going to be your sensitive ones, so the other ones--

DR. AURAND: Of benthic organisms or sessile?

DR. HECKER: Larvae of benthic organisms.

DR. AURAND: The only other thing I can think of that we talked about that you might include in this would be a statement about the commercial species. I think we dismissed that earlier this morning in large measure and you may want to put that in.

DR. HECKER: You may want to cross out "physical characteristics" and just say "would alter the physical characteristics of the sediment in the axis." Someone just pointed out that might mean topography. Physical characteristics of the sediment.

DR. RAY: There was one other category that Fred noted a little while ago and that was I made the comment about toxicity in drilling fluid related primarily to the hydrocarbons and Fred noted the hydrocarbon issue.

The use of diesel is no longer allowed in drilling fluids.

DR. GRASSLE: Actually, I was thinking about it when we were going through this exercise of saying whether this material that reaches the bottom is going to have any effect on larvae. The overwhelming effect is that chemically the stuff is different and it may or may not be good for organisms, if you are talking about larvae. It just has a lot more impact than any physical consideration.

DR. MILLER: Let me interject. Instead of taking a full break, we have got coffee and cokes out here, so if people would like to have that, they can get up and go out and serve themselves, if they would like.

(A brief recess was taken.)

DR. AURAND: On commercial species, would you want to propose something to add to this list?

DR. COOPER: If I could say something--

DR. AURAND: Forthrightly. Now that you mention it, the position of the Department of Interior on leasing canyon blocks was formulated by the Conservation Division [correct name?].

DR. COOPER: Let me just verbalize this first and see how it sounds, to me as well as you. Given the conditions of not drilling any closer than 500 meters from the canyon rim--

DR. VALENTINE: The Conservation Division [correct name?] is now MMS.

DR. AURAND: But it was done back when you guys were in control, no doubt about it.

DR. COOPER: It is very unlikely that there would be any measurable impact from the drilling for oil and gas on your commercial species in the heads of the canyons. There is one exception to this, and it is a very nebulous aspect of it. None of us has a very good feel for it.

I really do not have a very good feel myself of what impact an oil spill may have on the larvae.

DR. AURAND: We are going to talk about oil spills separately.

DR. COOPER: But in terms of the effects on the mobile commercial species and so forth, there would probably be little, if any, impact.

DR. AURAND: Okay.

DR. GRASSLE: I guess this is on the existing populations. You are saying you don't know about recruitment?

DR. COOPER: I do not know about recruitment.

DR. GRASSLE: That makes it a little bit more tricky, because it means that there could be an impact, if you don't know about recruitment.

DR. COOPER: We are separating spills. We are dealing only with operational discharges, and I don't think you should or necessarily would make that same statement for accidental discharges.

DR. GRASSLE: Yes, I think that's best.

DR. COOPER: Fred, one of my reasons for saying this is that your most commercially valuable species out there are lobster, which is highly migratory, in-shore, off-shore, in between canyons, at such a high rate that even if there was a 50 percent to 90 percent kill-off in a canyon, as long as there wasn't any long-term pervasive environmental stress that stayed in these canyons, those areas would be repopulated very quickly.

DR. GRASSLE: The tricky part is tile fish recruitment.

DR. COOPER: The tile fish is probably the only species that I'm aware of out there that is highly endemic to a given grotto area and very likely would not move out of the area, regardless.

DR. AURAND: What about commercial fisheries? What words did you use? I made it for operational discharges with a 500 meter set-back, it is unlikely that there would be any measurable effects on--

DR. COOPER: Commercial species in the heads of submarine canyons.

DR. HECKER: With the exception of tile fish or including tile fish?

MS. HUGHES: Do you mean of exploitable size?

DR. COOPER: I don't think there would be any effects from operational discharges. I don't mean a spill. We just finished discussing that. My comment is on the benthic-oriented population as it exists in that point in time.

DR. AURAND: Do you want "benthic-oriented" added?

DR. COOPER: I'm sorry. I was listening to two people.

DR. AURAND: Did you want "benthic-oriented commercial species" or just "commercial species"?

DR. HECKER: Existing benthic-oriented commercial species?

DR. COOPER: It is not going to have any measurable effects on the commercial species--on the benthic oriented commercial species.

DR. BUTMAN: Do you want to say adult stocks of the commercial species? We just talked about the recruitment issue.

DR. KRAEUTER: You can't just say adult.

DR. AURAND: Whatever, again, we are separating spills from discharges.

MS. HUGHES: Are you talking about drilling muds and cuttings or are you talking about produced water? Do you want to talk about the hydrocarbon concentration?

DR. AURAND: We haven't talked about produced waters, yet.

MS. HUGHES: Operational discharges is just drill muds and cuttings?

DR. AURAND: Yes.

MS. HUGHES: We're not off the exploratory phase, is that right?

DR. GRASSLE: The only difficult issue and I don't know if this might be important, but you could think of a situation where you have enough material on the bottom to inhibit larval settlement for chemical reasons rather than the sort of physical ones we are talking about. It is an unknown.

DR. COOPER: With virtually all of these canyons, at least from what I have been listening to in the last day and a half, there are at least sufficient high energy periodic events that are sweeping and flushing these canyons. The dilution factor is so great, I really don't see a net build-up with the possible exception that in some of the axis, there is an area between 300 and 500 meters in the head of Lydonia Canyon that appears to be a net depositional area.

DR. BUTMAN: I think we may have made this resuspension issue a little bit too strong. That is certainly the case on the axis, but as I said this morning and tried to say several times, the walls may be much more tranquil than that. They may not be swept clean often; at least, we don't have any data to suggest whether there is heavy resuspension on the walls or not.

DR. COOPER: Let me say this: The areas where you have your high abundances of commercial species on the walls of the canyons are high energy areas; they are not net depositional areas. I cannot see--I cannot imagine conditions where there would be a net deposition of anything on them.

Your boulder fields where most of your commercial species are located are areas where there is--as the fines are stirred up through biological activity, they are swept away.

This may seem like a daring statement on my part, but from everything I've heard so far, I don't think you would ever be able to measure the impacts on any of those fauna out there unless there was just a massive accident, a massive oil spill, or something.

DR. VALENTINE: I think the walls of the canyons may be characterized as areas where sediment is either being eroded by bio-erosion, say, or is in transit. The question is: How long does it take to move down the walls towards the floor?

DR. BUTMAN: You say it is in transit because it is mainly sand?

DR. VALENTINE: It is sand and silt mixed. The shelf sand becomes mixed with the bio-eroded--

DR. BUTMAN: What is the evidence that it is in transit, though?

DR. VALENTINE: Well, the evidence would be ripples, ripple marks, accumulation of the bio-eroded material on the canyon floor.

DR. BUTMAN: I guess from the limited dives I did in Lydonia Canyon, I didn't see any ripple marks in the canyon walls in the places that I dove. I think that that may be true in Oceanographer Canyon, but not necessarily true otherwise.

DR. VALENTINE: The walls are not uniformly rippled. They are patchy, but I think you would have to--

DR. HECKER: There are patches of ripples deep in Lydonia Canyon, patches on the walls.

DR. COOPER: I've seen rippled areas in a number of Lydonia Canyon walls, especially in some of the little tributaries.

DR. BUTMAN: I agree that there are some places there are and some places there aren't.

DR. VALENTINE: It might be depositional for long enough for a layer of fine grained material to have an effect, conceivably.

DR. GRASSLE: What is the toxicity effect?

DR. BUTMAN: I just don't want to give the impression that everywhere in the canyon it is violently being mixed every 5 minutes so that anything that settles there is immediately resuspended and transported.

There are some areas which, over long time scales are probably fairly tranquil, so I could see possibly chemical or physical effects of a layer of drilling mud changing settlement, but not for a long period of time.

DR. COOPER: I can see this for, what, some unknown period of time in the head of Lydonia.

DR. BUTMAN: I just wanted to make sure that we had the same conceptual picture here that it's not--

DR. VALENTINE: But the area that is covered by this so-called layer might not be very big because of the patchiness.

DR. GRASSLE: That is the mitigating thing, that there is always going to be some surface suitable.

DR. BUTMAN: Right.

DR. COOPER: Most of the commercial species that we are talking about here, they are very highly habitat type three and four oriented. If you sit for a period of time on the bottom in a submersible, for example, and watch the intensity and the frequency with which surficial sediments are stirred up because of the biological activity, and this stuff, as I remember it, hardly settles before it's stirred right back up. It's going up canyon and down canyon.

DR. RAY: One thing is that the materials are going to be coming from the drilling operation over a period of months or years. Aren't there going to be--there are going to be similar materials of similar grain size and composition also coming into the canyon at the same time as the materials from the drilling.

From the way the conversation is going, it is sounding like the only thing coming down the side of the canyon down the wall is going to be the drilling-related solids, yet, they are the same kinds of materials that are fine-grained stuff, you know, on the shelf.

I am asking the question: Aren't there other materials moving into the canyons of a similar particle size?

DR. VALENTINE: Also, the fine-grained stuff is generated by the bio-erosion right in the canyon itself.

DR. BUTMAN: Except the accumulation rates which Mike measured were 60 centimeters per 1,000 years, which is 2 grams.

DR. KRAEUTER: That is the accumulation, the long-term net accumulation.

DR. BUTMAN: That is .06 centimeters per year.

DR. KRAEUTER: Right.

DR. BUTMAN: We talked about if you spread all the drilling discharges over one kilometer, it is .1 centimeter per well, so there is still a big question about what area you spread those discharges over.

The point I want to make is that the natural accumulation rate is fairly low, also, in terms of--there is a lot of material coming in, but it is also being spread over a fairly big area.

DR. VALENTINE: There is also the timing factor here. The larvae settle at certain times of the year.

DR. GRASSLE: For deeper water things, it is usually more continuous, but I don't know whether that is known for these species. It probably is known.

Anyway, it is a far-out scenario, but if you have larvae that are settling in the same place as materials being deposited and that material has different chemical characteristics, even though it is nontoxic from the material that is normally getting into the canyon, then it could have an influence on recruitment.

DR. BOTHNER: Brad, if you want to compare the flux coming in over that 1 square kilometer with accumulation and worry about dilution and so forth, maybe the better number to use is not so much the long-term net accumulation, but rather the resuspension rate, assuming the sediment traps are doing a good job on the efficiency.

You compare the 0.1 of a centimeter per year versus 8 or at least 10 times higher than the average rates of accumulation, which brings it up to be 0.6.

DR. BUTMAN: Compared to the resuspended flux.

DR. BOTHNER: Compared to the resuspended flux. So, now, the scenario for the drilling accumulation is one-sixth of the natural flux rather than two or three times higher than the natural flux.

DR. GRASSLE: The scenario could still work with even a fine layer that is continually--you know, if it is always there, even though it is continuously in migration somewhere else over the long term.

DR. TEAL: But it will be patchy.

DR. GRASSLE: That's why the caveat, that the larvae have to normally prefer the deposition sites. That's where the higher organics will be.

DR. TEAL: The deposition is always very low in the whole place.

DR. GRASSLE: I was just saying that when they settle, they are settling in a place that at that moment is depositional.

DR. AURAND: When you talk about high organics, are you talking about--

DR. GRASSLE: No, I'm not talking about high organics. I'm talking about something that affects the larval behavior in the settlement. I'm talking about concentrations that can be lower than those that would cause toxicity. It could be narrow. I'm not talking something that usually you worry about in the sense of an effect on--

DR. AURAND: Most of the drilling muds and cuttings are clays.

DR. GRASSLE: There is an oil component. It is different from the normal stuff that is going in there. It is certainly measurable that there is some organic component that is somewhat different from the natural sedimentation.

DR. TEAL: The drill muds on the surface would taste different than they would normally, yes.

DR. GRASSLE: Yes.

DR. RAY: Most of your organic compound is lignin sulfanates. That's the primary organic compound of the drilling fluids.

DR. GRASSLE: It's those lignin type things. I guess that's the highest concentration, but there is a concern about concentrations that could have an effect on behavior of larvae, even though they are very low toxicity.

DR. AURAND: Are we ready to move on to the produced water?

DR. TEAL: I do not think we need to worry about the sewage; let's talk about the produced water.

DR. AURAND: Do you want to avoid any comments on it at all, or just say they are minor problems?

DR. TEAL: Well, they are minor in relation to these others.

MR. LANE: Jim, am I correct in assuming that the produced water is also run through the oil-water separator on the platform, and that is where you have the discharge limitation of 48 parts per million?

DR. RAY: There are a variety of different kinds of separation equipment. There is an oil-water separator and there are gas-flotation units. There are a variety of different things, but one way or the other, they put on whatever the necessary treatment is to get down to the allowable oil and grease maximum for discharge. Without that, they can't discharge it.

DR. AURAND: So, must meet--

MR. LANE: The 48 parts per million discharge standard.

DR. RAY: 48 milliliters per liter oil and grease.

DR. TEAL: What does that mean?

DR. RAY: What does that mean?

DR. TEAL: How do they measure it?

DR. RAY: That's where they measure the oil and grease test. What we find is in produced waters, the oil and grease test is primarily picking up the dispersed oil that we've got in there.

The thing that the oil and grease test does not pick up are soluble hydrocarbons, and that is what a lot of studies going on right now are dealing with. The total organic carbon of a produced water can range anywhere from about 200 up to about 600 milligrams per liter total organic carbon. Napthenic acids make up a good portion of this.

A lot of things that fall into that envelope are hard to even analyze for, but the oil and grease test is the primary test that is used and that is primarily picking up our dispersed oil that is in the water.

MR. VILD: Jim, do you have produced water even when you are doing exploration drilling or is produced water just kind of by definition associated with oil and gas?

DR. RAY: No, produced water is a byproduct of when you produce oil, when you bring oil to the surface. When you bring oil to the surface, it comes up with water and some gas. Then you go through a separation process to remove that gas and/or water so that you are shipping and transporting primarily just the oil.

DR. AURAND: I guess that takes are of that. Where do we go next?

MR. LANE: Does 48 parts per million scare you?

DR. AURAND: Also, if you were to discharge it on site, it would be probably a surface discharge; is that correct?

DR. RAY: Assuming there were no stipulations on us, it would probably be right at the surface or shunted probably within 10 meters of the surface.

DR. BUTMAN: What would a typical volume be?

DR. RAY: The average volume across all the operations in the Gulf of Mexico is somewhere in the vicinity of 2,000 barrels a day. In a lot of areas of the Gulf of Mexico, you have big collection facilities where you have one big production platform handling the production from a large number of wells.

When you have central collection facilities like that in older fields, the total discharge from a single location can be somewhere around 100 to 140 thousand barrels a day. That's the out-lyer, but there are very few of those large volume discharges; the average is in that couple of thousand a barrels a day range.

DR. GRASSLE: Is there anything known about the geology up in the northeast that would suggest it would be at the high or low end?

DR. RAY: Not that I know of. I don't think they have found enough oil to figure out what the produced water characteristics would be.

DR. TEAL: What did they produce in Hibernia?

DR. KRAEUTER: What about the larger California wells? Have you got any information on any of those?

DR. RAY: I don't know what their volumes are out there.

DR. BUTMAN: Is that per well or per platform?

DR. RAY: Per platform. Again, I don't have the foggiest idea of, you know, if you got into a 50- or a 100-slot platform. Let's say you got lucky out there and you had a big find, you know, you've got a big field and you have a 50- or a 100-well platform.

I don't have the foggiest idea as to what the volume of produced water might be from that. Again, as I said, if you have drilled your wells in the proper positions, you know, for quite a few years, you will have very little produced water production because you are trying to produce the oil.

As that field drains down, some of your wells out in the periphery of the field will start drawing more and more water, by percent.

DR. AURAND: Volume increases with age?

DR. RAY: That's the general trend, yes.

MR. LANE: Are similar volumes of water produced in a gas well?

DR. RAY: No, in gas, there is very little produced water involved. If you are lucky, you've got nothing but gas flow.

DR. AURAND: Does somebody want to say something about whether or not we even want to address this?

MR. LANE: It seems to me that we can't possibly have a mechanism for massive oil spills for oil getting to the bottom; we can't do much about it.

DR. AURAND: That's why I asked about the surface. I think if you are going to have to postulate impacts, you are going to have to postulate surface impacts.

DR. GRASSLE: The only conceivable time that you'd have an impact is when you have a lot of particles in the water column that are settling out; that's the only time you would have a significant transfer to the bottom.

MS. HUGHES: The only question I have about produced waters and that I think a lot of people have is the aspect of it that it is a chronic discharge over years. What does that mean, if anything, even if it is at the surface?

DR. TEAL: The soluble things are, to my understanding, the most toxic things in produced water are also the more volatile. They are fairly rapidly lost in the atmosphere. I don't know what that means in terms of how far downstream you would expect to find a particular concentration.

Jerry, when he was talking yesterday about the fluids in the drilling muds put into surface waters, said that within 200 meters of the platform, they were diluted--what I took him to say, he said diluted to 10 parts per million, which I thought meant a 10^5 solution within 200 meters of the platform.

MS. HUGHES: I was just wondering what he said.

DR. AURAND: In the produced water, he said that the saline brine impact agents are low molecular weight; hydrocarbons, there are several metals in the drilling muds. They are several-fold higher than sea water except that there is rapid dilution and, except in coastal wetlands, there is no measurable increase in salinity even within several meters of discharge; in other words, rapid mixing.

In 35 feet of water, hydrocarbons were detectable until out to 100 meters after several--I wrote down 5--years of a 1,000 barrel per day discharge; no elevations of background metals at that distance.

I can tell you that we are looking at the effects of produced water discharges in coastal embayments, but not in off-shore situations, because they have become an issue in coastal areas in Louisiana. In that case, they sometimes discharge them into canals and, in those cases, they have had a very definite impact because you will see hydrocarbon build up in the ends of the canals and you will wipe out the benthic fauna.

It has not come up as a study topic in the off-shore situation. Florida is concerned about it, but they don't know what to do with it, either, in terms of how to treat it. All the evidence is that there is very rapid mixing and a return to normal salinity levels, which is what everybody seems to use as a tracer for this.

DR. RAY: For water dispersement, I think a lot of those dispersion rates, in general, are fairly well known from a variety of different dispersion studies. For the produced water, probably as a category of materials, probably the highest concentrations of specific organics are in the volatiles that John was talking about, the benzene, xylene, toluene complex which can make up--in the worst case, I've seen--that was about 10 parts per million.

Most of the other different groups of organic compounds that are present there are in the part per billion level and that is before it comes out of the pipe. You are looking at these 10^4, 10^5 dispersion ratios in that first 1,000 or 2,000 feet or so.

But that is the concentrations you are starting at as far as the organics that are associated with the oil, so as I say, the BXT complex which, probably as a single group, are the highest in the low part per million range.

All of the individual compounds that you potentially track--the naptholene and some of those other things--are in the mid to low part per billion range as individual components.

DR. AURAND: Would it be reasonable to say: If there were to be production, the mechanism of discharge would have to be carefully examined? Do you want to say something like that?

DR. RAY: Pat had a comment to me here a minute ago. She was just saying that with some of the larvae and stuff that tended to congregate near the surface level, that probably if you just were shunting down 10 meters, you know, that alone, by the time the produced water--if it came back to the surface, by the time it gets there, it probably would have gone through a dilution.

MS. HUGHES: The question was: What is the surface discharge? Does industry consider a surface discharge to be anywhere from the surface to below 10 meters?

I was thinking of the surface waters and hydrocarbons with regard to lobster larvae.

DR. RAY: Your point, then--

MS. HUGHES: They are in the very upper few, less than 5 meters, of water.

DR. GRASSLE: I think it's safe to see a water column effect in an oil spill situation, even when there are larvae out there.

DR. BUTMAN: I just did a rough calculation. If you take that 2,000 barrels a day at 48 parts per million, that's about 1 liter of oil per day in terms of total volume.

DR. AURAND: At a volume of 2,000 barrels?

DR. BUTMAN: 2,000 barrels.

DR. TEAL: I guarantee the fishing boats are putting more than that into the water out there every day.

DR. BUTMAN: Right.

DR. RAY: Again, down the road, Don, from the MMS standpoint, you know, if there is a recommendation as to where that kind of a discharge should be, then it should probably come from MMS.

When I say "at the surface," I've seen them discharge it from the platform and it falls 100 feet through the air onto the surface of the water; I've seen shunt pipes down right near the surface of the water; I've seen shunt pipes 5 and 10 meters below the surface of the water.

Without direction, then it is totally variable depending on the operator. It's real variable. Without direction, they will do it the easiest, least expensive way possible, as a general criteria.

DR. AURAND: Based on information on the concentration of hydrocarbons and metals in produced waters, it is--

DR. GRASSLE: We don't anticipate any water column effects.

DR. AURAND: Is there any discussion?

DR. GRASSLE: Does that imply benthic?

DR. AURAND: That's a good question. If you don't expect water column effects, do you expect bottom effects?

DR. GRASSLE: The only conceivable way it could accumulate on the bottom are in situations where there is a lot of sedimentation. What is your sediment traps sedimentation rates finding? That's the sort of maximal rate at which stuff is going to be transferred to the bottom.

DR. TEAL: Not in the canyon.

DR. BOTHNER: Just on the shelf?

DR. TEAL: Yes, next to the canyon or something like that.

DR. BOTHNER: Next to the canyon, about 10 grams per day.

DR. GRASSLE: Then you have to make some assumption about the scavenging of hydrocarbons by particles, which is pretty good.

DR. BOTHNER: But the rate is not primary flux, which is what happens when you have phytoplankton blooming and falling out but rather, resuspended flux.

DR. GRASSLE: My feeling is that the accumulation would be very slow and based largely on the periods of spring blooming.

DR. AURAND: Do you want me to change this to say benthic and water column effects are not expected, or finish the sentence which says, "For benthic effects to occur"?

DR. GRASSLE: I think you should distinguish the two.

DR. AURAND: Then help me finish the sentence, "For benthic effects to occur."

DR. BOTHNER: Benthic effects are not expected, but would have the highest potential during periods of spring blooms.

DR. GRASSLE: Actually, even that is not a worry. What you are worried about is the gradual accumulation, so accumulation on the bottom--gradual accumulation of hydrocarbons on the bottom is likely to occur over a period of years.

DR. AURAND: And could occur in a localized area?

DR. TEAL: I don't think it is likely to occur. These things are both fairly degradable but also very volatile.

DR. RAY: You've got degradation going on the whole time you've got the--you know, in the stuff that's settling, you've got degradation going on the whole time.

DR. TEAL: We're talking here about the light end of things. I can see closer to the shore--

DR. AURAND: In situations that I know of, they are marsh channels about 5 to 10 feet deep. You can find hydrocarbons in there. Jeff said in 35 feet of water and the other case was 9 feet of water. In 35 feet and 9 feet of water, you could see hydrocarbons out to 100 meters.

DR. GRASSLE: I haven't finished with the sentence--"could occur and should be monitored."

DR. TEAL: I think that's too strong. I cannot agree with that. I think they are very unlikely. Then if you want to recommend that you monitor something that is very unlikely, why, that's okay, but I don't think you should.

DR. GRASSLE: I think that the issue of gradual accumulation of hydrocarbons, when we consider all the potential sources, needs to be addressed over the long term.

DR. AURAND: How about this: Gradual accumulation of hydrocarbon on the bottom has been shown in shallow areas and, if it has not been addressed by the time they go to production here, it ought to be examined. Is that clear?

DR. GRASSLE: No, I think the issue is--

DR. TEAL: I don't think it comes from this source.

DR. GRASSLE: That's right, and I think that's our hang-up, is that if there is accumulation, the betting is going to be that it is from minor spills or accidents or whatever.

DR. TEAL: Yes.

DR. GRASSLE: So, the point is that somebody is going to have to look to see if there is a gradual accumulation; it is going to have to happen if this goes to production.

DR. TEAL: That's true.

DR. GRASSLE: Pinning it on the produced waters is not something we want to do, but certainly, it needs to be considered in the overall plan.

DR. BUTMAN: This bring up a--this is the first time it has been brought up, but although we are saying that many of these things are not a problem, is there sort of an underlying feeling that there should be some monitoring program of some of these effects?

DR. TEAL: You guys just ought to wrap up your equipment and go somewhere else.

(Laughter)

DR. BUTMAN: That's what I want to hear.

DR. GRASSLE: Category 4 up there--

DR. BUTMAN: I just wanted to highlight what he said.

DR. AURAND: If production were to occur without further resolution, this issue should be monitored. It is more likely that such hydrocarbon build-up would be related to small spills.

DR. GRASSLE: Let's just say "accidental" before "small."

MS. HUGHES: Or small accidental spills.
DR. AURAND: We will let the editors do that later on. That takes care of operational discharges.
Accidental discharges: We have two, oil spills, gas and gas condensate blow-outs. Why do we have produced waters in accidental discharges?
MR. LANE: That comes out with the rest.
DR. AURAND: You meant produced water as a component of either oil spills or blow-outs.
MR. LANE: Is it useful to define a spill?
DR. AURAND: This is a game I've been playing in Florida for the last 6 months. I'm not interested in defining it.
DR. TEAL: I don't yet know why we want to define a spill.
DR. AURAND: I think one place to start is to differentiate between surface and benthic impact.
DR. KRAEUTER: Do we want to differentiate between spill and blow-out?
DR. AURAND: If you accept what we heard yesterday, by and large, the ultimate impacts seem to be the same because this stuff goes to the top.
DR. KRAEUTER: I'm not willing to accept that.
DR. AURAND: You didn't like it when they said that, but you just kept quiet because it was getting late; is that it?
DR. KRAEUTER: They didn't provide very much evidence to suggest that their model was correct.
DR. AURAND: Well, that's what you guys were supposed to evaluate.
DR. RAY: What was the issue here, John?
DR. KRAEUTER: If you spill oil on the surface, that is substantially different than oil riding through the water column in terms of the potential mixing of the toxic fractions into the water. A lot more can be mixed with water as opposed to that which is spread on the surface and then it's got to go down by mixing with surface waves or something, so for the toxic fractions, you have a lot more of it in the water.

DR. AURAND: A standard assumption appears to be to minimize the surface area of the column as the oil rises from blow-out point through the water column and then spreads. The assumption is that most of the exchange occurs in the surface layer as it spreads.

John's comment is that he is not sure he accepts that.

DR. KRAEUTER: Particularly concerning the potential effects on--

DR. RAY: What did the data from the Ixtoc study show, the water work, because they did a lot of water column profiling for composition and concentration? It has been too long since I have looked at it.

DR. TEAL: It's been a long time since I looked at it, too. What Jerry said here this afternoon, when he was doing all this stuff, was something to the effect that there was relatively little of that oil that got into the water.

I can remember seeing the contours of oil in the water. It seems to me they were parts per billion, downstream.

DR. AURAND: It was the last thing he addressed this afternoon. The three things I wrote down was that the surface, no mechanism that he knew of to transport oil to the bottom in any quantity. I'm not sure that addresses the issue of the oil that starts at the bottom and comes up to the water column. At Ixtoc, there was not much oil on the bottom.

DR. TEAL: There was very little oil on the bottom at Ixtoc and there wasn't a whole lot in the water. Most of it did go to the surface and spread out over the surface area.

DR. BUTMAN: How deep was the water at Ixtoc?

DR. RAY: A little over 200 feet. The big well head was about 180 feet or something like that, as I recall.

DR. TEAL: It was a little shallower, I thought. I suppose you'd probably expect a little more to be in the water. In the modeling we were talking about, they assumed that all the water got into only the top 10 meters of the water. I thought that was a method to get a worst case scenario, net dilution of the oil by confining it to the top ten meters.

MS. HUGHES: What about if we had a blow-out so that it would be from the bottom moving up, what effect might that have on retaining the oil or retaining more oil in the water column coming up?

DR. TEAL: What effect would what have?

MS. HUGHES: This shelf/slope front, this frontal system on it?

DR. TEAL: What did you say?

MS. HUGHES: Would the shelf/slope front affect movement of oil through the water column if there were to be a blow-out at the edge of the shelf?

DR. BUTMAN: I guess the stratification of the water column definitely would have some effect, but my gut feeling is that the--

MS. HUGHES: Not any more than any other factor?

DR. BUTMAN: You'd have to look at the buoyancy of the oil versus the--I think it would probably be a minor effect.

DR. KRAEUTER: I'm not talking about the dissolved toxic fraction. I'm talking about if you've got a frontal system in there and these frontal systems tend to be areas of concentration, particularly in larger things like that, it just goes through that.

We've already talked about the recruitment problems and stuff, and I would expect something to happen in--

DR. BUTMAN: Also, the suspended sediment concentrations are certainly higher near the bottom, even on the shelf.

DR. GRASSLE: I didn't hear the models, but it seems to me that the model you really need to take into concentration are standing stock particles and, you know, since all the materials tend to be absorbed by the particles.

DR. TEAL: The model business is a different issue.

DR. GRASSLE: But, if you are trying to decide rates of accumulation, it is going to depend a lot on the standing stock particles.

DR. RAY: I'll throw one other thing in. The particular scenario we are talking about, you know, we always talk about probabilities of different incidents occurring. This one particular type of scenario is probably the least likely of all, because it is an exploration scenario with a blow-out preventer at the bottom that fails.

Other than the exploration phase where you actually have a blow-out right on the ocean floor, once you go to development drilling, you don't have that type of weak link in the system, you know. You are driving a conductor pipe 100, 200, 300 feet down into the ocean floor from the platform, and you have a continuous connection there.

You don't have a movable breakable joint like a blow-out preventer that's used in the exploratory drilling at the ocean floor, so just from perspective, you are dealing with the lowest probability of all the scenarios you can possibly deal with when you deal with a sub-surface blow-out.

DR. AURAND: Actually, if I remember what we have been presenting in Florida--and it has not been my job--I don't think we have had a blow-out on an exploratory rig.

DR. RAY: In the U.S., as far as I know, we have never had an exploratory blow-out.

DR. AURAND: We have never had an exploratory blow-out and there are something like in excess of 8,000 exploratory rigs in that data base. Even that is an extremely unlikely event, but--

DR. RAY: It's worth considering, but you need to put it in perspective with the other scenarios.

DR. KRAEUTER: I was bringing it up more for just discussion purposes, separating it from a spill.

DR. AURAND: Yes, and I think it is a worthwhile discussion as to whether you want to separate the two. The only thing I would tell everybody is that it is 5 minutes after 5:00.

DR. TEAL: One other thing. The National Academy Committee on Dispersements, the whole discussion of dispersements is to mix the oil with the water; that's the whole point of the dispersements.

I think it would be fair to say that the general conclusion was that in water as deep as 2 or 3 hundred meters, dispersing a spill into the water column was considered to be one of the best ways of diluting it rapidly to the point where it would have little effect.

If you argued from that, I don't know if this is right, but I'm just trying to argue from that, you might say that a blow-out is less likely to have as serious an effect as some of the oil is dispersed on the way up and therefore diluted, than if that same spill occurred right at the surface and spread out and was concentrated in the surface layer.

MS. HUGHES: In 200 meters of water, why do you want to disperse an oil spill into the water column?

DR. TEAL: The reason is to dilute it down to the point where it isn't toxic to the organisms.

MS. HUGHES: What is it doing at the surface? What is it doing toxicologically?

DR. TEAL: On a spill which is spread out along the surface, you get quite a bit of oil right along the surface, right underneath the slick, then at levels which are highly toxic.

The idea is to dilute it out into a large volume of water rapidly so that instead of having parts per million, you get it down to the low parts per million.

MS. HUGHES: The only reason I asked is that generally, you think of the use of dispersements in shallow, coastal areas. You want to get it into the water column so that you don't get large amounts of it on the beach.

DR. TEAL: That is the best place to use it.

MS. HUGHES: I have often heard that you don't want to put oil in the water column. You don't want to mix it into the water column where it then has a higher chance of attaching to particles and ending up getting incorporated into the sediment.

DR. TEAL: The point is--the whole question I'm trying to raise in your mind is the question of diluting it sufficiently by spreading it through a large volume of water.

MS. HUGHES: To dilute the toxicity.

DR. TEAL: Yes.

DR. KRAEUTER: We are only talking about a small part of the time that there is something like that. The toxic fraction is going to be evaporating very rapidly, anyhow.

DR. GRASSLE: I guess I picture if you have more mixing over a large area of the surface, you're going to get it on more particles and, therefore, have a greater potential of getting the material to the bottom.

I picture a blow-out at the bottom, I see that stuff as just shooting up and not going laterally much at all and getting above the bottom boundary layer pretty rapidly, and spreading out.

DR. RAY: Just to add on to what John said, there are a number of other reasons coming out in the whole dispersements argument as to the advantage of it.

One is that there is a suggestion that particulates are less susceptible to oil wetting if they have actually been dispersed and then the other key thing in a lot of the dispersements decisions is to dilute it below toxic levels as quickly as possible, because the more severe impacts that are perceived are what happens if the oil, as a surface slick, impacts on marine mammals and sea birds, and especially gets into shallow, sub-tidal, and inter-tidal areas. That is where the real measurable, significant impacts occur and that is all part of the overall picture as to the pros and cons of dispersing it into the water column.

DR. TEAL: For our argument here, those things are not a priority for us.

MS. HUGHES: That is correct.

DR. BOTHNER: With respect to this issue on submarine canyons, I think Jerry's comment this morning about not having a true mechanism for getting it to the bottom, except for Fred's comment regarding spring blooms, that is still the opinion of the group, I imagine.

DR. AURAND: I'm not sure you can say a whole lot about this, given the time and the information that you have, other than to address the issue of whether or not you think there is a way it can get into the canyon.

The rest of it depends so much on what you spilled and when you spilled it and how much of it you spilled that other than saying oil spills aren't a very good thing to do, I'm not sure what you can really say about it.

You can, perhaps, reach a consensus on what you think is largely a--if there is any reasonable mechanism to get it into the canyon.

DR. GRASSLE: All you can say is there is a potential for long-term accumulation. That's all you can do.

DR. TEAL: I think you can say there is probably more potential for getting it into the canyon than depositing it on the surface of the bank.

DR. GRASSLE: That's right.

DR. TEAL: You've got indications of sediment accumulation.

DR. GRASSLE: That's right. Doesn't Hudson Canyon show hydrocarbon concentrations are higher in the canyon?

DR. TEAL: That may be from the dump there, too.

DR. GRASSLE: It's from the dump, but the fact is it is higher in the canyon.

DR. KRAEUTER: I hesitate to try to interpret Jerry Neff's comment, as he and I talked about this mechanism of transport several times. Really, I think what he is saying is that we don't know of any. We know it gets down there, but we don't know what the mechanism is.

DR. TEAL: He said, I think, that he did not see any mechanism for transporting large quantities of oil. He didn't say he didn't know of the mechanism of transport to the bottom. He was talking about putting--I sort of think what he was saying was he didn't see how you could get great amounts of oil down to the bottom if you had a spill.

DR. RAY: I think that is probably what Jerry was meaning. There is sedimentation and the fecal pellet routine and a lot of these others, but there are methods to get large quantities down there, as in looking at an impact of a large oil spill, or even to get large quantities of hydrocarbons into the water column, per se, solubilized at depth like that, there just doesn't appear to be any evidence to suggest that that happens.

If you get down much below 20 or 30 meters in the water column, it is very hard to measure very high elevations of hydrocarbons below a slick. I think it is a question of quantity. I think there is no doubt that some of it is going to make it to the bottom.

DR. GRASSLE: What do you mean by the surface layer?

DR. AURAND: 10 meters. I'm just trying to paraphrase what you all are saying so that you can take this and change it however you want to change it.

What I heard was that there may be some mechanisms through krill, sedimentation, to get more of it in the canyons than would get on the slope benthic areas, but that most of it is going to go to the surface, whatever you want to define surface as meaning.

So, I would go on and say that most of your impacts are going to occur on organisms which are in that area.

DR. KRAEUTER: I would include the frontal system.

DR. AURAND: Where, up here?

DR. KRAEUTER: The surface layer, the frontal systems.

DR. AURAND: Frontal systems. There is a possibility of higher accumulations of hydrocarbons on canyons than on adjacent areas of the slope, e.g., krill, sedimentation; however, the major impacts would occur in the surface layer.

DR. BUTMAN: And on the shelf/slope water front.

DR. AURAND: And at the shelf/slope water front.

DR. RAY: Does that make any assumption of a sub-surface blow-out? A surface blow wouldn't affect that shelf/slope interface? Are you talking about at the surface?

DR. KRAEUTER: You've got a surface front.

DR. RAY: Okay.

DR. AURAND: I assumed from what I heard that you did not want to differentiate between these two.

DR. GRASSLE: I would just insert the words "short-term" between "major" and "impacts"--major short-term impacts.

DR. AURAND: There is no reason to differentiate between blow-outs and spills?

DR. GRASSLE: I guess I would prefer to have the phrase, in terms of the benthic environment, the concern would be about long-term accumulation and that says the same thing--to me, it does, anyway. In other words, it is--

DR. AURAND: The major short-term impacts would occur in the surface layer and at the shelf/slope water front.

DR. GRASSLE: Benthic impacts, if they occur, are likely to be the result of long accumulation from whatever source.

DR. AURAND: If they occur, are more likely to be long term?

DR. GRASSLE: Long term and from a variety of sources.

DR. VALENTINE: Are major impacts on the surface layer going to affect the canyon biota?

DR. GRASSLE: Not particularly.

DR. VALENTINE: Or is it more likely to affect the--it's on the bank, in general, right? I mean, the fish larvae on the bank in general would be a lot more impacted than the canyon biota.

DR. GRASSLE: I think the main effect people have talked about are for fish larvae in the very surface layer.

DR. AURAND: I don't know that I would restrict it to fish larvae but planktonic organs.

DR. COOPER: Fish and shellfish.

MS. HUGHES: I'd say that should include eggs. You'd best add crustaceans, so that we take in our important ones.

DR. COOPER: Fish and shellfish.

MS. HUGHES: I was separating them.

MR. VILD: What about birds?

DR. COOPER: But that's not canyons.

MR. VILD: Don't certain birds congregate at the heads of canyons, also?

MS. HUGHES: At the front, along the edge.

DR. AURAND: I think maybe we need to go back and address only impacts to canyons. I don't think you want to try to get into defining all oil spill impacts.

MR. VILD: I was going on the assumption that birds congregate on the canyon--

DR. AURAND: I don't want anybody to think that you are defining all oil spill impacts. That's a good point, but the major canyon impact would be on eggs and larvae which can include commercial benthic species.

MR. LANE: Do we want to define species that we know have surface eggs and larvae during some critical life stage?

DR. KRAEUTER: Actually, maybe we should leave out the benthic and commercial species.

DR. AURAND: Associated with canyons?

DR. KRAEUTER: Yes.

DR. AURAND: I don't think you could quantify that. That's where the impact would be, but nobody has any idea what, because it is too situationally dependent.

DR. GRASSLE: Why don't you say surface spills would impact canyon species only through the effect on planktonic larvae?

DR. AURAND: Surface spills--

DR. GRASSLE: Surface impacts, sorry, would be limited to effects on planktonic larvae.

DR. AURAND: Surface impacts would be limited to the effects on planktonic larvae.

DR. GRASSLE: Of canyon species.

DR. AURAND: Of canyon species.

DR. GRASSLE: Of canyon fauna, especially.

DR. AURAND: Yes.

DR. GRASSLE: I think that's better.

DR. AURAND: The magnitude of the impact is situationally dependent.

Does anybody want to say anything else about that? I certainly do not want to predispose myself to citing the oil spill impacts.

DR. BOTHNER: Fred, regarding your statement on produced waters, this is a lot milder statement for an effect that is potentially a lot greater.

DR. GRASSLE: Actually, no. I thought in a sense that we decided that in this, to really in terms of potential effects, to limit both effects to the potential build-up over long periods of time on the model.

We actually made a statement that we thought the other would be less than this.

DR. BOTHNER: Right, but that statement probably should come at the end of this, rather than at the end of waters.

DR. GRASSLE: Maybe.

DR. AURAND: It is much more likely that such hydrocarbon build-ups would be related to accidental, small spills. Is that the one?

DR. BOTHNER: That's the one I was thinking of, the one about benthic effects are--

MS. HUGHES: Not expected.

DR. AURAND: Benthic effects are not expected, but gradual accumulations of hydrocarbons on the bottom has been shown in shallow water.

DR. TEAL: That's related to production.

DR. BOTHNER: Right. I was just thinking a statement that recognizes the impact on the bottom, if it is a mild effect expected here, perhaps it ought to be mentioned after the oil spill/blow-out chapter of this story rather than after the production water for hydrocarbon accumulation on the bottom sediments.

DR. AURAND: I don't know. I think the situation is somewhat different. This is a discharge that continues over a long period of time. This goes on and on and on, whereas the oil spill, hopefully, won't go on for long periods of time.

DR. GRASSLE: I guess all modifiers of spills seem less relevant in terms of our generic category later on. You say we'd catch that in the editing, anyway. As I see this now, the "accidental" and "small" could be deleted, since there is a whole category later on.

In other words, you are referring to a category later on, rather than a--

DR. AURAND: It is more likely that such hydrocarbon build-up would be related to spills.

DR. GRASSLE: Yes, referring to this later category rather than to something more specific.

DR. KRAEUTER: See spills/blow-outs, progressing to that category.

DR. RAY: With regards to your "shallow water" comment, Don, you know, in the 9 feet of water, you know, out to about 100 meters, you were able to measure hydrocarbons in sediments; in 35 feet of water, you were down to near 20 meters.

In the Buccaneer Field [correct name?] study in 70 some feet of water, you were having a hard time saying anything about produced water contamination in the bottom sediment, so it appears with depth, you are very quickly getting away from it.

DR. AURAND: Yes. Personally, I wouldn't expect to see anything here, but it's never been looked at that I know of in these water depths, so--

DR. RAY: You're right.

DR. AURAND:--it's possible that right near the platform, you could find some. It would probably depend on the density of the plume that came out, for how far it might get.

DR. RAY: Even in the case of the density due to salinity, you get 200 meters below and that salinity is going to be background to sea water salinity wherever it exceeds 200 meters. We see it get--even in shallower waters, we don't see much of a salinity with distance.

DR. AURAND: On gas condensate blow-outs, unless you want to say something else about oil, I know of one paper which I think actually was sent to you, Pat, at one point about one study of a gas blow-out in the North Sea. That is the only one I know of and the impacts there were not all that great.

MS. HUGHES: There are a couple of studies available, done in Canada.

DR. AURAND: There was one in the North Sea.

MS. HUGHES: One was done by, I think, Mobil Canada.

DR. AURAND: On gas blow-outs?

MS. HUGHES: Yes. Actually, there were at least two--I haven't seen them in the scientific literature--two reports of studies done on gas blow-outs in Canadian waters, one I want to say in the Arctic and the other in the Canadian Atlantic. I have, I think, both of them somewhere.

DR. AURAND: My interpretation of what little I do know is that the impacts from gas blow-out were much less severe, obviously, than oil spills.

MS. HUGHES: The only reason I remember raising it in looking at the draft EIS was, if I remember correctly, the measurements that were done in the Canadian Atlantic, they were finding somewhere between 5 and 10 parts per million total hydrocarbon concentrations below the surface, almost in a subsurface plume. I will find the reports.

DR. AURAND: Does anybody really want to even say anything about these, since we don't have anyone here to talk to them?

MS. HUGHES: One question, since there hasn't been very much work done on them, I have the feeling the answer is no.

DR. AURAND: There haven't been that many gas blow-outs that I know of that have been studied. I think there was one paper. I think we sent you a copy.

MS. HUGHES: You did. I have two reports on Canadian work.

DR. AURAND: So, there are three pieces of information.

DR. RAY: Most of your gas blow-outs that occur down there occur on the platform itself, so you have a combination of sand, rocks, gas, and water getting blown up into the air. Very few of the situations actually happen subsurface where you are injecting high volumes of gas directly into the water.

I don't know of--I haven't seen any of the studies on that that have been done related to a gas blow-out, but as I say, most of them are on the platform itself with the exception of shallow gas blow-outs where you have ruptures totally outside the casing and everything else.

DR. AURAND: Does anybody want to say anything about these? In my mind, a gas blow-out has less of a significant impact than an oil spill, but I don't even know if you want to say that. Does anybody have an opinion or do you just want to pass?

DR. GRASSLE: Pass.

MS. HUGHES: Just say no comment due to insufficient information.

MR. VILD: That way, it is at least reported that we looked at it.

DR. AURAND: The next thing down there is space-use conflicts, and Jim and I talked a little bit about this. I think it is a given that space-use conflicts will occur inside the platform. That's just that they exist.

DR. TEAL: This doesn't apply particularly to canyonheads since we are starting on the basis that there won't be anything within 500 meters of the canyon head.

DR. GRASSLE: I would add that to our original rationale for the fact that the canyonheads would minimize space-use conflicts.

MR. LANE: I assume there are going to be no discharges for 500 meters, the platforms will be outside that.

DR. AURAND: Anchor chains?

DR. COOPER: You may have an anchor or two.

DR. RAY: What kind of fishing gear are they using out there, Dick?

DR. COOPER: You'll have a fair amount of autotrawling out to about 200 to 300 meters.

DR. RAY: How close to the canyons do they get?

DR. COOPER: In the canyons, it's entirely on line bait and hook gear for swordfish, tile fish, and deep-sea traps for lobsters.

DR. RAY: On the ottertrawling, how close do they get to the canyons?

DR. COOPER: On the ottertrawling, they go right to the edge of the canyons, right up to the rim.

DR. KRAEUTER: On the long lines or the trap lines, those are multiple traps on the line.

DR. COOPER: Yes.

DR. KRAEUTER: So, there is a potential for entanglement on the anchor lines, then, as opposed to a single trap.

DR. COOPER: These fishermen are overlaying their lines all the time. That goes on between individual fishing vessels.

DR. RAY: They could set traps all day long between those anchors and never have a problem, as long as they don't lay them across them. That might be logical to figure there is a chain between a buoy and the platform, though.

DR. COOPER: The other side of this coin, though, is it is probably good human relations politics for the oil companies, if they ever do set up out there, to have a few anchors out to give the fishermen who lose a half a million dollars of gear a year to put in a claim.

DR. RAY: Thanks.

MS. HUGHES: The other issue about space-use conflicts is one that I know at least the Atlantic Off-Shore Fishermen's Association brings up regularly when talking about this area of the North Atlantic is they fish here because this is where there are concentrations of the species that they are looking for, number one.

Oftentimes, you will hear the argument, "Oh, well, there are other places that they can go." It is fierce competition for a small amount of space, whether it is among fishermen or between fishermen and other industries.

I think the other thing that is important to point out is that we do often think that they are just being excluded from the area that is marked by the anchors. In fact, a lot of fixed gear is set with the tide or along isobaths or whatever, so that oftentimes, their grounds--that's the only way I can put it.

Depending on the placement of a rig, a much larger area of fishing ground could be excluded to them because of its location, not just the area that the rig and the anchors take up. It's actually that it interferes with the patterns in which they set their gear in that area.

DR. TEAL: Is this specifically related to the canyons? This is a general problem.

MS. HUGHES: Well, it would be specifically related to the canyons in that this is an area where at least fixed gear is set for--

DR. TEAL: In the canyonheads, yes.

MS. HUGHES: Yes, in the canyonheads, not only in the canyonheads, but also along it.

DR. BUTMAN: Along that same line, it seems like one rig along the margin of the canyon if it has--did we say 1 mile or 2 miles worth of anchors?

DR. TEAL: A mile each way.

DR. BUTMAN: So it's 2 miles or 4 kilometers. The canyon only goes 10 or 15 kilometers into the continental shelf, so that's 25 percent of a 200-meter isobath along one side of the canyon if you put one rig in. A 200-meter isobath is a very protected place.

MR. LANE: I am not sure what limitations we should place on the drilling operation unless you want to say no anchoring within the 500 meters.

DR. VALENTINE: The anchoring problem would be a temporary problem during exploration. If they ever went into production, then conceivably, they could come closer to the rigs if there were a pipeline or something.

DR. BUTMAN: My only point was that 4 kilometers is a large fraction of the area around the canyons.

DR. KRAEUTER: The oil companies and the fishermen could get together on how to place those anchors so as to create the least possible disruption to the way they set traps.

DR. RAY: In the cooperative meetings, for example, let's say they worked out the difference and the rig is going to go in there for a few months, as soon as they get the anchor problems worked out and know exactly where everything is, they can give notice to the fishermen as to exactly where they are and everything else.

Again, with long-line equipment, with traps, there's no reason in the world that I know of, unless there is some regulatory exclusion, why they couldn't have those things set inside those buoy markers.

DR. COOPER: That's exactly what they will do, as long as the anchors on the bottom are marked above with some kind of a proper buoy, they will set their trawl gear right in there.

DR. RAY: One of the benefits of a cooperative thing like that, also, is that the industry sends out notice to the fishermen saying, "Hey, 2 weeks from today, we are planning to go out and pull anchors." They will be notified, you know, so that if you've got some of your traps or something near our anchor chains, it probably wouldn't be a bad idea to move them.

It's that communications thing again which minimizes claims and everything else and ill feelings. That's something that can be worked out.

MS. HUGHES: That has been going on. There have been a couple of seismic permits, one to Texas A&M University and one not so long ago and that was worked out.

DR. AURAND: Let's try this: Space-use conflicts in the canyons themselves are minimized by the 500 meter set-back. They would occur around the platform and the anchor lines. This could include a large fraction of preferred fishing area near a given canyon. Some accommodation could be achieved by industry-to-industry coordination.

Does that capture everything?

DR. COOPER: Jim, do your rigs ever provide cold beer for these boats?

DR. TEAL: They never put cold beer on the rig.

DR. RAY: Unfortunately, we don't do it for our own workers. We've got enough trouble with those guys without giving them a few beers.

DR. COOPER: You would be surprised at how much cooperation that stimulates.

DR. BUTMAN: Do you ever give lobster back to the rigs, Dick?

DR. COOPER: We've traded buckets full of lobsters for a few cold six packs of beer many times or maybe a submarine dive or two. There are a lot of pretty neat ways to cooperate with these guys.

(Laughter)

DR. AURAND: The remaining item on the list is noise.

MS. HUGHES: Mike.

DR. BOTHNER: I thought it was interesting to bring it up. I've got no information, so we can't pursue it.

MR. LANE: No comment.

DR. AURAND: Do you want to put "no comment" or do you want to just quietly erase it?

DR. BOTHNER: Cross it out.

DR. AURAND: Hearing no objection, okay.

A couple of things occurred to us while we were at the break. John, I think you want to get a couple of your cronies together and maybe a copy of this, and try to do your back of the envelope calculation again so that at least there is some documentation that the concentrations that you came up with were somewhat close.

There are no numbers in here, but I just think for your own sake, you might want to run through it again. Actually, that's up to you, I suppose. The last time we did this at the last Florida Task Force meeting, we had an order of magnitude difference in our separate answers, and it turned out it was a missed decimal point and the benefit was to us, so I didn't mind the mistake. I had it way too high.

It would be awfully easy to make a mistake like that. You probably would want to sit down and see if you can find some numbers in here for calculation to see if it is the right order of magnitude.

The other thing is we will type all of this stuff up tomorrow morning. Those of us who are still here tomorrow morning will look at the whole package and make whatever corrections we think need to be made.

We still have two things. If there are any definite needs that you see that relate to your being comfortable with these conclusions, we should probably try to list them.

If there are any minority opinions that anybody wants to append to this, I suppose you could write them in the privacy of your own room or you could bring them up for public discussion and have more than one person involved as a minority opinion. But we would include either one of those things, as individuals or as a group, if you think that there are things that you want to add.

Of course, our biggest concern was getting through this part of it, but I think it is only fair to get all of that stuff in, too. Anyway, while you are thinking about that, we will all be back in here tomorrow to look at what they type up and then it will all go out to everyone.

Jim and I have--it is the first time we have ever tried to do this and our roles have suddenly changed from what we thought they were going to be. If anybody has any suggestions whatsoever about the way this worked out and whether or not some improvements in our technique are needed, it is a useful way to approach the dissemination of information and formation of some kind of consensus on issues, we would love to hear it.

We had some of our own ideas. For example, I think it is very clear that we should have had some select few documents here for reference. We got hung up on a couple of points on oil spills, for example, that if I had my copy of "Oil and Sea" here, somebody could have gone off and looked up some numbers and we would have been a lot better off.

This is one thing that we have already seen, that there should be some people around with some basic reference material to pull out some information. If there are any format things or ways it was handled that you think could be improved, we would like to hear them.

DR. RAY: Just one quick observation, Don, and that is that the next time you do this, whatever the subject may be, not only do you lock people in for the set period of time that they've agreed to come--of course, I'm guilty because I'm leaving in the morning.

Not only that, but at the minimum, stick to the timeframe you've got, because in trying to hurry this thing up, I think we found, the way the discussions went today, there probably could have been a lot more discussion.

We kind of rushed into this phase and now you are losing a lot of people. It would have been nice to have a little bit more time to explore some of these issues and discuss them a little further and not be trying to run on a time crunch.

You could probably very easily have spent a little bit more time on some of these topic areas today to be sure all the ideas came out and everybody was really satisfied with them, and then we would have had time tomorrow to work towards the conclusions.

I would just say I think you need a little bit more time when you do one of these types of things.

DR. AURAND: I think we also needed a little bit more up-front discussion of certain topics, such as formation waters. We didn't know exactly where this was going to go when we started, but there were some topics where we clearly, as a group, could have used a little more information to work from--hand-outs or something, numbers where we could have begun our calculations. I think that's pretty clear.

DR. KRAEUTER: I think the important thing there is you are going to have to emphasize up front and re-emphasize and re-emphasize if you want everybody here for the entire time, because we always tend to cut it short at the end and say, oh, we can get it done. That last day tends to disappear into a half a day. Here, a half-day disappeared entirely.

I think you are going to have to call people and talk to them and tell them exactly what you are trying to do, so that everybody understands how long it is really going to take.

DR. AURAND: We did try to do that.

DR. KRAEUTER: You tried, but you are going to have to double or triple your efforts to be sure that people understand that particular aspect.

DR. AURAND: The other way is to get more redundancy in the attendees, but then that increases the size of the meetings, which is not necessarily--it is hard to manage.

DR. TEAL: I don't think there is much you can do about those problems.

DR. AURAND: John, we could use the approach we do at the Scientific Committee which is fake the last day's agenda.

DR. VALENTINE: Well, in a way, I am glad the day went as it did because I think a combined discussion from these Panels A and B was much more productive than it would have been separately.

DR. BUTMAN: I think it is also important to have a leader for the discussion. I think you filled that role very well this afternoon.

DR. AURAND: You did a nice job this morning.

DR. BUTMAN: I think you almost need to nail people ahead of time, and say, "We need you to lead," to have a little bit better idea ahead of time and have discussion leaders that will push that, because the group just can't provide its own direction.

DR. AURAND: What do you think about professional facilitators in that regard? There are people who do that for a living, drive meetings along.

DR. GRASSLE: I think it's much better to have somebody knowledgeable.

DR. TEAL: I think he was better than a professional facilitator and you were better.

MS. HUGHES: You want someone who is familiar with the subject.

DR. TEAL: Brad's idea of designating someone, I mean, he, I think, is saying he would have been a little happier had he known.

DR. BUTMAN: I was sort of designated because I was a discussion leader. I think if you lined people up ahead of time and you need to find people who are really interested in the problem and who are willing to work and spend the time to do it.

DR. AURAND: Clarify what their job is. Your job is to come up with, and exactly what that is. I wasn't expecting to do this, either.

DR. KRAEUTER: I agree that you need people who are professionally trained at things like this, rather than facilitators, because if they don't understand where things are going and they don't understand the field, the whole thing falls apart and the scientific community gets disgusted and walks away because the facilitator doesn't understand the fundamental errors in trying to drive the discussion in a way that people don't need to go or they don't know when the consensus has been reached.

DR. GRASSLE: One thing you might do for the introductory material is ask each person in each area to Xerox a few pages of relevant material so that people don't have to have everything in the talk, everything in the abstract or whatever, don't have to write anything new.

Usually, there are a few very relevant pages of a report that would pertain to some of the basic issues.

DR. BUTMAN: Also, this is one of the few meetings I've ever been at where they've had everything recorded verbatim. I'm not sure what the use of that in the long term is going to be, but it's different than a free exchange.

In some ways, I guess it doesn't really limit your discussion, but it makes you think twice about what you say. It seems like--

DR. TEAL: You get used to it very quickly, Brad. Scientific committees are always recorded, every word, the whole thing.

DR. BUTMAN: It never comes back to haunt you?

DR. AURAND: Most people never read it.

DR. BUTMAN: So, why do it? Is it actually ever used?

DR. TEAL: To produce a summary, yes.

DR. AURAND: We do have the option, in the case of some possible misinterpretation of the conclusions or a disagreement over the consensus, it is conceivable, but I think very unlikely--sort of like an oil spill--that we would ever go back into the transcript to try to straighten it out.

If it came to that, you would have at least a record of what everybody said, and you can try to figure out if a mistake had been made and that's really what it is for. I don't know that it was necessary.

We have never done this before. I suppose one of the things you could comment on is whether or not you think it is useful to have it recorded or potentially useful. It is possible to use it to work on comments back on the draft and things like that, too.

DR. COOPER: I'd like to make a comment here. I myself personally found these two days very productive and very stimulating. I am especially pleased, having been a Fisheries employee when a lot of our canyon work was done, to see our input play a major role in some of this decision making. Frequently, that has not been the case.

DR. AURAND: Does anyone wish to have discussions at this time about needs? This doesn't have to be studies needs. There could be--I don't know what you might think of that you do need in terms of evaluating canyon impacts, but this is an opportunity to put it into a document where it would get some dissemination, and the same thing with minority opinions.

Is there any discussion of either of those points before we break? The opportunity exists to either come tomorrow morning or give it to someone who is coming tomorrow morning and get it included in the report. Certainly, you don't have to do it now.

But if there is anything that anybody can think of that they wish to discuss?

DR. BOTHNER: Will the needs take the form of, for example, research needs, the things that we would need to--

DR. AURAND: I would like to see it approached from the point of view of if there are portions of these conclusions which you are uncomfortable with because of a research need, it would be appropriate, I think, to explain what that research need is and how it would address the issue, going back to my original statement about when should you do it and what is the issue to address and all those kinds of things.

Yes, I think that would be all right and that would be useful.

DR. KRAEUTER: I think along those lines, one thing that we seemed to get most hung up on was the rates of accumulation or the mass balances of things. It seems if we had that, we might have been willing to make more definitive statements concerning what was going on. I don't know how everybody else feels, but that is one thing I see.

DR. BUTMAN: Yes, I think rates and net transport directions.

DR. KRAEUTER: And transport directions.

DR. AURAND: On that last point, I have to tell you that the way this thing originated in the first place was a discussion between Jim and I that, "Gee, if we could only decide that canyons were erosional, we could probably figure out where it was okay or not okay to lease."

I said, "But, Jim, we have one that appears to be erosional and one that appears to be depositional and we don't know what to do in those, so why don't we have some kind of a meeting where we discuss what the implications of that are and see if we can resolve it for those two areas and then we'll decide?"

The first thing that happened yesterday was, "Well, some places are erosional and some places are depositional." So, I'm not sure that that is something that we can really ever really resolve completely.

DR. GRASSLE: I think flux of materials in that, too--rates of accumulation and flux of materials. I guess that is implied in the rates of accumulation.

DR. AURAND: Let's see, rates of accumulation, flux of materials, and deposition are poorly known and this inhibits ability to--to what?

DR. GRASSLE: Make definite conclusions.

DR. AURAND: Hence, the qualifiers. Anything else? Any minority opinions? Does anyone want to say anything about the process, for the record?

DR. VALENTINE: I'd like to talk about a need. In our discussion of the characteristics of canyons, I think it started to become clear that some of these features that are called canyons out there might not fit the criteria of what we think of as canyons, based on their shape and their contained biota. Some of them may be more like the upper slope and shelf edge.

We really don't have much information on these small canyons. Very few have even been sampled, so conceivably, if we could obtain more information, some of these could be removed from the canyon exclusions list. We can talk more about that tomorrow.

DR. GRASSLE: Some of the gullies may be the most depositional sites out there.

EVENING SESSION

(6:00 p.m.)

DR. VALENTINE: When we are talking about the biota of the canyon, the heterogeneity of the canyon as the main reason for it, some of these areas seem to be very homogeneous, like the upper slope and really don't support all these attached organisms.

DR. AURAND: I suppose it would be appropriate to mention that there are still sections, just on the descriptive information on the canyons, that have to be written and turned in, so that we can have those typed up tomorrow, too.

The material that was to be written just on the descriptions of the canyons, the stuff that we worked on this morning is being typed now and will be available in the morning.

DR. BOTHNER: You are talking about the 100-word paragraphs?

DR. AURAND: The 100-word paragraphs. Do they have all of that?

DR. COOPER: The ones that Barbara and I are putting together, I will take over what she started. I'll have those done this evening if they are not done yet.

MR. LANE: Has everyone reviewed their presentation material?

DR. MILLER: Let me ask one question. Has everyone prepared their written statements for the 100-word paragraphs that they want to have included?

DR. AURAND: From this morning's discussions?

DR. MILLER: From this morning's discussions.

DR. BOTHNER: We've been here all day. It's hard to write something.

DR. MACIOLEK: Barbara and Dick worked on species diversity.

DR. MILLER: That's what has got to be accomplished in the morning. We need to have that material put together.

DR. AURAND: Either tonight or in the morning.

DR. BUTMAN: I want to make one other comment on needs. We talked about--I'm not sure you can directly relate this to specific questions, but it came up several times that we only really have information on one or two canyons and, for those, only for the heads. We haven't really talked about the deeper parts.

DR. GRASSLE: You are thinking of the physical attributes?

DR. BUTMAN: Yes.

DR. GRASSLE: The physical information.

DR. COOPER: I just want to say from the fisheries and habitat types, we have got a lot more.

DR. GRASSLE: Right.

DR. BUTMAN: Even then, only primarily at the head. They have a little more information deeper.

DR. AURAND: This makes extrapolation difficult or do you just want to leave it at that?

DR. BUTMAN: I think that--yes, I think that limits our ability to predict effects on the deeper parts of the canyon.

DR. AURAND: You obviously have drawn some conclusions, so while it may limit your ability--

MR. LANE: Remember, the purpose of this was to look at biological impact and most of the biota concerned are within a 2 to 3 hundred meter depth; I guess that's why these conclusions were drawn.

DR. BUTMAN: I don't think that Barbara would say that at all; I think it's throughout the whole canyon.

DR. BOTHNER: The other reason for looking deeper in the canyon would be to see if the processes of, say, accumulation in the canyons also translates to periodic accumulations downslope for some reason.

We don't find much accumulation and we don't expect much accumulation in the head of the canyon, but there may be some further down. That would be a reason for looking deeper in the canyons to look at the mass balance of sediments; that would be very useful in understanding the whole system.

DR. COOPER: What is MMS' desire here? Are you interested, as we are, in the entire canyon or are you just interested in the commercial species?

DR. AURAND: I am not quite sure how to answer that. We are interested in the system and any possible impact we might have on it.

Clearly, a lot of our concern is generated by the commercial species because those are the ones where we run into problems, but I don't think we want--I would never say that we just wanted to study commercial species because you can't understand what is going on if all you are going to look at is two or three commercial species.

I would say we probably would not be interested in spreading out geographically all over the place. What we want to do is understand what is going on in the areas where we are likely to have an oil and gas impact.

We do want to try to understand what the processes are that are influencing the system. Tomorrow morning? I've had enough. You can all get together, you can caucus tonight and come back with a minority opinion if you want to, but remember, there aren't going to be as many people here tomorrow.

(Whereupon, the conference was adjourned at 6:07 o'clock p.m.)

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DAY 3--THURSDAY, FEBRUARY 9, 1989
MORNING SESSION

DR. BOTHNER: The outline is pretty much like that sheet that is pasted to the wall, right? That is the document we end up with?

DR. MACIOLEK: That is what starts, in my opinion, on page 20. What comes before that is a summary of what we talked about in the morning that led up to what we put down on paper in the afternoon.

My first question is, do we keep this first part as a summary of that discussion? Do we say we will incorporate the important points into the second section? Do you want this to show how we led up to some of the points that were presented in the second section?

I guess I see this second section as the product of the group, and the first few pages essentially back it up or remind us of how we got there.

DR. TEAL: I agree with you. I think from page 20 on is what is the final report, so that is pretty much--

DR. MACIOLEK: So, John, what is your opinion as to what we do with the first 19 pages?

DR. TEAL: I would just as soon forget it, but--

DR. MACIOLEK: Fortunately the editor agrees with us--not that we forget it, but that the second part is important.

DR. TEAL:--it is part of the record of the conference, but this is the thing, from page 20 on--that is important.

DR. GRASSLE: Is it going to be in the report or not--that is the question. Is it?

DR. MACIOLEK: The first piece?

DR. MILLER: Yes, it has got to be in the report.

DR. AURAND: There is a place on page 20 there where it says, "25 word--100-word paragraph insert here."

I think some of what is in the first 19 pages goes in there--I think--some goes in that spot.

DR. MACIOLEK: What is in the first part says that we ought to prepare 100-word paragraphs. That is less important than presenting those paragraphs.

DR. TEAL: Oh, yes, that part, of course, gets crossed out.

DR. AURAND: John, what you are saying is that you save the part that relates to the 100-word paragraphs and put it into that location?

DR. TEAL: Yes, sure. But there is a lot of it that in our discussion. Virtually everything I said is directed toward what the report should say, not as part of the report.

DR. MACIOLEK: Right. If what you asked us for as a final report is this statement--now, you have records--the verbatim transcript and the tapes.

Now, if you want, you will also have this written copy, but I think that, instead of editing this that extensively, we should worry more about our final document here.

DR. VALENTINE: Are we going to lift out a few pieces from the first 19 pages and put them in there?

DR. MACIOLEK: It seems appropriate--

DR. GRASSLE: There is really a lot more information in the first 14 pages. It is from about 15 to 19 that to me is a little vague about what is being said.

DR. MILLER: This represented your morning discussion--

DR. MACIOLEK: Right. We know that.

DR. MILLER:--and the basis for the conclusions that were reached on pages 20 through 29.

DR. MACIOLEK: If what you want from us is a statement of our conclusions, this is very distracting to read as the first part of that document.

DR. MILLER: I think it needs to be reorganized in some kind of form, in such a way that is in the record.

This is your conclusion, right here, starting on page 20, but this needs to be reorganized back into the document so that there is a record of what was said and how you reached those conclusions.

DR. MACIOLEK: Okay, so--

DR. AURAND: It needs to be presented as a discussion. If there are parts in there which are factual that you want to put in as part of--"This is what we think about canyons"--I think I agree with you that something that looks like a transcript of discussion is very distracting.

DR. GRASSLE: That is correct.

DR. AURAND: If you have paragraphs that you want to list something that you know about canyons or geology or whatever and put them in the right place, that makes sense.

DR. MACIOLEK: I think that sort of material probably would go in the first section. It may be that we end up with more than 100-word paragraphs if some of that information is especially relevant, but it just should be presented as--you know, here it is.

DR. TEAL: The biggest problem I see in that first part is--actually, really it is questions at about page 14--that there are not good answers to those questions.

You know, to try to summarize from this just briefly, for somebody who was not actually at the discussion, which is the case for me--I could not see what anyone was driving at in the next few pages.

DR. MACIOLEK: That is because it was just before lunch and no one was coherent.

DR. GRASSLE: Yes, it just does not make any sense to me.

DR. KRAEUTER: (Inaudible) discussion groups--

DR. TEAL: We cannot--there is no information, no answer to most of those questions.

DR. GRASSLE: Maybe we should just say that it was discussed.

DR. TEAL: You could say that it needs--it would be useful to know whether those topics--whether sessile animals in the canyons are separate populations.

I mean, Barbara sort of leads toward the idea that they are relatively isolated. I think that is highly unlikely, but she knows more about it than I do.

DR. GRASSLE: I guess what I am saying is that, if we use 14 through 19, it really should be reduced to about a page of comments that are really pertinent.

DR. MACIOLEK: Do you think we should go through this first section to see what we want to use in this final statement? We had some things to say about these tables?

DR. GRASSLE: Oh, yes--to me the tables are very hard to understand, too, because the question is stated clearly and then the line-up of things that are relevant to the question are such in such cryptic form that it is almost impossible to know what people were thinking of.

I guess everybody who is still here should try to put those into sentences--you know, a sentence that could be answered "yes" or "no."

DR. VALENTINE: I think it would be helpful in the "Characteristics" section if we had some kind of overall definition of what we are talking about physically--in other words, are we talking about the canyons from the heads down to what I generally focus on, from the head down to what I call the mouth--where it crosses the shelf edge, say, at two hundred meters.

DR. MACIOLEK: In other words, a definition--

DR. VALENTINE: Some canyons are very deep, you know, like Oceanographer, like--1300 meters deep at that point. Those are shallow.

DR. GRASSLE: My understanding was that we were considering canyons including deep areas, right? Otherwise, we--

DR. MACIOLEK: Most of the discussions were on the shallower--

DR. GRASSLE: That is the area that is going to be potentially closest to the drilling activity, unless they start drilling out in deeper water. So I think we might want to clarify that one way or the other.

I guess I think there should be two definitions. I think that right at the front of the report there should be a definition of a submarine canyon. That should be right on the second page of the report, because that says what all the part one stuff was about. That included a lot of consideration of the deeper parts.

DR. MACIOLEK: The presentation--
DR. GRASSLE: The first day things included a lot of information about the deeper parts of canyons.
I think it is okay to preface the second day discussion by saying that we decided to focus on the upper parts of canyons, and define what that is.
DR. MACIOLEK: It certainly makes sense to have a statement up front defining what it is we are talking about.
DR. GRASSLE: Yes. I think we have to have the definition of "canyon" up front, because that is the point all through here.
Page, are you going to write that?
DR. MACIOLEK: He suggested it. He can take a crack at it.
DR. GRASSLE: I think you should write both definitions, yes.
I guess I would go below two hundred meters for the second part of the discussion. A lot of what Dick is talking about is quite a lot deeper than that.
DR. VALENTINE: What I mean is where the canyon crosses the shelf break--where it crosses the 200-meter isobath layer. Some of them are quite deep at that point. Is that what you were referring to?
I had the impression you were talking about a maximum water depth of 200 meters. I mean, if you took a vertical plane through the 100-meter isobath on the continental slope, where the canyon intersects that, that is the part I am--
DR. GRASSLE: There are a whole lot of canyons I do not immediately visualize--that is my problem with that.
DR. VALENTINE: Otherwise, some of these canyons go out endlessly on the continental slope.
DR. MACIOLEK: It would probably be useful to have an illustration--a generalized illustration--but I think it is going to have to--
DR. GRASSLE: Maybe what we could do is just do the best you can with this in terms of--so long as we include the upper parts of canyons.
Maybe you could just add a phrase that says that. Mostly this would just include--why don't you just give examples for specific

canyons as to what depth that will work out to for specific canyons. Your definition seems fine, but give people something that makes it concrete.

DR. VALENTINE: I mean, in this diagram here--this for the 200-meter isobath--

DR. GRASSLE: I understand, but it just--that's good.

DR. MACIOLEK: While we are talking about defining things or that first part there, on page 3 was the list that Brad had come up with that I thought we were using as the subjects of our 100-word paragraphs, if you will.

As far as I know, we do not have anything on several of those topics. I just wanted to find out where we were with them.

We have got something on topography from Page--there is something on resuspension of sediments from Mike--something on species diversity and density from Dick Cooper and Barbara that Fred and I will add a little bit into.

There is something on the fisheries, but we do not have anything on current sediment texture--

DR. VALENTINE: Yes, we have volumes on sediment texture, if you have read it.

DR. MACIOLEK: Oh, so you have combined the two, then. Okay. About a one-to-ten inventory.

DR. BOTHNER: That is where I put that in. That is tied in with the resuspension. Yes, it is there.

DR. MACIOLEK: Oh, okay. You can tell I have not read these.

DR. TEAL: I think that Brad just has not gotten it in.

DR. MACIOLEK: Okay.

DR. GRASSLE: What does anyone think about the pages up to 14?

It seems to me that those maybe could be edited to make those tables reasonable, if we wanted it in tabular form. I mean, is it reasonable to--1 to 14 pages?

DR. MACIOLEK: You started at page 5?

DR. GRASSLE: Yes, starting on page 5 through 14.

DR. VALENTINE: Maybe if we went through this stuff now we could--

DR. GRASSLE: The problem is that there is a lot of--it is a lot of getting these things into sentences, which is really tough to do as a group.

DR. VALENTINE: I wanted to make some corrections, but I suppose--

DR. TEAL: Let's go over it and see what is correct and incorrect--where corrections need to be added.

Somebody will have to put it into sentences so that it is readily comprehensible, but we will not try to do that now. Red says he is going to do that, or has done it--so we leave that to him.

DR. GRASSLE: Sometimes it is very unclear, when you come to--

DR. TEAL: He was here during the discussions, so he has a pretty good idea of what it was that we were saying.

DR. BOTHNER: It may simplify the tables, too--Red, do you agree with this that we sort of came to the consensus that there was probably much, much of a need for designating direct or indirect for the evidence that led to the "yes" or "no" answer?

You remember, we had a discussion about that?

DR. WRIGHT: That is not true--I was not here yesterday morning.

DR. BOTHNER: Oh, well, somebody was.

DR. WRIGHT: Jude Wilber, who wrote this, was here, so I cannot respond to that question.

What I was saying is that I can--rapporteurs will undertake to put it into sentences and paragraphs--orderly sentence and paragraph form--the items and concepts that you want us to include.

DR. GRASSLE: What I would like to say is, wherever there is a "yes" or "no," I would like to see what the statement was that the "yes" or "no" applied to--and I do not think that is possible without having been there.

It says, "A fine grade of sediment accumulates," and then suddenly we see "textured bed forms--yes or (inaudible) no." What does that mean?

DR. VALENTINE: There is a mistake there in there, for one thing.

DR. AURAND: What it meant was, either that data supported or did not support the--

DR. GRASSLE: Okay, so that is what I am asking for--the data on texture and bed forms supports--I want to see that written down, because otherwise it is hopeless.

DR. TEAL: All right, but I think--of course I do that--

DR. GRASSLE: They could do that easily, other than if there are some mistakes.

DR. MACIOLEK: All right. Let's just go through it, starting with page 5. Does anyone have any questions?

DR. TEAL: I agree with Mike. I do not thing "indirect" or "direct" offers very much additional information.

DR. GRASSLE: Excuse me, would you speak very clearly, because these folks are going to be putting this into the draft form for you.

When you state something, we are going to put it in the way you wanted it stated, so make sure the words that you choose are careful and they way you want it done.

DR. BOTHNER: So maybe we ought to stay where we are on the page.

DR. MACIOLEK: Okay. Let's go to page 5, letter A. Does anyone--

DR. TEAL: I was supporting Mike in a general statement that the common label "D/I" I do not think is very useful to the general reader. I suggest that we leave it out.

DR. MACIOLEK: Does anyone have any objections to leaving out that column?

It seems as though we all agree, so we can strike the column labeled "D/I" from all of the tables?

Anything on--

DR. AURAND: This is out?

DR. GRASSLE: Yes.

DR. MACIOLEK: Does anyone have anything on item "A?" How about item "B" on page six?

DR. VALENTINE: Yes. I think "B" should read, "Fine grain sediment accumulates on canyon floors." That is where your samples were from, right, Mike--

DR. BOTHNER: Yes.

DR. VALENTINE:--over the last 2,000 years. Then, under "B," "texture and bed forms for Oceanographer Canyon" is "yes."

DR. BOTHNER: Oceanographer Canyon does not have much fine grain sediment.

DR. VALENTINE: Well, the reason that it is confusing is that capital "B" says that it accumulates on canyon floors, but--

DR. TEAL: Let's drop the sentence--why don't we make it a question? "Does fine grain sediment accumulate on canyon floors?"

DR. VALENTINE: All right, let's turn that around--yes, that is better.

DR. MACIOLEK: Actually, that is a good point. All of the statements, capital "A," capital "B," should be posed as questions. I mean, they should not be statements--they should be written as questions.

DR. VALENTINE: Yes, they should be written as questions.

Let's change that, then--that would read, "Does fine grain sediment accumulate on canyon floors?"--and then "Texture and bed forms--no for Oceanographer Canyon."

DR. BOTHNER: You might--do you want to restrict it to fine grain sediments? What would happen if you just said "sediments?" Then you would just say "coarse grain sediments seem to accumulate in Oceanographer Canyon."

DR. VALENTINE: That would be fine. The focus was on fine grain sediments because of the ones that--

DR. TEAL:--carry contaminants. I do not think we want to change it around.

We are not trying to be scientifically inclusive--we are trying to direct ourselves toward the effects.

DR. KRAEUTER: On "A" we really are talking about all things entering--the particles entering. Here we are talking about accumulation.

DR. VALENTINE: "Do particles--sand fines--enter canyons from the shelf?" That is how that would read.

DR. GRASSLE: Where are we now?

DR. KRAEUTER: Back on page five, item "A"--the first one we did.

DR. MILLER: Turn that into a question. Do you want to restate that as a question?

DR. TEAL: All of them.

DR. MACIOLEK: Yes.

MR. VILD: Is "fines" an acceptable word, or is that some colloquialism that one would change?

DR. VALENTINE: We could say "silt and clay."

MR. VILD: Well, what about just "fine-grained sediment?"--"fines," you know.

DR. VALENTINE: All right.

DR. GRASSLE: Just as an example, for the rapporteurs, let's state the question in little "a" on page 5.

DR. BOTHNER: This is a suggestion, because fines are what actually was found on the Boston Harbor clean-up.

DR. GRASSLE: Now, little "a"--the question for little "a."

DR. TEAL: In all of those cases, does the evidence from the flow regime--

DR. GRASSLE:--and calculations?

DR. TEAL: You do not have to say "and calculations." Calculations based on the measured flows support this conclusion? Yes.

DR. GRASSLE: It is not a conclusion--it is a question.

DR. TEAL: What answer do they give? "Yes" or "no?"

DR. GRASSLE: Just as a guideline, let's just state a question once, so that it is easier for her.

DR. TEAL: Do the calculations based on--oh, I see. You would make it as a statement, then? The calculations based--

DR. VALENTINE: Why don't you say--can't you say, "Do particles enter canyons from the shelf based on--" and then list "A," "B," "C," "D."

DR. TEAL: Yes, you could do it that way. That's good.

DR. GRASSLE: I think we are going to run into trouble with that.

DR. TEAL: I think we can deal with it. I mean, you can try a couple of ways to look at it and see what makes sense.

DR. MACIOLEK: Okay, so we are back to item "B" again, page seven--"B" at the top of page seven. Item "C" on page seven?

Most of that table is not even filled in.

DR. TEAL: No, and the problem with this whole set under "C" is that the question as it is more or less stated there is, "Is there a potential for--"

Well, it is hard to say no to that, regardless of--

DR. MACIOLEK: How about, "Is there evidence of--"

DR. TEAL: I would rather say, "Is there evidence of--" because you have got evidence in the case of your lead-210 inventory and concentrations in the case of the vertical mixing in the sediments.

You know that there is sediment resuspension, and so there is definite evidence of that. In the hydrocarbon concentrations, the filter feeding pelletization concentrations in their tissues, the evidence is lacking.

DR. KRAEUTER: So you would have to say no for those, based on your question.

DR. TEAL: If the question is, "Is there evidence for it?" If the question is, "Is there potential?"--then I think the question is useless.

DR. VALENTINE: How about this, John. "Is there evidence for an enhanced potential for accumulation in the canyons compared to the continental slope? That is really the point of the--

DR. TEAL: That is right.

DR. BOTHNER: So we really want to put that relative sense in there.

DR. GRASSLE: So it now reads, "Is there evidence for enhanced contaminant accumulation in canyons in comparison with the adjacent slope?"

DR. BOTHNER: Yes, except that you left out the word "potential."

DR. GRASSLE: Actually, I think I like John's point earlier, that "potential" does not--

DR. TEAL: Okay, "enhanced" relative to is better than "potential," because I think John is right.

DR. GRASSLE: I do not think "potential" should be there.

DR. MACIOLEK: Okay, so it reads, "Is there evidence for enhanced accumulation of contaminants in canyons as compared to the slope?"

DR. TEAL: Yes.

DR. MACIOLEK: Okay.

MR. VILD: No answer to the question does not really--it says there is no evidence, but that does not really distinguish between somebody going out and having studied these particular categories and determining whether or not there is evidence versus the studies not being done.

DR. TEAL: It should not be down if there is no study.

MR. VILD: Okay. So we are assuming that there have been enough studies on these--at least one study?

DR. TEAL: Not necessarily enough.

MR. VILD: Well, there have been studies, at least one study.

DR. TEAL: Yes. I don't think it should be there if no one has looked for it.

MR. VILD: Okay, fine. We need to make that clear, then, because the way we have got--you are partly right--what we are throwing ourselves into is that we are going to list all things in the world that are possible--

DR. KRAEUTER: You are right, but we need to say that somewhere, because otherwise somebody is going to say, "Well, they did not think about--"

DR. TEAL: You could add to the sentence, "Based on existing studies." Let's put that in there in some way--it does not have to be added to the sentence. I mean, I know that could go on for two or three pages.

DR. KRAEUTER: Our particular thinking, John, on item "G"--

DR. TEAL: I do not think it should be there.

DR. KRAEUTER: In that case we would cross it out, because nobody has really studied it in the canyons with that kind of--

DR. GRASSLE: We could add the word "limited" to "existing studies."

MR. VILD: Okay, so we are going to strike "G" and "H" then?

DR. TEAL: Yes, I think so.

DR. MACIOLEK: Did we provide answers for "D," "E" and "F"--

DR. TEAL: "F" is no, as I see it, looking at Boehm's data, which is the only stuff we have got. He shows enhanced levels on the slope and canyon--that is what he says.

DR. GRASSLE: Although the extreme case is Hudson canyon, and I think there is evidence there from the sludge materials--we are talking contaminants--

DR. TEAL: From sludge material being in Hudson Canyon.

DR. MACIOLEK: All right--in Paul's presentation did we--

DR. TEAL: Generally no, but there is in Hudson Canyon.

DR. GRASSLE: I think there is.

DR. TEAL: I think we should be very specific about that, because there we are talking about sludge dumping, and that has not got anything to do necessarily with what this whole document is about.

DR. GRASSLE: But then?

DR. TEAL: I agree that it ought to be put in about--not just making that a "yes."

DR. KRAEUTER: You could either put it in as a "no" or a "yes."

DR. GRASSLE: You cannot put it in as a "no"--

DR. TEAL: Hudson Canyon "yes," the rest of them "no."

DR. MACIOLEK: Sure.

DR. BOTHNER: Did Paul Boehm review the data from the slope program? I see his diagram. There are some dots in Lydonia Canyon.

DR. MACIOLEK: He did include that.

DR. TEAL: Did he? Okay. I just read his report a few minutes ago--Pete, we can ask him about that.

DR. BOTHNER: All right. These maps--the data has dates on it that predate the slope program.

DR. MACIOLEK: No, Mike, he went back to I think even some of the benchmark data and also included the slope and rise program results.

What about the trace metals in there? Did we have--

DR. BOTHNER: You saw how weak the signals were, but they were consistent with the hypothesis--that is why I like the word "potential" in there, because these guys--you know, no one would look at that data and say, "There is a contaminant increase."

They would say, "Gosh, you have got a little higher concentrations after you normalize and massage and look at it--you know, almost not worst case necessarily, but look at it very carefully. Then you see a slight increase in the indicators, showing the canyons are--"

DR. TEAL: I would suggest that you can get around that either of two ways.

You can leave it in, you can put a "yes" and you can put an asterisk by it that says, "Signals very weak"--

DR. BOTHNER: I like that just that way.

DR. TEAL:--or you can cross it out. I think the first one is better.

DR. BOTHNER: Sure.

DR. GRASSLE: Tell me, which canyon is that? That is this fellow in the blanks here. Trace metals is "yes" in which canyons?

DR. BOTHNER: Lydonia.

DR. GRASSLE: Just Lydonia? And we decided that for hydrocarbons it was "yes" for Hudson and "no"--Boehm said it was in Lydonia and Oceanographer? What canyons?

DR. MACIOLEK: Lydonia, certainly.

DR. BOTHNER: Only in Lydonia it was the slope program.

DR. GRASSLE: Okay, so we will just say Lydonia for now.

DR. KRAEUTER: We are going to asterisk that--"yes" for Hudson and explain that it was the sewage or the sludge as opposed to being transported in some other way?

DR. GRASSLE: Well, we'd better check that paper out.

DR. AURAND: Are we supposed to be restricting our discussion to the north Atlantic canyons? Hudson Canyon is a mid Atlantic canyon, so that may decide the issue for us right there. Or do you want to put the asterisk in the middle?

DR. GRASSLE: I guess that, in the interests of making the document informative, we probably should not be too strict in that regard.

MR. VILD: All right, so let's go with the asterisk. We will need a double asterisk.

DR. GRASSLE: I think we need the canyon list that Dick prepared. He goes down as far as Norfolk, saying that the--

DR. TEAL: I think that probably the asterisk--as we say, there are so few data on this point. There are very few data on this point, so we include the possibly unique situation of sludge dumping in the mid Atlantic, Mike.

DR. KRAEUTER: We did not say anything about the vertical mixing sediments as to the canyons--

DR. TEAL: There is good evidence to that.

DR. KRAEUTER: --but we did not say which canyons. Well, it is only in Lydonia that--and, remember, when you deal with the rates of mixing, this preliminary analysis of the data suggests that there is not a great deal of difference between the canyon and the slope, so that rate will not--yes.

The fact that it exists is worth noting, that maybe there is a sentence there that says, you know, any contaminants introduced will be--

DR. TEAL: That is true everywhere. That does not belong in the table. I mean, it is a known--if the results show that it is not enhanced, then the answer is "no."

DR. KRAEUTER: If the answer is "no," why not say so? We have got the evidence.

DR. GRASSLE: I am sorry, how does the vertical mixing tell us that there is no contaminant?

DR. MACIOLEK: It says that it is not enhanced in the canyon over the slope.

DR. BOTHNER: Just one thing that falls out of the analysis of the radioisotopes is the observation that mixing is significant in the canyons.

DR. KRAEUTER: Mixing in sediments, you are talking about?

DR. GRASSLE: But what does that--if there is more mixing there is less accumulation, is that what you are saying?

DR. BOTHNER: No.

DR. GRASSLE: I mean, how would it come out as a "yes?"

DR. MACIOLEK: It is not coming out as a "yes."

DR. GRASSLE: It should not come out as a yes. I think maybe we ought to just strike it, because if part "C" refers this to a slope environment, there is really no difference in that regard, based on what little data we have.

I think we should strike it.

DR. BOTHNER: Somewhere in this document we ought to indicate that that is an important process when we are dealing with the introduction of contaminants in these offshore sediments.

DR. GRASSLE: Can you add that in somewhere?

DR. TEAL: Put it in your paragraph--in your little write-up?

DR. GRASSLE: That is a good idea.

DR. MACIOLEK: Okay. So for item "C" we have stricken lines "E," "G" and "H" from the list and we have added a "yes" for trace metals and a "yes" and "no" for the hydrocarbon concentrations, as we have been discussing.

Okay, item "D" on page 8?

DR. KRAEUTER: Just a point on that. The next sentence says, "Bothner's research source for the first five data categories"--and we have just changed the first four--certainly not for the hydrocarbons--just so they can make a note of it so that the sentences that follow refer to the changed document.

DR. MACIOLEK: Right. Yes, that sentence needs--Okay, item "D"--if you read that, not only the little statement at the top, but the whole paragraph, it concludes with, "The statement was eventually stricken from the list."

DR. BOTHNER: That is right. This document should be so amended.

DR. TEAL: Right--"E" should be "F."

DR. MACIOLEK: So all of "D" comes out?

DR. TEAL: Yes, all of "D" comes out.

DR. MACIOLEK: Okay. Item "E."

DR. KRAEUTER: That should not be "D."

DR. MACIOLEK: That should now be "D." You see, we have lots of good editors.

DR. KRAEUTER: Making it a question?

DR. MACIOLEK: Yes, making it a question would be, "Are sedimentary environments of canyons similar?" We have two categories--the answer to both is "no." Any changes there?

DR. GRASSLE: Can we delete this statement--"Met with general disagreement?"

DR. KRAEUTER: It is not a statement anymore--it is a question.

DR. BOTHNER: You know, I do not like the premise here of "E." How about, "Are sedimentary processes similar in all canyons?" Something like that.

DR. VALENTINE: Similar environments and processes exist in different canyons, but taken as a whole canyons, you know, are not identical to each other in these characteristics. So if that is what we are trying to say--

DR. TEAL: What you would write in the beginning, in the definition of canyon, is going to point out that they are not all the same.

DR. VALENTINE: It is going to be in this "Characteristics" part.

DR. KRAEUTER: Why do we have to have "E" at all? All it says is that all canyons are not the same, that is all.

DR. VALENTINE: Well, it just tells them that we have some data from these two canyons here.

DR. GRASSLE: I guess it should--to me, it should say, "Are sedimentary environments of canyons different?" And then the "nos" should become "yeses."

DR. MACIOLEK: Okay, good idea. Mike was focusing more on processes rather than environments.

DR. BOTHNER: Yes, somehow I did not know what sedimentary environments meant.

DR. VALENTINE: It is a combination of sediment source and sedimentary processes--what the end result of the processes working on the sediments is. You know, is it a gravel patch or rippled sand or bioeroded cliff? That is the environment.

DR. GRASSLE: Shall we say "sources and processes?"

DR. VALENTINE: Fine.

DR. GRASSLE: "Are sedimentary sources and processes of canyons different?" And the "nos" become "yeses."

DR. KRAEUTER: Read your statement again.

DR. TEAL: It has to be a little different. It has to be fixed up a little. Canyons differ from one another.

DR. KRAEUTER: There is something wrong--

DR. TEAL: Yes.

DR. GRASSLE: Are sediment sources and sedimentary processes different from canyon to canyon? Canyon to canyon is better.

DR. BOTHNER: Well, actually the question is, "Are sedimentary sources different from canyon to canyon?" Sources are the same, kind of.

DR. GRASSLE: Look, are we going to do something like this, or do you want to delete the whole thing?

DR. BOTHNER: I would rather delete it, and I would rather have that introduced in the beginning as a true statement that we have data from two canyons, you know, a lot of data, physical data from two canyons, biological data from a lot more, and we find a whole range of characteristics in these canyons, such that you cannot automatically predict the processes of impacts in all the canyons.

(Simultaneous discussion.)

DR. GRASSLE: Maybe you can amend your 100-word thing to highlight that point.

DR. VALENTINE: Where is the 100-word thing going to come? Before this or after it?

DR. GRASSLE: It is going to come--

DR. VALENTINE: It is coming up later on.

DR. TEAL: I think we should put it in the definition of canyon--characteristics of a canyon.

DR. VALENTINE: That is where it is going to go, but where is this thing going to go?

DR. GRASSLE: No, in the new thing you will write that we highlight this point--let's do that.

(Simultaneous discussion.)

DR. KRAEUTER: We have not decided where all of this stuff we are editing right now is going to go.

DR. MACIOLEK: Well I thought we were leaving it in the front section.

Now, remember where these--now they are questions--but where they originally came from. Brad presented them as a series of summary statements to be considered for consensus opinion, and then we are showing whether or not data supported his statements as they were at the time.

We have changed them into questions. We are probably getting away from--his original intent, I think, was to summarize what we knew.

(Simultaneous discussion.)

DR. GRASSLE: It clearly goes where we talk about a typical canyon--it also should be said that there isn't such a thing.

DR. MACIOLEK: Well, I was just thinking of Mike saying, "Let's toss the whole thing out." The group agrees to toss it out?

DR. GRASSLE: Yes, but it should go in the beginning where we define what we are talking about.

DR. MACIOLEK: It seems to me that those tables that deal with the geology ought to be followed by similar presentations on the biology.

DR. TEAL: There is one.

DR. MACIOLEK: Before we get to it we encounter on page 9 the list of data categories on which limitations exist, so I was going to suggest that some of that is either repeated in the needs section in Part II, or it can be moved to that section, rather than occurring at this place.

DR. BOTHNER: Sure. It would go behind the biology, at the end of the needs.

DR. KRAEUTER: It ought to go before the needs.

DR. GRASSLE: I agree.

DR. KRAEUTER: If you look it over at that time, when we get back there.

DR. MACIOLEK: And the box model that is on pages 10 and 11?

DR. GRASSLE: I do not see that it adds a great deal.

DR. MACIOLEK: It was an effort to summarize possible pathways of exchange.

DR. KRAEUTER: I know. There are an awful lot of question marks.

DR. MACIOLEK: So we want to take that out--pages 10 and 11 are out.

Okay. The table, the Biological Characteristics on page 12, I guess, becomes item "D" in that sequence, following on from the geology?

That table differs from the other in that it does have a list of which canyons we know these date pertain to.

DR. BOTHNER: Do you have a question?

(Simultaneous discussion.)

DR. KRAEUTER: We do not have a question, either.

DR. GRASSLE: "Do biological characteristics of canyons differ," right?

(Simultaneous discussion.)

DR. MACIOLEK: Are you saying characteristics of canyons? Differ from community--are you saying characteristics of canyons or--

DR. GRASSLE: Do biological characteristics of canyons differ from the slope?

DR. MACIOLEK: Do you want to rephrase the small "a" through "e" or just leave it as is?

DR. TEAL: That will be changed in the same way the other things were changed so that they read properly and make sense.

DR. GRASSLE: Is biomass higher?

DR. MACIOLEK: Do we need to add something about which canyons we know this for? It is easy for infauna because the only formation--I did not consider Gil Rowe's information, but I was going to say we only know it for Lydonia, but he looked at another canyon.

DR. GRASSLE: How many canyons do you have?

DR. KRAEUTER: I put together a list, Nancy, of the canyons that I felt came under the umbrella characterization of canyons as unique, special environments. You will see it in my notes.

DR. GRASSLE: Can you put the canyons--the codes for the canyons?

DR. MACIOLEK: Do you think the 12 that you listed here--

DR. KRAEUTER: Maybe you just need to split--have you looked at them all?

(Simultaneous discussion.)

DR. GRASSLE: You know Barbara's data well enough to--she only has--

DR. TEAL: All canyons--

(Simultaneous discussion.)

DR. COOPER: That list includes canyons that our group has been in--in the head--with regard to types of fisheries and ones that Barbara has been in.

DR. GRASSLE: So if it is all--will include all--we will just put the codes for each of the megafauna? Nancy and I will do the same for the infauna.

DR. MACIOLEK: So we will mark that up on our hard copy and give it to the editors directly.

DR. KRAEUTER: I am a little puzzled by item "d"--nurseries.

DR. MACIOLEK: Oh, small "d?"--instead of "nutrients" should be "nurseries?"

DR. KRAEUTER: Yes.

DR. COOPER: I think that is what Barbara had there, was "nurseries." It makes sense and "nutrients" does not.

DR. MACIOLEK: Yes, I am sure you are right.

(Simultaneous discussion.)

DR. KRAEUTER: I was really puzzled. I could not remember what it was.

(Simultaneous discussion.)

DR. COOPER: The process of elimination--

DR. MACIOLEK: Okay, well, there is only that one table pertaining to biology about halfway down page 14, where we got into that impact.

Fred, you were saying that you thought this material just did not belong.

DR. GRASSLE: I think it should just be tightened up a lot, and maybe we could quickly go through it.

Maybe this whole business about the area of the 1 percent--say that one approach to looking at this is to take an arbitrary 1 percent and consider the surface area involved and what the source is and do some calculations, and have that as an approach to the problem.

Then it seems to me that life stages and sensitivity needs a separate section. We had some more of that in the afternoon.

It really could be tightened up into a couple of pages.

I think that, John, maybe you could have a go at the 1 percent and the surface area--

DR. TEAL: I would like to leave that out.

DR. GRASSLE: No, but instead of having the discussion verbatim, let's say an approach to thinking about the problem is to calculate--you know, the sort of calculation Brad was going through of the surface area, and Mike and so on--that approach to things--the calculation, and then you could put in--I mean, I am happy to leave the 1 percent thing out, but that approach to thinking about the problem should be apparent.

So can you write that paragraph?

DR. TEAL: I have to leave in 45 minutes so I am just going to write it.

DR. GRASSLE: Write that paragraph?

(Simultaneous discussion.)

DR. MACIOLEK: Does that take us to the end of this first section, then?

DR. GRASSLE: I will have a go at the life stages and sensitivity one.

How about impacts on sessile organisms? I think that can go under John's section, whatever its use was in there, and I think that--I do not think that the measure quantifying biological impact says anything by my quick reading of it. I do not know whether anyone else felt it said anything.

DR. COOPER: I would leave that out, myself.

DR. GRASSLE: Yes, I would leave it out.

(Simultaneous discussion.)

DR. GRASSLE: I guess I feel that development versus exploration--I think everything we have said includes both things.

DR. KRAEUTER: That is part of the same thing.

DR. GRASSLE: Yes.

DR. MACIOLEK: Some of that is repeated in the second part, too.

DR. KRAEUTER: I think what that is is where Jerry came in--he had to leave, and we wanted to get him to summarize that stuff right after lunch or something.

DR. MACIOLEK: That is the first part of--

DR. GRASSLE: Are we through going through the report?

DR. MACIOLEK: Let's just back up a minute. We are rewriting part of pages 14 and 15, but on page 17 we want to eliminate the section called "Measuring and Quantifying Biological Impacts," and do we want to eliminate page 18 also?

DR. GRASSLE: Yes.

DR. MACIOLEK: "Site-Specific Monitoring and Development Versus Exploration?" So that just all comes out? What about the area occupied by canyon?

DR. GRASSLE: That is going to be incorporated into the section John is going to give you.

DR. TEAL: What is? The area occupied by canyon?

DR. MACIOLEK: Actually, that is a good thing to put into a characterization/definition type thing, as well.

DR. GRASSLE: Maybe it could be both places, but insofar as the area is relevant to that approach, to thinking about it, it should be there, and also it should be in the part that Page is writing.

I think it may actually be there. I cannot remember. How about you?

DR. MACIOLEK: Okay. That is Part I.

Now, for Part II, which is the real document, we will incorporate under "Characteristics of Canyons" the paragraphs that people have written on these different topic areas.

Do we have something on characteristics of discharges?

DR. GRASSLE: That is really part of Jerry's stuff, isn't it?

DR. MACIOLEK: No--

(Simultaneous discussion.)

DR. MACIOLEK: Jerry's paragraph on drilling muds really does not characterize discharge.

DR. KRAEUTER: That is true.

DR. MACIOLEK: This is where Jerry just did a brain dump onto the paper.

(Simultaneous discussion.)

DR. GRASSLE: Anyway, that part is pretty straightforward. I think that we used what Jerry had and it goes into the editor and then people-- I do not think there is anyone here who can add anything to that.

(Simultaneous discussion.)

DR. MACIOLEK: Jerry's material right now is under Part II, "Possible Impacts," and it is sort of a little unit, because he said all of his before he left.

I do not think it should stay in there at all as it is right now. Maybe what he has given us can be used elsewhere, but it should not be just a little unit by itself.

DR. GRASSLE: I think it should come after the section Page is fixing up, and Jerry's section could come there and then the paragraph John is writing.

DR. MACIOLEK: You are back in a different--

DR. GRASSLE: No, I am still here. I am still in the front of this.

DR. MACIOLEK: We are in a different place.

DR. GRASSLE: I suggest that Jerry's stuff be moved to just before page 14.

DR. MACIOLEK: Okay, so it comes out of what we have been calling the second section, and it goes back into the first section.

(Simultaneous discussion.)

DR. KRAEUTER: Then we do not have anything on possible impacts, other than physical obstructions and blowouts.

DR. MILLER: That was the reason they put that there, possible impacts based on Neff's discussion.

DR. KRAEUTER: The "Possible Impacts" section, I think.

DR. GRASSLE: I think we do that in the conclusions. The stuff we put up here, it seems to be...really hit the possible impacts in a much better way than what Jerry said.

DR. KRAEUTER: That is why I am saying that we do not need "Possible Impacts" unless we want a section describing the characteristics of the canyons and characteristics of discharges.

DR. MACIOLEK: We also have essentially an outline form on page 23, which is just a list of what agents could possibly cause impacts. If anything, maybe we can just include the list.

It is just that Jerry's material is, you know--it does not really blend in. It is just information that he gave us.

DR. GRASSLE: Let's try putting it into 14 and look at the possibility of deleting it if it is easier done that way.

DR. MACIOLEK: I think as we went through the conclusions there were various times when people brought up or referenced something Jerry had said, and so it comes in there and it therefore is included.

DR. GRASSLE: So you think it is redundant?

DR. MACIOLEK: I guess I do think it is redundant--it does not have to be written elsewhere. So we have decided to leave it out?

(Simultaneous discussion.)

DR. GRASSLE: How about on page 24?

DR. MACIOLEK: Okay. Are we going to leave the "Potential Impact" section simply as this list, as it is on page 23, or does that need--

(Simultaneous discussion.)

DR. WRIGHT: May I say a word here. We used that as the skeleton on which we hung the conclusions that begin on page 24.

DR. KRAEUTER: It is just an outline or sort of a table of contents.

DR. MACIOLEK: What is your suggestion--that it is necessary as a guide, or that it is not necessary at all?

DR. WRIGHT: I think I would like to see the final text before deciding how much of a guide is needed.

DR. MACIOLEK: It seems to me that it is useful to have at least a brief statement as to where we think potential impacts will come from--that is what this gives us.

DR. GRASSLE: Yes--let's use this as an outline for a part of what Jerry had--in other words, to define what these are. I think we have got that and can maybe go through the things that Jerry had, as that is sort of what drilling mud is.

DR. MACIOLEK: Be careful, because it sounds like what you are saying is to use some of his information back in here to define this, and we do not go through the whole thing, so--

DR. GRASSLE: That is right. I mean, he has got physical obstructions, blowouts, produced water--but I think we need something of that. Maybe if we do not have it--who could add it in?

Maybe we ought to have three sentences for each one of those. Where it is Jerry's stuff we will put in--and where we do not have it, I guess--can you put it in, Bob? I mean, it has got to be somebody who really knows a lot about the drilling operations.

(Simultaneous discussion.)

DR. MILLER: I do not think I want to. I should stay out of this.

DR. KRAEUTER: I think then either Jim or Ray--

DR. GRASSLE: We do not have anybody who can do that.

DR. MACIOLEK: Right, but they are going to get this document and we can ask them specifically to add to that particular section.

DR. GRASSLE: Let's get Jerry to finish it off, to keep it brief. Do you think it would be too long?

DR. MACIOLEK: No, I think it will be hard to get Jerry to do much more at this particular point, but we can try. We can ask Jerry and we can ask Bob Ayers--

DR. GRASSLE:--get them to say what these are, and that is easy to do.

DR. MACIOLEK: We had--you know, as we went through the afternoon we decided ultimately to eliminate two of these categories, hydrocarbons and sewage. We just said they were minor compared to the others. I suppose we should still leave them in there.

(Simultaneous discussion.)

DR. GRASSLE: It is important to have it in there, because it will show what was minor.

DR. MACIOLEK: I think we probably had better spend more time on the conclusion section, which is probably harder than that.

DR. BOTHNER: Are we past page 24 yet?

DR. MACIOLEK: We are sort of on page 24. That is where the "Conclusions" section starts.

DR. VALENTINE: Have we decided where the "Characteristics" are going in? These 100-word things?

DR. MACIOLEK: That is the first part of the document. It is number one and number three, which will be conclusions. So the "Characteristics" information will go up front, at the beginning.

DR. VALENTINE: And not in that first 19-page stuff?

DR. MACIOLEK: No.

DR. GRASSLE: Instead of Ray or Ayers, can we just say it was agreed?

(Simultaneous discussion.)

DR. KRAEUTER: Where are you now?

DR. GRASSLE: Starting down on page 24, in the comments. I was on the fourth one.

DR. TEAL: What happened to the pages before that?

DR. GRASSLE: We just discussed page 23. We are going to have three or four sentences on each of those points and we are going to use it from Jerry's section, if we have it--otherwise, we are going to get it from Ray or Ayers.

DR. TEAL: Page 24, under the set-back thing--I think one of the reasons that the set-back is desirable is because it would prevent disturbance to the boulder fields and areas of burrows and grottos, which are the most important nursery areas of the canyon.

DR. GRASSLE: Because it would--

DR. TEAL: It would prevent disturbance--I have got it written down. I will give it to you--"to boulder fields and areas of burrows and grottos, which are the most important nursery areas."

DR. VALENTINE: That may eventually become the first item underneath "A."

DR. KRAEUTER: I have been wondering why we have "A" there. "A" is fine, but then it does not match with "B."

DR. MILLER: Those were used for organizational purposes, when there was just writing--these were just simply used as an outline.

(Simultaneous discussion.)

DR. KRAEUTER: They can figure it out.

DR. GRASSLE: Maybe we can just delete this heading, "Operational Discharges."

DR. VALENTINE: That is really part of the justification and conclusions, right? All of this section here?

DR. GRASSLE: I think the big heading is "Conclusions." We do not need a big heading, "Operational Discharges." I would delete the word "set-back," because that is--you know, these are just statements--"A," "B," et cetera.

DR. VALENTINE: But are we going to have a separate heading to replace "Operational Discharges?"

DR. GRASSLE: I would suggest not.

DR. VALENTINE: I would like to make a few little changes or suggestions.

"No rig should be closer than 500 meters to the--" instead of rim put in "boundary of a canyon as defined by NOAA." These boundaries go around the canyon on the shelf, but also down the slope.

Then, further down, where it says "it would be possible to contain all operational discharges," I would insert something like, "for on-shore treatment." Is that what they do?

I mean, they are not going to discharge anything at sea, but it is being contained for on-shore treatment, is that--

MR. VILD: No, it can be just transported off site and then dumped.

DR. VALENTINE: Oh, all right. Do we have to qualify--it says, "to contain all discharges--"

Would anybody want to know what is going to happen to that stuff?

DR. GRASSLE: We are making that "to be transported off site."

DR. KRAEUTER: If they store it there, who cares?

DR. VALENTINE: It has to be transported off eventually. It is not discharged there, that is the point.

(Simultaneous discussion.)

DR. GRASSLE: Okay, so we amend this--"Within canyons it would be possible--"

DR. TEAL: "--to transport all operational discharges--"

DR. KRAEUTER: "--to contain and transport--"

DR. TEAL: You do not need to have "contain."

DR. GRASSLE: "--to transport operational discharges off site--"

(Simultaneous discussion.)

DR. MACIOLEK: Off-site--deeper in the canyon?

(Simultaneous discussion.)

DR. GRASSLE: You do not like "off-site?"

(Simultaneous discussion.)

DR. MACIOLEK: Away from the canyon?

DR. TEAL: Away from the canyon--simple and straightforward.

DR. VALENTINE: The next sentence might be clarified to say, "Production wells could be added to reach up to 2 miles away from the platform and thus drill beneath a canyon."

It was not really too clear to me--somebody who has not really thought about this--

DR. GRASSLE: This raises a more general question. Do we need this parenthesis at all? We are in conclusions.

DR. TEAL: I do not think so.

DR. GRASSLE: Let's take it out.

DR. TEAL: It is better not to have Ayers' and Ray's names.

DR. GRASSLE: Yes, I definitely wanted to take that out.

(Simultaneous discussion.)

DR. VALENTINE: Since these are our conclusions, this half page here, we ought to be a little careful with our language, I think.

DR. BOTHNER: I think that is right--so we will take this out. But you are not going to take away the reason why--

DR. GRASSLE: No, no, just the parenthesis.

DR. BOTHNER: Did we underline here?

DR. GRASSLE: We did, in fact, but I do not know if we would want to.

DR. MACIOLEK: I do not think the document should have underlines.

DR. GRASSLE: Yes, let's not underline.

DR. MACIOLEK: Page, you were saying that that last sentence probably will not be clear to the general reader, and I am sure you are right.

DR. GRASSLE: We deleted it. The whole parenthesis is deleted.

DR. MACIOLEK: Oh, everything in the parenthesis is deleted?

(Simultaneous discussion.)

DR. GRASSLE: Only the underlining there would appear--

DR. BOTHNER: Would you read the sentence, Fred? How much is left?

DR. GRASSLE: "There would appear to be little impact on the feasibility of exploration or production drilling."

DR. BOTHNER: The person who reads that and who is not aware of the fact that we are talking about the advantages of slant drilling will never get it.

I think you have to say, "Because exploration is possible by slant drilling, there is no need to drill immediately above a canyon."

(Simultaneous discussion.)

DR. AURAND: In exploration they do not like to do slant drilling. For exploration they do not like to do it. They do a little bit of it, but not very much.

They are trying to do the stratigraphy as they go down through the hole and if the hole is off to some angle it is difficult to measure, and then they have a difficult time figuring out where they are, so they do not like to do that.

They have no objection to doing it for production, but for exploration the closer to vertical they are the better they like it.

DR. KRAEUTER: All we are doing is citing a reason why we say no rig should be closer than 500 meters.

DR. GRASSLE: I do not think the technical explanation is required.

DR. BOTHNER: Okay, but I do not see, then--

DR. MACIOLEK: But then it does not make sense, because how can they explore a site in the canyon if they cannot be closer than 500 meters and they cannot--

(Simultaneous discussion.)

DR. AURAND: Part of that depends on--Jim and I talked about this a little bit--some of it depends on how deep you are trying to go.

With a 500 meter off-set you are looking at a 20,000 foot--you are aiming at a formation that is 20,000 feet down. It is not much of an off-set, so you are pretty close to vertical.

If you were looking at a shallower horizon, then it would become more of a problem. That, I think, is what led to the discussion.

If it was a discussion where they absolutely had to put something in the middle of a canyon, they just would not discharge it.

I do not know that--I do not know what would happen from the agency's point of view if they were trying to do that, but they have to be close to vertical. I do not think anybody ever defined what "close to vertical" was, other than the deeper you go, the further back you can be from the rim of the canyon and still approximate a vertical hole.

DR. KRAEUTER: They seemed to think it was feasible for most of the basic information?

DR. AURAND: In most cases 500 meters would not be a problem.

DR. MACIOLEK: Okay. I think that first reason, then, probably should go to the bottom of the list because, if we are saying that no rig should be closer than 500 meters, and our reason is because it is not going to impact exploration, it just does not compute.

Our reason for the set-back is to avoid a direct--

DR. VALENTINE: By deleting the material in parentheses you are excluding the possibility of drilling within a canyon, which we have concluded would be possible if containment were achieved.

By deleting that material you are not addressing actually drilling in the canyon at all. So that changes it.

DR. MACIOLEK: I guess my point is just that, as it reads now, this statement may be correct but it does not appear to me to be reason for requiring a set-back.

It is just saying that the set-back will not affect operations, but it is not a reason for it.

(Simultaneous discussion.)

DR. AURAND: The key problem is to prevent the accumulation of footprint in the canyon.

There are two ways to do that. One is to move back to the edge. The other one is to drill there but not discharge there. Then you can say, in most cases you probably will not need to drill there and a 500 meter set-back will be okay.

If the real goal--if the real intent of that is to not have anything accumulate in the canyon head, then I think there are two ways to do it--not let anything out, or move back away from the edge.

DR. VALENTINE: In exploratory drilling, at least on Georges Bank, the routine was that the first 1,000 feet, which was like a 36 inch hole--the first 1,000 feet was an uncased hole. So they set casing at a 1,000 feet.

So for the first 1,000 feet of a 3-foot diameter hole the cuttings go on the floor.

DR. AURAND: Yes, there is--I do not know that it always has to be a 1,000 feet--

(Simultaneous discussion.)

DR. VALENTINE: I think the 1,000 feet in that case was that they wanted to get into the Eocene limestone. You cannot entirely eliminate it.

(Simultaneous discussion.)

DR. AURAND: You would certainly minimize by doing that, but you could not get rid of all of it. That is a good point. That is probably the reason to prefer the 500 meter set-back.

DR. KRAEUTER: If you were a company and you could get the same, it has got to be a lot cheaper, just from their point of view. It has got to be a lot cheaper than containing, transporting and doing a whole lot of other stuff.

DR. AURAND: From talking to Jim and Bob, I am sure they would prefer the 500 meter set-back. I mean, they recognize, number one, the cost of taking this stuff away, and two, there would be the public perception problem.

They do not want to drill a hole in the head of a canyon more than anybody else does, simply because of public relations, so I am sure they prefer the set-back.

DR. KRAEUTER: Considering how few holes they have out there, the first hole they drill somewhere close is going to be an awful lot.

DR. AURAND: I am no geologist, but if you are going to a deep formation and you cannot figure out what is going on....

(Simultaneous discussion.)

DR. GRASSLE: Okay. I think we have reached a conclusion. This phrase will now read, "This set-back would appear to have little impact on the feasibility of exploration or production drilling." It will be at the end of the list.

DR. MACIOLEK: It goes at the end?

DR. GRASSLE: It goes at the end, and the new one goes at the beginning--the new one that was stated by John.

Okay. Are there other comments? On page 24?

DR. KRAEUTER: Do we really want to include things like tools dropped off the rig in the results?

DR. BOTHNER: Fred, I suggest that you change the word "feasibility" to "results."
DR. GRASSLE: What line are we on?
DR. MACIOLEK: Where are you?
MR. VILD: No, I do not think so, because I think by "feasibility" you are also talking about economic feasibility --
DR. VALENTINE: Engineering feasibility?
MR. VILD:--but also economic feasibility. You are talking about containing and transporting all of the cuttings off site, or even angling--now, that is going to cost more money than just cutting a straight hole and dropping it as you go.
DR. BOTHNER: "Feasibility" has got more breadth.
DR. VALENTINE: One question--are we down to the second one now?
DR. GRASSLE: Yes. The question was raised whether we want this list of cuttings, a major fraction of drilling muds and tools.
DR. VALENTINE: Is it clear what the first word in this refers to, or should we put in that the 500 meter set-back would exclude--
DR. GRASSLE: Yes. I think actually that that is a general comment--should be the 500 meter set-back to start each of these. Each of these phrases should start, "The 500 meter set-back would--"
DR. VALENTINE: Are we going to change tools to something like refuse or debris?
DR. GRASSLE: Debris.
DR. KRAEUTER: Debris is much better.
(Simultaneous discussion.)
DR. GRASSLE: Okay. Are there other comments on page 24?
DR. MACIOLEK: Now you are getting to--
DR. GRASSLE: Yes, section "B."
DR. KRAEUTER: Did we check that calculation?
DR. GRASSLE: Yes. We are going to delete names, right?
DR. BOTHNER: That is a good idea. However, I would like to change some of the numbers because I got data from Lydonia Canyon. Rather than the fine grain sediments, which was all I had yesterday--
DR. GRASSLE: Go ahead.

DR. BOTHNER: Background values are 45 (35 to 68 ppm).

DR. GRASSLE: Where were we, now?

DR. BOTHNER: The top of page 25--

(Simultaneous discussion.)

A PARTICIPANT:--the second line. I will just read them and then I will give you the data. If everybody likes the way it sounds, I will give it to you directly, all right?

Okay. That would add 20 ppm to the chromium content of the top centimeter of sediments.

A PARTICIPANT: Wait a minute--sorry. He is reading from the top of the page.

DR. GRASSLE: The top of page 26.

DR. BOTHNER: The first sentence must change. "Background values are--" and we should perhaps say--"Background values average 45 ppm (35 to 68 range) in sediments at the head of Lydonia Canyon, so the increase could be close to 50 percent. However, that assumes that all of the chromium added to drilling mud reaches the canyon and that all of it is deposited in one square kilometer."

Scratch the rest of it, and then add, "The 50 percent increase calculated in this worst case scenario results in a concentration that is within the range of observed background chromium concentrations in canyon sediments."

(Simultaneous discussion.)

DR. GRASSLE: Okay? When we finish this page, maybe you could take it over to them so they can read it over to make sure they have it right now.

Okay. "C?" "D?"--including 25(a)?

MR. VILD: What about this business about "probably" and "likely?" Can't we just strike the "probably?"

DR. GRASSLE: Yes. Strike "probably." I have already done that.

(Simultaneous discussion.)

DR. BOTHNER: Then 26a has got some inaccuracies.

DR. MACIOLEK: Oh, 26a? I am sorry, 25a.

DR. GRASSLE: Go ahead.

DR. BOTHNER: I do not know that the first rate is--somebody has come up with a 0.1 cm per year--I do not know where that came from. The rate we are comparing it to is 0.06 cm per year.

DR. GRASSLE: That is 0.06 cm per year.

DR. BOTHNER: But that changes the conclusion so much that I would like to know where that 0.1 came from. I do not remember it.

DR. KRAEUTER: Was that Bob Ayers talking about those calculations they were doing at Toms Canyon? That is what I remember, but I am not sure at all.

DR. BOTHNER: What I got from his talk is in the canyon axis--you know, he had 1 percent drilling mud contribution, something like that. But I do not understand where that value comes from.

DR. MACIOLEK: I do seem to remember Bob Ayers tossing out a number--

DR. TEAL: It was 1 percent--it was less than 1 percent in content. He had them up on (inaudible) board.

DR. KRAEUTER: What does that calculate to?

DR. TEAL: It calculated to be less than 1 percent of the deposition in Toms Canyon--that was what he showed.

DR. MACIOLEK: Did he leave his--I do not doubt you, but I wondered if he left his written information from that study.

DR. TEAL: We have some tapes that narrow it down--

(Simultaneous discussion.)

DR. MACIOLEK: I think the comment is somewhere--it was in the middle of the afternoon.

DR. TEAL: Where this came from, I think, was the same kind of calculation as we did for chromium, but I do not know whether 0.1 is correct. I think that how we got to whatever number we came to--

DR. BOTHNER: Maybe he and Brad and Jim Ray were sitting in the back of the room, and I know Brad uses a thousand cubic meters as the discharge of mud for a well. I mean, that is his thing, right?

(Simultaneous discussion.)

DR. BOTHNER: So let's see if we can reproduce that.

DR. TEAL: A thousand cubic meters spread over a million square meters--

(Simultaneous discussion.)

DR. BOTHNER: That is 10^3.

DR. GRASSLE: I think we probably ought to let Mike write this paragraph with help from John. I think we are taking too much time writing it now.

DR. TEAL: That comes to--assuming that it is all concentrated in--

DR. BOTHNER: My notes attribute that to Butman. I mean, I might have gotten the notes wrong.

DR. GRASSLE: That's right. Brad started it and Mike did it and John did it--all three did it.

DR. MILLER: That is right.

MR. BOURNE: It says 2 grams per liter per day and then I have in parentheses 0.6 cm/year compared with 0.1 cm/year. Both rates are relatively low, is what it says.

DR. KRAEUTER: Yours was the real one and theirs was the theoretical.

MR. BOURNE: What it should have said is 0.06 cm/year. If it is really 0.1 cm/year, based on what you just got through--yes, that is close.

DR. GRASSLE: So the two of you are going to get a paragraph straight.

DR. TEAL: Just change it to 0.06. In the paragraph that I am writing about making these calculations, I am going to say that the assumption there is highly conservative--

DR. GRASSLE: Yes.

DR. TEAL:--and that the resultant values are going to be way above what we could reasonably expect.

DR. VALENTINE: I just have one question about this millimeter rate. How far from the rig was that?

DR. TEAL: That is more than 500 meters away--or, assuming that everything gets transported--

(Simultaneous discussion.)

DR. GRASSLE: In the next paragraph, the second sentence, I suggest we change that to "Although constituents of the muds may not result in direct mortality--" and then delete the parenthesis.

DR. BOTHNER: Where are you?

DR. GRASSLE: Page 25b, second paragraph, starting with "The principal effect of the drilling muds upon the biota--"

"Although constituents of the muds may not result in direct mortality, there may nevertheless be an effect." That is the way the sentence should read, the rest is all right.

I am not sure we need the first sentence. Maybe we do--I guess we do, yes.

DR. VALENTINE: When you are done with that, could we go back to page 25.

DR. GRASSLE: We are done with it. What is on 25?

DR. VALENTINE: In that section "D" about "It is unlikely, et cetera, et cetera--this conclusion is based on similar worst case scenarios."

Then we jump to 25a. Where does that addendum come in, right after metals? On the sheet we have it says "See addendum, part E." That part at the bottom of 25a should come under "D," right?

DR. GRASSLE: Yes. I guess that really this is an editorial question. Maybe we should just put an asterisk there and put this calculation as a big footnote.

(Simultaneous discussion.)

DR. GRASSLE: This calculation is 25a and the top of 25b. That calculation would be a big footnote to the last sentence of section "D."

(Simultaneous discussion.)

DR. GRASSLE: Do you like footnotes as an editorial thing? That the calculations be footnotes to the simple conclusion?

DR. VALENTINE: In that footnote, which is the bottom of 25a, I think it should say, "Measurements near Lydonia Canyon head." "The canyon head" does not tell us where it is.

DR. GRASSLE: Yes, good.

DR. VALENTINE: For 25b, first paragraph, last sentence, it says, "Thus the physical effects on the substrate within the medium to far field should be quite small." Could we put a parenthesis in there after field and tell how many meters or kilometers from the drill site we are talking about? "Medium to far field" does not really tell much about distance.

DR. GRASSLE: Do we mean canyon there?

DR. VALENTINE: I mean, I know the oil company guys are always talking about, you know, 300 meters, 500 meters.

DR. TEAL: We said 500 meters. Why don't we just say that? Beyond 500 meters?

DR. MACIOLEK: Then take out the "medium to far field?"

DR. BOTHNER: I would like to ask the co-chairs of this discussion if they would consider a break for coffee for 5 minutes or 10 minutes or anything.

DR. MACIOLEK: Well, what about it--I think a lot of people are hoping to be gone by noon. Do you want to just go out and get a cup of coffee and bring it back in? There is some right outside the door.

I think--Don, did you have a question about that paragraph?

MR. BOURNE: There was a sentence I did not hear the end of, but Red has got it. Where are we now?

DR. GRASSLE: We are at the end of the paragraph in the middle of 25b, where the suggestion is to delete the phrase after the dash. I think that is reasonable.

DR. TEAL: I think we ought to change the next sentence, which is starting with the paragraph that is labeled "Produced Water."

DR. GRASSLE: Yes, go ahead.

DR. TEAL: How I have it changed is, "Most of the toxics in produced water are volatile, et cetera, et cetera--" to the end of that line, comma, "especially with the near surface discharge."

MR. BOURNE: After atmosphere?

DR. TEAL: Atmosphere, yes, especially with a near surface discharge. Then period. Then the next sentence, "It has been difficult to find any water column effect in the existing studies." I do not think it is existing oil spill studies--it is existing discharge studies.

DR. GRASSLE: So just cross out "oil spills." Others?

MR. BOURNE: I think the idea there, which may not be well expressed--the way I heard it yesterday was that even in oil spills--I mean, a much more drastic case--you cannot find the stuff. So this, being a much less drastic case--

DR. TEAL: There is some controversy about that, however. There are people who believe that there have been evidences of effects below oil spills.

MR. BOURNE: So the way you would like it to read is to scratch out "based on existing oil spill studies" and then to start the sentence "It has been difficult--"

DR. TEAL: "--to find any water column effect in existing studies."

(Simultaneous discussion.)

DR. GRASSLE: I think changes in that--

DR. MACIOLEK: Is it correct at the bottom of 25b--is it correct to say, "Plankton blooms can increase impacts?"

DR. KRAEUTER: "Can increase the transport down--"

DR. GRASSLE: It is not clear.

(Simultaneous discussion.)

DR. MACIOLEK: But we might want to say that, rather than just "increase impacts." We might want to say, "Plankton blooms can increase transfer of material--"

(Simultaneous discussion.)

MR. BOURNE: The way it might read is, "Plankton blooms--" for example "--might increase particulate in the water could expected to increase."

DR. GRASSLE: Okay. I have got a sentence now. "During periods of increased particulate in the water column during spring blooms, increased transport to the bottom could occur--"

MR. BOURNE: Could you read that again?

DR. GRASSLE: "--increased impacts on the bottom could occur."
MR. BOURNE: "Increased particulate--"
DR. GRASSLE: "--in the water during the spring bloom conditions."
DR. MACIOLEK: I think you ought to not use the phrase "increased impacts." We should be a little more specific about what the plankton bloom is--
DR. GRASSLE: I guess it has to be two sentences. "Particulate produced during spring bloom conditions could scavenge hydrocarbons from the water column. The settlement of this material to the bottom could increase impacts." Is that right?
DR. MACIOLEK: Isn't that stated in the paragraph on produced waters?
DR. GRASSLE: It is a problem.
DR. MACIOLEK: It is not really true just for produced waters.
MR. BOURNE: There is a lot of reorganization of this.
DR. GRASSLE: Instead of "increased impacts" it should be "could result in impacts" instead of "increased," because "increased" assumes that there were impacts at all.
DR. VALENTINE: Do we have a little descriptive phrase outlining what produced water is? Could we say, "Produced water is water mixed in with extracted oil and gas--"
(Simultaneous discussion.)
DR. GRASSLE: On page 23 we have three sentences.
DR. VALENTINE: Oh, there it is.
MR. VILD: Then, again, maybe since we are talking about produced water there should be some sort of sentence saying that this is only encountered during production and development.
DR. GRASSLE: That should be back on page 23.
MR. VILD: Oh, okay.
DR. GRASSLE: Let's note that.
DR. KRAEUTER: Good point.
DR. GRASSLE: Is there any comment on "E" then?
DR. KRAEUTER: I have got a lot of problems with that sentence just below the one you were working on.

DR. GRASSLE: Okay, sorry. The gradual increase?

DR. KRAEUTER: Yes.

DR. MACIOLEK: That refers back again, I think, to produced water and the statements based on the study results?

DR. KRAEUTER: Yes. That is why I think we ought to do something. I keep thinking that that first part--maybe what we need there is something like "In data from in-shore oil production areas increases in hydrocarbons and sediments have been attributed to--"

We have got Jerry's data, which is not right up there, but he had a 35-foot well and 9-foot well, telling us very precisely what had been experienced, and that ought to be put in there.

Off-shore, however, in hydrocarbon accumulations and then we need to get that first sentence in there about the gradual increase of hydrocarbon accumulation in surficial sediments over the years may be expected. Do you think we can really--

DR. MACIOLEK: That whole first phrase is in relation to the shallow-water-produced oil.

DR. KRAEUTER: Right, but I would like to get the shallow water one saying, ah, it happens in shallow water now of-shore in these canyons. I think we are extrapolating because we do not know.

DR. GRASSLE: I have a suggestion for this--

(Simultaneous discussion.)

DR. GRASSLE: "From this course, a gradual increase in hydrocarbon accumulation in surficial sediments over the years might occur, comma," and then skip down to the third line from the bottom, "however, net hydrocarbon accumulations, if they occur, would likely be due mostly to other sources such as accidental spills from ships, et cetera."

DR. KRAEUTER: All right, so we do not put anything about the in-shore data?

DR. GRASSLE: I do not see that it is very relevant.

DR. KRAEUTER: Okay.

(Simultaneous discussion.)

DR. GRASSLE: I replaced it with "might occur."

DR. TEAL: I would be willing to go on with "are very unlikely to occur."
DR. GRASSLE: Okay.
DR. KRAEUTER: I have a lot of trouble with "can be expected."
(Simultaneous discussion.)
DR. TEAL: The problem is too, though, that if we just say "might occur" and not reference--we have data of a shallow water study that may come back to haunt us in that somebody in a public hearing could hold that data and say, "We have data that it does, in fact, accumulate."
So what I would like--we may be afraid of saying that there is shallow water evidence of it, but because we are talking about depths of an order of magnitude greater than shallow water studies--
DR. KRAEUTER: I am sure that it was in the 9 foot when it was only out to a 100 meters after 5 years at a 1,000 barrels a day.
DR. BOTHNER: That is pretty minor. That is really stretching it to say we are going to see it.
MR. VILD: So I would go along with what you say, extremely minor or--if we are going to have anything about "might occur"--that we qualify it, saying that there have been some sort of effects observed in very, very shallow water but, since we are talking about a depth of 200 meters or more, the effects would be attenuated.
DR. MACIOLEK: I agree.
MR. BOURNE: In yesterday's meeting it was stated that over a long period there would be a gradual accumulation of hydrocarbons over the years.
DR. KRAEUTER: That was shallow water, though.
MR. BOURNE: I do not think it was.
DR. MACIOLEK: Yes. I am familiar with that particular study and I know that they were talking about shallow water.
I guess I agree with the two of you--
DR. GRASSLE: Start the paragraph, "Hydrocarbon accumulations in surficial sediments from this source are likely to be undetectable." Then leave out the next bit.

Then say, "Net hydrocarbon accumulations, if they occur, are likely to be the result of-- "instead of" due to other sources such as--"

DR. BOTHNER: Other discharges?

DR. GRASSLE: Yes. So maybe just "other sources."

DR. BOTHNER: Sure.

DR. GRASSLE: That cleans it up, don't you think? Does that sound good? Okay. I am going to read the whole paragraph again.

"Hydrocarbon accumulation in surficial sediments from this source are likely to be undetectable."

DR. KRAEUTER: Why don't you just put from "produced waters?"

DR. GRASSLE: Yes, "from produced waters are likely to be undetectable. Net hydrocarbon accumulations, if they occur, are likely to be from other sources."

Then, I guess, in parentheses it should be, "See "i," oil spill. The first sentence may need a little tidying up.

DR. KRAEUTER: Do you want to get "G" up? Next to that? It is almost the same thing. I mean, we are still under--I am getting lost here, but we are talking about produced water.

DR. MACIOLEK: A lot of what is under "G--"

DR. GRASSLE: "--really characterizes produced water.

DR. GRASSLE: "G" should go under page 23.

DR. KRAEUTER: One of the things there--there was a calculation that I think it was Brad made concerning how much that really was in terms of a discharge--like 1 liter of oil per day or something like that.

We ought to find that and have it in there, too, so if we are talking about the general public they can understand, even though there are 2,000 barrels per day, 600 parts per million came out of--I think it was about a liter of oil per day was being discharged. That is important for communicating with the general public.

DR. MACIOLEK: I remember that 1 liter number somewhere.

DR. WRIGHT: That 2,000 barrels referred to total organic carbon in the water.

DR. KRAEUTER: Yes, that is the total volume that is being put out, because I remember somebody commenting on it that boats put out more than that; one boat going by put out that much.

DR. GRASSLE: What I realized is that the next paragraph at the top of 25d was our main statement.

DR. VALENTINE: Could we get back to "G" before you go on? I think that paragraph needs some work.

For instance, "Produced water must meet the 48 ppm." Wouldn't it be better to say, "Must meet the current--" whatever it is "--EPA discharge standard" or something like that? I mean, why be locked into some number that might go--

(Simultaneous discussion.)

DR. TEAL: I am saying, it ought to meet whatever--

(Simultaneous discussion.)

DR. VALENTINE: Then it says "Total organic carbon can range from 200 to 600 ppm." Is that in produced water?

DR. KRAEUTER: Discharges?

DR. MACIOLEK: Right--that is the characteristic of the water, not of the standard.

DR. BOTHNER: I am not sure why that is helpful to have that in there at all. I mean, that total organic carbon issue.

DR. VALENTINE: It just tells you what is in the produced water.

DR. BOTHNER: I know, but I mean, I am amazed--I am guessing that much of that 200 to 600 ppm is organic carbon that is not oil or grease.

DR. TEAL: It is.

DR. BOTHNER: It is all oil and grease?

DR. TEAL: It is not oil and grease.

DR. GRASSLE: I think it is not a useful thing.

DR. BOTHNER: It does not help me, then, because it is a red herring.

DR. GRASSLE: Then, to continue--does anyone want to leave it in?

DR. TEAL: Okay, let's leave it out.

DR. GRASSLE: Then we can delete "This is only a concern during production?" "Produced water is only a concern during production?"

DR. KRAEUTER: We were going to throw that back.

MR. VILD: It is a redundancy.

DR. VALENTINE: Then the next sentence--amounts of what? Produced water? Are hardly variable?

DR. KRAEUTER: Yes.

DR. BOTHNER: It might even be the discharge or something.

DR. VALENTINE: I mean, you cannot tell whether that is for organic carbon or what, so "amounts of produced water are highly variable."

DR. GRASSLE: I think that the editors are probably going to find also that Jerry's statement is a bit better than this. I mean, we happen to have two places to work (inaudible) for these few sentences.

(Simultaneous discussion.)

DR. MACIOLEK: Jerry's statement on produced water was on page 21, just for the reference.

DR. VALENTINE: Here it is.

DR. BOTHNER: 13 pounds per gallon--

DR. TEAL: 14 pounds per gallon and 42 gallons per barrel. What I've done is liters, which is about the same. In 2,000 barrels it comes to 315 tons.

(Simultaneous discussion.)

DR. GRASSLE: All right.

DR. MACIOLEK: Does anybody have a page 26? Is there a page 26?

DR. AURAND: I think that is just a misnumbered page.

DR. GRASSLE: I would suggest that we delete "E" and "F" as well, because when I come to the last paragraph this is our concluding thing. The first sentence of the paragraph at the top of 25d is not a complete sentence, and I think instead of that paragraph we could use what we edited at the bottom of 25b and top of 25c--instead of that sentence.

Then our main conclusion is "Benthic effects are not expected." Now, that really should replace "E" and I think "F" as a sort of--I do not think we need to say anything about it.

DR. KRAEUTER: But those are part of our original list.

DR. MACIOLEK: The third list--

DR. KRAEUTER: They should not be under produced water, that is for sure.

(Simultaneous discussion.)

DR. GRASSLE: Okay, sorry. I guess I was confused by the fact that it is out of order. The paragraph at the top of 25d should be before "E."

DR. VALENTINE: Well, that paragraph is "H," hydrocarbons, right? The name of that paragraph is on the preceding page--at the bottom of 25c is the title for that paragraph.

DR. KRAEUTER: It is just that we got this produced water thing all twisted up here somehow.

DR. GRASSLE: Okay. I think the way it goes now is that we have the paragraph at the bottom of 25b--we just added it. Then it goes to the paragraph at the top of 25c.

Then, immediately before "E" goes the paragraph at the top of 25d, without the first sentence. It should be "benthic effects from produced water."

DR. VALENTINE: I thought that was under "Hydrocarbons."

MR. VILD: Yes, it is under hydrocarbons and deck drainage.

DR. GRASSLE: Oh, was it?

MR. VILD: Yes, I think so. That is what that "H," hydrocarbons, at the bottom of 25c is.

DR. GRASSLE: Oh, of course it is, yes. Excuse me.

DR. KRAEUTER: The one that is sort of out of place is "G"--the produced water piece in "G" is what really messes everything up.

DR. GRASSLE: Okay. Well, "G" was supposed to be--where is the outline--moved to the earlier part, where you define these things. So "G" is out.

MR. BOURNE: If you just take "E" and "F" out of there where they do not belong, doesn't that cure the problem? Either delete them or move them?

MR. VILD: "E" and "F" would probably be better as "H" and "I" or something like that, after "G."

DR. KRAEUTER: What have we done with "G?"

DR. MACIOLEK: It went up into page 23, I think it was, characterization of some of these impact agents. In our list of potential impacts on page 23 we wanted to add some real brief explanation for each of these. That is where we will put information of what produced water is.

DR. KRAEUTER: All right. So what, really, then--

DR. MACIOLEK: In conclusion we are going to conclude--

DR. KRAEUTER: If you put "H" up ahead we have hydrocarbons and then deck drainage.

DR. GRASSLE: Okay. It looks to me as if produced water has to be "E," right?

DR. VALENTINE: Produced water is "E."

DR. GRASSLE: Produced water on page 25b is "E." Then "E" on page 25c becomes "F."

DR. KRAEUTER: We ought to follow our outline--the outline we did on page 23.

DR. GRASSLE: Oh, no, I do not think so--I do not think we can.

DR. KRAEUTER: That is what we have been doing.

DR. GRASSLE: No, I do not think we have, because we decided that "B" is the start of that outline.

DR. WRIGHT: Excuse me, but I think we can reorder those. It comes out 50 parts per million times 2,000 barrels a day--it comes out to about 4 gallons--16 liters.

(Simultaneous discussion.)

DR. TEAL: Here is this thing--should I just give it to somebody or do you want me to read it now?

DR. MACIOLEK: Oh, that is your rewrite of--

DR. TEAL: That is my rewrite of that thing.

DR. GRASSLE: I think you can just give it to somebody.

DR. TEAL: Which somebody should I give it to?

DR. MACIOLEK: Why don't you give it to us and we will insert it or mark which page it belongs with.

DR. GRASSLE: Okay. Produced water is "E" and the present "E" is "F." The deck drainage is "G." The present "G" goes out to page 23, and "H" hydrocarbons is as it is.

DR. MILLER: Where did you place "D?"

DR. GRASSLE: The paragraph at the bottom of 25b, produced water, is "D."

DR. MILLER: No, the existing "E."

DR. GRASSLE: The existing "E" just gets changed to "F" and it is where it is. And "F" on 25c becomes "G" and the "G" that is there is going to page 23 and the "H" stays "H," including the first sentence and the first sentence now needs to be fixed up.

(Simultaneous discussion.)

DR. GRASSLE: Say what you have to say about new "F."

DR. KRAEUTER: I think we need to rework how we arrange--

DR. TEAL: I think it needs to say, to start off, "Considering the information" or something like that.

"Benthic effects are not expected or gradual accumulation of hydrocarbon on the bottom that is shown in shallow water." Is that water less than 35 feet deep?

DR. KRAEUTER: 35 feet or less.

DR. TEAL: Less than 35 feet deep.

DR. GRASSLE: Yes.

DR. TEAL: "If production were to occur near a canyon head without further resolution of this issue, possible build-up should be monitored." That makes it clear what we are talking about.

(Simultaneous discussion.)

DR. GRASSLE: "If production were to occur near canyon heads without--near canyon heads--without further resolution, possible build-up should be monitored."

DR. TEAL: "Without further resolution of this issue."

MR. VILD: Should we identify where these hydrocarbons are coming from? Because I think when we talked about hydrocarbons and deck drainage and so on we were assuming that some of the hydrocarbons would come from deck drainage.

DR. TEAL: Deck drainage hydrocarbons are not generally involved because they have had a chance to evaporate on the deck and come out as little tarry things--it does not get down into the water.

MR. VILD: Then what source of hydrocarbons are we talking about here?

(Simultaneous discussion.)

DR. TEAL: It is hydrocarbons from all sources. We do not really know where they come from.

(Simultaneous discussion.)

DR. KRAEUTER: If you look at our outline we were going down that outline and what we had was produced water, and then we had hydrocarbons, deck drainage, et cetera, under operational discharges. That is where it is. All the other sources of hydrocarbons--

(Simultaneous discussion.)

MR. VILD: We should identify where those hydrocarbons are coming from.

DR. GRASSLE: Why don't we put "H" or most of "H"--I guess I was right the first time. That paragraph at the top of 25d without the first sentence should go into "E." I was right the first time. I got confused. It goes into "E" and the present "E," which is produced water on 25b--

DR. KRAEUTER: The new "F"--given 500 meters set-back, it is unlikely that there would be any measurable effects from drilling muds and cuttings on commercial species in heads of canyons.

DR. GRASSLE: Do you want to repeat that slowly?

DR. KRAEUTER: Sure. "Given a 500 meter set-back, it is unlikely that there would be any measurable effects from drilling muds and cuttings on commercial species in the heads of canyons."

(Simultaneous discussion.)

DR. GRASSLE: Maybe "G" should just be "Deck drainage and sewage discharges are minor and therefore do not need to be considered."

DR. VALENTINE: What about putting a little caveat in there about "given the dilution factor"--you know, something about the high dilution that would occur and that these are minor issues.

DR. GRASSLE: "Given the low volume and high dilution, deck drainage and sewage discharges are minor contaminants and therefore do not need to be considered."

DR. VALENTINE: Or will have no effect--is it the feeling that we do not need to consider because we are confident that they will not have an effect?

DR. KRAEUTER: Just say, "will have no measurable impact."

DR. GRASSLE: It will have no measurable impact on the canyons. Are there comments on "I" now? First of all, it is "H," not "I." We got rid of "H," so it is "H" now instead of "I."

MR. VILD: Are we striking "accidental discharges," a as we did for operational discharges?

DR. GRASSLE: Where are we now?

MR. VILD: Right above where it says "oil spills and blowouts," "accidental discharges."

DR. GRASSLE: Oh, yes. Yes, strike "accidental discharges."

DR. VALENTINE: Well, in the third line down--"Because most of the canyons are physically and biologically more active--" I mean, some of these so-called canyons are really like the slopes.

DR. GRASSLE: Good point.

DR. VALENTINE: Then, down at "filter-feeders such as krill, and sedimentation--" include "of fine particles in a depositional environment." It is really the fine particles that we are talking about, right? And "the depositional environment" kind of assumes there is some activity.

DR. GRASSLE: Yes, good point.

DR. BOTHNER: You could also add that more active resuspension of sediments in that list.

DR. GRASSLE: Are you raising a sentence, Mike, or--

DR. BOTHNER: For example, "Due to abundance of filter-feeders such as krill, more intensive sediment resuspension and active sediment accumulation of fine grain sediments--"

DR. GRASSLE: Why would resuspension result in more rapid accumulation?

DR. BOTHNER: Because of the possibility that there are any hydrocarbons in the water column at all.

DR. GRASSLE: Oh, Okay.

DR. BOTHNER: You would have the opportunity for--I mean, that is the only mechanism that anybody has even proposed to get the stuff down.

DR. GRASSLE: Okay.

DR. VALENTINE: The last sentence--"Benthic impacts, if they occur, are more likely to be long-term--"

Would these be major impacts or minor impacts? Does long-term mean that they would not show up a long time or they would not be corrected for a long time?

It is kind of vague.

DR. GRASSLE: Yes.

DR. COOPER: The depths we are talking about--I do not think we know anything about that at all one way or the other.

DR. VALENTINE: Does "long-term" refer to recovery time or what?

DR. KRAEUTER: I think it also comes into what you are talking about with benthic impacts. We talked about how the commercial species

DR. COOPER: At depths of 1,200 feet, I cannot imagine anything... [unclear]

DR. GRASSLE: I think the point was here that the benthic impacts, if they occur, are not likely to show up except as the result of long-term accumulation.

DR. VALENTINE: We ought to say that, then.

DR. BOTHNER: How about "long-term low-level exposure?"

DR. KRAEUTER: Yes. "--are not likely to appear except as the result of--"

DR. BOTHNER: How about just "--are likely to reflect long-term exposure?"

DR. GRASSLE: Yes. "--long-term exposure as a result of gradual accumulation of material" or something like that.

DR. KRAEUTER: That is the key.

DR. GRASSLE: I do not think it is the long exposure. It is likely to be the result of very gradual accumulation of material.

DR. BOTHNER: You do not expect that from an oil spill.

DR. GRASSLE: The whole point is that oil spills over time result in the gradual accumulation, even though any one does not really show up.

DR. BOTHNER: Okay.

DR. GRASSLE: If there is a problem, it is a problem of increasing background. You know, will this environment get up to the 0.3 ppm?

DR. KRAEUTER: Do we have evidence in an environment like this of gradual accumulation anywhere from oil spills of the kind we are talking about?

I know in shallow water I would tend to agree with you.

DR. GRASSLE: I think that in oil fields the background can be slightly higher than otherwise, and that is what we are talking about.

DR. KRAEUTER: Is it produced water?

DR. VALENTINE: It is probably both.

DR. GRASSLE: I think with this document we are saying that it is likely to be the long-term effects of spills.

DR. KRAEUTER: If any.

DR. BOTHNER: How about "the long time cumulative effects of various discharges" or something like that?

DR. GRASSLE: Yes, that is good.

(Simultaneous discussion.)

MR. BOURNE: You could just strike the paren altogether and end it the way you say, "the gradual accumulation"?

DR. COOPER: Don't we make the point earlier in the report, here, though, Fred, "Gradual long-term build-up, given that it may exist, is more than offset by the biodegradation and decomposition?"

DR. KRAEUTER: We do.

DR. COOPER: We made that point very clearly earlier in the report.

DR. GRASSLE: I think the "if it occurs" covers that. All this is saying--we are not really prepared to say unequivocally that there are no impacts, I do not think. At least I am not.

I am saying that if there are, then this is where I would see it happening--as a result of something very low level and long-term.

MR. VILD: That statement of the rate of hydrocarbon accumulation being low and being substantially offset by breakdown processes is on 25c--I do not know whether that is the only place where it occurs in the report.

I just want to make sure it is not something we struck.

DR. GRASSLE: Where?

MR. VILD: It was right in the thing about the gradual increase in hydrocarbon accumulation--the first full sentence, reading "over the years can be expected--" I am just quoting from the language here--"but because the rate is low it may be substantially offset by breakdown processes."

Did we keep new wording or just strike that out?

DR. VALENTINE: No, it stays in.

DR. GRASSLE: We did strike it, and we are going to put it back in, okay?

MR. VILD: I am not sure if that is the only place where it occurs in the report.

DR. GRASSLE: I think it is. We are putting it back in. We did strike "may be offset by breakdown processes."

DR. KRAEUTER: Is breakdown the correct word? Is it decomposition or is breakdown a more general term that is acceptable to everybody?

DR. GRASSLE: I can live with either one.

DR. COOPER: To me breakdown is probably more generic.

DR. GRASSLE: Okay, fine.

DR. BOTHNER: Can we read the sentences as they are to read, now?

DR. GRASSLE: If I can get to where we changed it. "Hydrocarbon accumulations from produced water--"

MR. BOURNE: "Hydrocarbon accumulations in surficial sediments from produced water--"

DR. GRASSLE: Yes. "--are likely to be undetectable." Even after a number of years, since accumulations would be substantially offset by breakdown processes.

(Simultaneous discussion.)

DR. BOTHNER: I hope you have some evidence for that, because I sure do not. Do some things jump right to your mind?

DR. GRASSLE: Can we say "could be?" Actually--no, I do not. I am just responding to the comment. It was a major point that everyone agreed on.

Maybe, from what you are saying, it certainly should be "could be" or should we leave it out?

DR. KRAEUTER: I think John may have been the source of that. John Teal was thinking about microbial breakdown. I think he was the source of that. Maybe we could call him up and ask him.

DR. GRASSLE: At the very least it should be "could" instead of "would."

MR. VILD: The real language is "it may be substantial."

DR. GRASSLE: "Could" is a little short.

DR. KRAEUTER: We ought to check with John on that and see if we can get it. I think he is the source.

DR. VALENTINE: Have we finished with that part?

DR. GRASSLE: Yes.

DR. VALENTINE: Getting back to page 25d, where we were talking about benthic impacts, didn't we have a discussion about the unlikelihood of oil spill material getting to the bottom? Where is that?

It seemed to me we had a long discussion about the fact that most of the stuff from blowouts and oil spills was going to be confined to the surface layers.

MR. VILD: That was in the morning discussion.

(Simultaneous discussion.)

DR. VALENTINE: Should that be included in that to kind of to support the contention that benthic impacts--or to state that benthic impacts are unlikely?

MR. BOURNE: Sort of offsetting that, as I remember, was--maybe that was where this came from--was the statement that, compared with the shelf, the canyon environment was more likely to have transport to the bottom of those various factors.

DR. MACIOLEK: Jerry Neff kept making a point that he did not know about a mechanism to--

DR. KRAEUTER: It is right up here. "No mechanism transports oil to the bottom in any quantity." That does not mean that it does not occur--it is just that we do not have any mechanism to do it.

MR. VILD: Now, what does that statement mean? I could think of a couple of things right away.

DR. MACIOLEK: I think he means that he does not expect the oil to settle in big glops to the bottom.

DR. KRAEUTER: This was for an oil spill. We were talking particularly about--I have talked to Jerry a lot about this.

In all the models and things that are used there are no mechanisms in any of the models that really transport the oil to the bottom after an oil spill that we have any substantial documentation for, although oil does get down there.

The observation that it does, particularly in shallow water, is very real. We cannot explain it. I do not know about deep ones.

MR. VILD: What about just the forming of emulsions that are heavier than the water and just sink? I am guessing. I am not a chemist by any means.

DR. COOPER: The oil tanker that split up on Nantucket shoals ten years ago, the *Argo Merchant*, weren't there documentations of lots of oil on the bottom? Admittedly, this was at shallow depths, and I am not too sure about what depth they are talking about.

DR. KRAEUTER: Yes, you are right. We have it documented but we do not know the mechanism.

DR. AURAND: It was not as much as you get on the surface.

DR. KRAEUTER: Right.

DR. COOPER: It seems to me that at this point it is almost academic. If, in fact, there is documentation of oil from the spill reaching the ocean floor, we need to address that.

DR. BOTHNER: I thought the mechanism--and I do not have this document, either, but I thought the mechanism was that you had lots of suspended matter in the water column and of course the water depth at that location--which was one hull depth of the ship, whatever big ship that was--suspended in that concentration during a storm there very high--we measured that.

There was a lot of turbulence, because it was shallow.

DR. KRAEUTER: This is primarily the mechanism that has been proposed--

DR. BOTHNER: For shallow water? A shallow water problem.

DR. KRAEUTER: But it has not, as far as any evidence I know--it has not been proven.

DR. AURAND: We try--and John and I were talking about this the other day--we have funded a study in Alaska to try to show absorption on particles that transport down in Norton Sound, it is the Yukon River.

DR. AURAND: If there is ever an oil spill here, it is going to get on this stuff, it is going to go straight to the bottom. They could not get the stuff onto the particles.

Now the next question is--that is where they are still hung up--what is unusual about the particles that they cannot get this to work?

We have not been able to make it happen in the laboratory. Something clearly happens.

DR. KRAEUTER: That is what Jerry was talking about there, because he knows those studies, and just the mechanism--he knows it occurs, he measured it.

DR. GRASSLE: Okay, a suggestion for that.

(Simultaneous discussion)

MR. VILD: My initial objection is the way that is worded, "There is no mechanism of transport of oil to the bottom in any quantity." That means that there is no method, or nothing that we have documented. It means that there is nothing--to me, anyway.

(Simultaneous discussion.)

DR. AURAND: I would argue that in that particular case Jerry may have meant more than that, because the operative words are, "What did you mean by in any quantity?"

DR. GRASSLE: That is it. I think it is misleading, in what it is.

DR. AURAND: Most of the oil stays at the surface in deep water.

DR. KRAEUTER: Absolutely.

DR. AURAND: But some of it gets down to the bottom. How much depends on how deep you are and how it gets there--we do not really understand how it gets there.

I think that would be a fair characterization.

DR. GRASSLE: I think we may have taken care of that before. I have a suggestion for this paragraph.

DR. MACIOLEK: The paragraph on 25b?

DR. GRASSLE: Are the rapporteurs ready? Gentlemen? Under "H"--oil spills and blowouts.

MR. BOURNE: I just wanted to remind you of the source of this, which may have been distorted. There was a question yesterday, is there a potential for getting oil into the canyon.

Then out of the colloquy between you and John Teal I got that there is more chance to introduce oil into the canyon than onto Georges Bank, depending on the sediment transport, et cetera, et cetera. That is where that came from.

DR. GRASSLE: It is not right the way it is. "H"--oil spills and blowouts. Delete "A fundamental question is what are--"

Start with "Mechanisms of transport of the products of an oil spill or blowout into the canyon environment are not well known."

DR. GRASSLE: Okay. Is there a problem?

MS. BAYLY: Could we stop for a minute?

DR. GRASSLE: Yes, stop.

(A brief recess was taken.)

DR. VALENTINE: If you say that--stating that they are coming from organisms that are feeding at the surface water, it is not very--

DR. COOPER: I would put the term "krill" in there, Fred, and fecal pellets of the krill.

DR. GRASSLE: "The feeding activity of krill in surface waters results in the production of rapidly settling large fecal aggregates." Beautiful.

I am still not clear on the other point. You are just thinking of a natural rate of particles from whatever source, is that what you are thinking of?

DR. BOTHNER: Yes. I am thinking that the fact that the canyons are a sink for fine-grained sediments.

DR. KRAEUTER: That is the physical process as opposed to the biological one that you just described.

DR. BOTHNER: Maybe the net accumulation--net long-term accumulation of fine-grained sediments in some areas, in some canyons--

DR. GRASSLE: "--and the net accumulation of other large aggregates from the water column."

DR. MACIOLEK: No, that sounds potentially biological.

DR. BOTHNER: How about "the long-term and net accumulation of natural particles from the water column?" Or "particles from the net accumulation of these natural particles from--"

DR. KRAEUTER: Why don't we say concentration? Do they, in fact, concentrate in the canyon?" When you say the net concentration of fine-grained--

DR. GRASSLE: Record this paragraph, now. This is paragraph "H," oil spills and blowouts. It now reads: "Mechanisms of transport of the products of an oil spill or blowout into the canyon environment are not well known. In comparison to the adjacent slope, most of the canyons are physically and biologically more active (for example, the feeding activity of krill in surface waters results in the production of rapidly settling large fecal aggregates)."

DR. BOTHNER: Except that you have not said--

(Laughter)

DR. GRASSLE: "Therefore, over long periods of time there is a possibility that hydrocarbons will accumulate."

MR. VILD: So you are not mentioning any physical impact of the fecal pellets on the krill? The rapid settling thereof?

DR. GRASSLE: It is a poor example and--

MR. VILD: Yes. I realize you are going to have to--

DR. GRASSLE: The difficulty is that, whatever other statement we make is not in parallel with the example of the krill. Any other statement we make refers to all of the particles from whatever source and their long-term accumulation.

DR. BOTHNER: Fred, could I just put a--

MR. VILD: That example does take in the biological and physical effects. I just wanted to mirror the early language of the sentence, that is all.

DR. BOTHNER: Is it possible to get into that phrase in the parentheses, the phrase that says--after the word "krill," something that says "observed at higher concentrations in canyons." That may not be necessary, but as you read it, it seemed to--if you got that phrase in there, that would crystallize it for me.

DR. GRASSLE: Yes.

(Simultaneous discussion.)

DR. BOTHNER: "Observed in extraordinary concentrations over canyons." You know.

DR. GRASSLE: I have just got--there is some other problem here I have to fix up before I read this thing again. It was correct up until we got to the point "biologically more active," just before the parenthesis.

The parenthesis might now be, "(for example, feeding activity of krill occurring in high concentrations in the water column of canyons results in the production of rapidly settling large fecal aggregates)."

Okay. Then I think we need to--

MR. VILD: Hold it, hold it. You are talking about feeding activity of the krill. The phrase "in high concentrations in the canyon"--is that modifying feeding activity or is it modifying krill?

Because if they are feeding in a canyon they are not feeding at the top of the water, where the oil is.

DR. VALENTINE: I think he said water column.

DR. GRASSLE: That is right. "For example, the feeding activity of the high concentrations of krill in the water column of canyons results in the production of rapidly settling large fecal aggregates." Is that right?

Okay. I think we should skip to the last sentence of that paragraph and say, "Benthic impacts, if they occur, are likely to be the result of oil absorbed on the particles, which may accumulate on the bottom over long periods of time--" or "--which might accumulate on the bottom over long periods of time."

Then we should go back to the previous--

DR. VALENTINE: Excuse me. What about saying, "if they accumulate on the bottom over long periods of time."

DR. GRASSLE: Yes, that is better. "If they accumulate on the bottom--"

Then we go back to the previous sentence and say, "Major short-term impacts would occur in the surface layer and at the shelf water-slope water front."

DR. VALENTINE: How about saying, "sea surface layer?"

DR. GRASSLE: Yes. Does that sound right now?

DR. BOTHNER: The only thing that troubles me now is that the shelf water/slope front is a phenomenon that goes all the way to the bottom. That is a plane that cuts through the sea water.

DR. KRAEUTER: It is also a mechanism of transport if you are looking for one.

DR. GRASSLE: Okay. So it should read, "Major short-term impacts would occur in the sea surface layer, especially at the shelf water/slope water front.

DR. KRAEUTER: That is where you like to concentrate things.

DR. GRASSLE: Yes. So that means it modifies surface water.

DR. KRAEUTER: Good.

DR. GRASSLE: Okay. Actually, I think that maybe that "major short-term impacts" should be a new paragraph. Then the next sentence should be "Planktonic eggs and larvae of canyon fauna would be most vulnerable. However, the magnitude of the impact is too situationally dependent to quantify."

DR. BOTHNER: Good.

DR. GRASSLE: So "major short-term impacts" starts that paragraph. Okay. Now we are down to "J," gas blowouts.

DR. KRAEUTER: I think just the first comment gives insufficient information. That is what all those other things say.

DR. VALENTINE: What does gas carry in the way of oil and hydrocarbon contamination?

MR. VILD: I think there is some question about the condensates and I do not really know what that carries, but we in the North Atlantic have always seen these environmental impact statements talk about oil spills and then almost in the same breath say that, however, there is very small likelihood that there is oil in the North Atlantic since the problem seems to be a gas problem.

Our response has always been, "Let's talk about a gas blowout or gas condensates." Just the release of gas condensates, if that ever happens chronically, I am really not aware of it.

DR. VALENTINE: The sense I got here is that during an oil spill a lot of the stuff volatilizes right away, anyway. During a gas blowout, I mean, wouldn't that even be--wouldn't most of the stuff be quickly volatilized?

DR. KRAEUTER: The point is that there are hydrocarbons with the gas, it is not just the gas--much smaller amounts, obviously. What Jim Ray said was that gas blowouts tend to be at the surface or on the platform itself--so wear your hardhat.

(Simultaneous discussion.)

DR. GRASSLE: Maybe it should be something like, "No information exists to suggest special impacts from this source."

DR. VALENTINE: Yes. "No comment" is not good.

DR. GRASSLE: "No information exists to suggest any special impacts from this source."

MR. VILD: What about sufficient information? Because when you say "no information" that also may imply that there is information but it shows no negative thing. In other words, nothing--showing nothing, no effect.

DR. GRASSLE: "Insufficient information exists to indicate--"

DR. MACIOLEK: "--to allow the comment on--"

DR. VALENTINE: Most people do not think there would be a major--you know, their first impressions are that, compared to an oil spill, it would be much less likely to cause a problem. I mean, that is a general feeling, although we cannot quantify it.

DR. BOTHNER: How about, "Insufficient information is available to evaluate this threat--" or "--this concern, but none is expected."

DR. MACIOLEK: Something along those lines is good, "insufficient information--"

DR. KRAEUTER: "Little or none is expected."

DR. BOTHNER: "Little or none." That is great.

DR. KRAEUTER: "None" is pretty strong. Somebody could dream up something.

DR. GRASSLE: "Insufficient information exists to evaluate the possibility of particular impacts from this source."

DR. MACIOLEK: That is good.

DR. GRASSLE: Okay. We are deleting the comments of the individual people--Hughes, Ray and Kraeuter.

DR. KRAEUTER: Absolutely.

DR. GRASSLE: Then "Space Use Conflicts." I think we can probably delete the discussion there, too.

DR. KRAEUTER: No, I think that is very important.

MR. VILD: It is to the States.

DR. KRAEUTER: It is to the States and to the fishermen and people like that.

DR. VALENTINE: What do you mean by discussion, the quotes?

DR. GRASSLE: The big hyphens.

MR. VILD: Oh, okay. You could get rid of those. Oh, yes, absolutely.

DR. GRASSLE: The statement "K" stands.

DR. COOPER: That needs to be modified a little bit. That first sentence is not correct--"These would be minimized in the canyons by the 500-meter setback."

At the spring and early summer, when these lobsters are moving out of the canyons and inshore, any kind of a rig with a total of 2 miles of anchors set out is going to restrict setting the long line that can be more than 2 miles in length, with 80 to 100 traps per trawl line.

DR. KRAEUTER: I tried to work on this a little bit. "The canyons represent a large fraction of the fishing grounds for some species. The 500-meter setback would minimize a portion of this space conflict. Anchor lines could occupy a large fraction of the preferred fishing area near a given canyon. Some accommodation could be made by industry-to-industry coordination."

DR. VALENTINE: What about qualifying the anchor lines as being only during exploratory drilling--

DR. KRAEUTER: That is a good point.

DR. VALENTINE: It could be a temporary facility.

DR. GRASSLE: That sounds good to me.

MR. VILD: Now, wait a minute. During production the platforms are anchored also.

DR. KRAEUTER: Not by those anchor long lines, though.

MR. VILD: If you have a guide tower, I understand that the anchor lines are even longer--or maybe they are just deeper into the sediment.

DR. COOPER: We were told yesterday--I asked the same question and we were told that production operation would probably be done by a large fixed-legged platforms, hard in contact with the bottom. They do not require these 8 or 10 or 12 anchors out at these 1-mile distances.

DR. KRAEUTER: We could weasel it.

MR. VILD: They have something that can anchor like that in 200 meters of water?

DR. KRAEUTER: The new platform they put in the Gulf is much deeper than that.

DR. COOPER: They are impressive.

DR. GRASSLE: Okay. Are we okay on John's--

DR. KRAEUTER: Do you want me to read it for the record?

DR. GRASSLE: Okay.

DR. KRAEUTER: "Canyons represent a large fraction of the fishing grounds for some species. The 500 meter setback would minimize a portion of this space conflict. Anchor lines could occupy a large fraction of the preferred fishing area near a given canyon. Some accommodation could be achieved by industry-to-industry coordination."

DR. GRASSLE: You could have a "however" before the "could represent."

DR. KRAEUTER: All right. That covers some of those things.

DR. GRASSLE: Then I suggest deleting the comments after the long hyphens.

DR. KRAEUTER: Yes. I tried to incorporate some of that in there.

DR. GRASSLE: Then, under "Needs," there was a comment earlier to incorporate "Needs" from page 9, but the problem exists that the three that were mentioned here were perhaps of more concern than some of the ones that were listed on page 9.

So maybe we should attempt to prioritize these.

DR. MACIOLEK: I think what exists as written on page 9, though, is going to come out of that--if anything remains from page 9 it is going to be incorporated here.

DR. KRAEUTER: So we just try to rebuild it?

DR. GRASSLE: Is there anything on--let's pull out of the things on page 9 things that someone feels strongly should be added to the list of "A," "B," "C."

It seems to me that a lot of the things like geochemical and textural, et cetera, are taken into account by "A." "A "is a pretty all-encompassing concern. Now, whether it is too general is another issue.

DR. MACIOLEK: There is nothing on page 28 that addresses any biological data limitations or needs.

DR. VALENTINE: I want to alter "B," and that would include a little bit of that.

In "B," if we are ready--are we ready to talk about "B?"

DR. GRASSLE: Go ahead.

DR. VALENTINE: I would like to say that some canyons may not meet the biological criteria. It seems like these canyons are heavily described under "Biological Criteria." We are talking about their uniqueness biologically and that kind of stuff.

I would say that some canyons may not meet the biological criteria defined by the group, and be similar faunally to the slope, and then the other sentence I did not change.

It seems that the canyons have lots of attributes, but the biological aspect of it is the one that is really critical for oil and gas impact.

DR. GRASSLE: Okay. Maybe that "B" should be then, "More information is needed to specify the special biological characteristics of canyons."

DR. VALENTINE: Well, the idea is that some of these places called canyons do not fit the biological criteria of the large ones that we have studied.

DR. GRASSLE: "More information is needed to specify which canyons--"

DR. COOPER: "--are important from the fisheries point of view."

DR. GRASSLE: No. "--have special biological characteristics."

DR. COOPER: I put in the term "fisheries." I think it is more pragmatic.

DR. KRAEUTER: You could define some that might not have a fishery but would have the kinds of corals and things like that and still probably meet the definition of canyon and just might be really hard and so you would not have the grottos and stuff. I am not saying you should exclude it.

DR. GRASSLE: "More information is needed to specify which canyons have fisheries or other special biological characteristics."

DR. VALENTINE: "--and which do not."

DR. GRASSLE: "--and which do not."

DR. VALENTINE: See, some of the canyons might be just like the upper slope. There might be a red crab fishery on the upper slope in this little canyon, so it has got a fishery but--

DR. GRASSLE: I understand. Can that one sentence be all of "B?" Is that okay, Page?

DR. VALENTINE: Yes.

DR. GRASSLE: So we are deleting the first sentence and the whole thing is--

DR. VALENTINE: No--wait a minute. Oh, you want to delete the first sentence?

DR. GRASSLE: Yes, as a question.

DR. MACIOLEK: Page had suggested a revision of that sentence.

DR. VALENTINE: In our characterization of canyons, is there a biological--is somebody writing--Dick, you and Barbara wrote that up?

DR. COOPER: Fred and Nancy are going to add some infaunal--

DR. VALENTINE: So that really is our characterization of what we think of as canyons that should be protected, et cetera. I think that first sentence kind of sets the scene for the second sentence.

DR. GRASSLE: Okay, and that is amended to say--

DR. VALENTINE: It says that some canyons may not meet the biological characterization or whatever defined by the working--

DR. GRASSLE: Okay. So it is now, "Some canyons may not meet the biological criteria defined by the group and be more like the slope. More information is needed to specify which canyons have fisheries or other special biological characteristics and which do not."

DR. VALENTINE: Okay. What about saying upper slope?

DR. GRASSLE: The upper slope biological criteria?

DR. VALENTINE: No, no. "--and be more like the upper slope."

DR. GRASSLE: Oh, "--more like the upper slope."

DR. COOPER: That is really a comparison.

DR. GRASSLE: Yes, I understand.

MR. VILD: Let's add the word "may" between "and" and "be" in that sentence.

DR. GRASSLE: Okay.

DR. COOPER: Fred, if we are off this specific item "B" here, I would like to ask a generic question.

To anybody's knowledge has there ever been a reasonably comprehensive before-during-and-after study of a production platform? I know a lot during--early put together during and some after--

DR. GRASSLE: I think that that is the whole point of the California program--to see what the effects of a production field are on communities.

DR. COOPER: Is that actually going to be funded or is that underway?

DR. AURAND: It is underway.

DR. AURAND: The problem is that there has been difficulty in getting permits to install the platform, and what--we thought by this time we would have had 2 or 3 years of pre-platform data and be into the placement of the platform phase, and we are not.

So that whole study is being rethought right now, because we are ending up with this 5 or 6 years of pre- data, which is not a bad thing to have 5 or 6 years of pre-platform data--

DR. COOPER: It is probably good, as a matter of fact.

DR. AURAND: Yes.

DR. COOPER: The problem is that nobody can tell us when the platform is ever going to get permitted, and so we do not know whether we are going to continue yearly--we are doing quarterly sampling and it costs a lot of money and we cannot tell how long we will have to do this before something happens.

DR. KRAEUTER: You may not want 10 years of that data.

DR. AURAND: So we are trying to figure out what we will do with that.

Now, as far as the same platform in all three of those places--since they put a platform in for 20 or 30 years--there is no instance where we have documentation of exactly what was there before they put them in, what happened while they were putting them in, what went on while they were in operation and what happened when they took them out.

That does not exist anywhere.

DR. COOPER: My obvious reason in asking is, first of all, a comment. I look at that narrow shelf environment out in California as being totally different from our environment here--two very different worlds in many respects.

It is a production platform--a large production platform with 30 to 50 to 60 pipes going down--if that is ever likely to occur in our area, at our research center in Connecticut we are definitely going to do a report during that to study impact. If I ever see a situation that requires it--

DR. AURAND: Remember what "after" in this situation is. "After" is 30 years later, and that is the key to this. We have tried in some cases to look in the Gulf at areas with and without platforms and that kind of thing, but we have not even figured out what we are going to do in California--if we are going to take it through actual production and monitor it after they are done drilling all of the wells and then for a couple of years, probably, when it is in operation.

Whether or not we would be even around to see what happened thirty years from now--

DR. COOPER: I should not say "after." However, there is no area of our shelf that is known as extensively as two or three of these so-called Georges Bank submarine canyons. They have received a lot of effort.

With this kind of a database, a benchmark we have over the last 10 or 12 years--8 to 10 years--to me it would be criminal not to have--it does not even have to be a difficult area and most likely would not be--but it is obvious to think that we know exactly to go about setting up such a thing from the years of experience we have had.

I am curious as to what other people's gut feeling is here. I am not asking for MMS support. I think that this is something that would be very worthwhile doing. If you agree, regardless of who funds this, I would suggest that such a needs statement be formulated.

DR. AURAND: MMS' emphasis right now is on long-term impacts associated with production and development because, if you look at where most of the controversy remains, it is on chronic low-level, long-term impacts, not really what is cuttings to any extent anymore.

So in most areas where there is production development we are trying to do monitoring-type studies. We have one already underway in California. We have an attempt to design some in the Gulf of Mexico--although that is much more difficult because of the situation with the length of time that we should have been there.

DR. COOPER: You are not starting off with a pristine environment down there.

DR. AURAND: No, but we have a plan in place for implementation in the Arctic in the Beaufort Sea, and should, in that case, even exploration begin, but certainly should production and development begin.

So it is reasonable to presume that if there were production there would be some effort to look at the long-term consequences of that production--not meaning yearly monitoring for 30 years necessarily, but certainly up through the placement of the platform and what happens immediately after that.

Exactly what form that would take I cannot say, but it is not an unreasonable thing--and, in fact, depending on what had happened, we may or may not still think it is important. It depends on what kind of things we find out at some of the other areas prior to something happening in New England, if it ever does.

DR. COOPER: I think we ought to go ahead and do this, regardless of what we find in these other areas.

DR. AURAND: Production and development seems to us to be the place where we want the most information now, and that is not consistent with what you just said. It is also not a statement that we necessarily would do it for one platform in an area. However, clearly that is the area we are most concerned about. I do not think we would have a problem with that issue.

DR. GRASSLE: I have a suggestion for "A" under "Needs." It might read, "Rates of accumulation, flux and deposition of particulate material are poorly known." It is funny, we have "of materials" after "flux," and it should be "Rates of flux, deposition and accumulation--" is what it should be.

DR. BOTHNER: That is a little redundant, Fred. "Rates of flux--" May I suggest an alternative?

DR. GRASSLE: Yes, please.

DR. BOTHNER: Let me just read it fast to give you the flavor. "Data on the rates of sediment accumulation and flux of associated contaminants is needed in the canyons of the North Atlantic area. The lack of this basic information inhibits our ability to make definitive conclusions."

DR. GRASSLE: That is excellent. Would you read it again?

DR. BOTHNER: Here we go. "Data on the rates of sediment accumulation and flux of associated contaminants is needed in the canyons of the north Atlantic area. The lack of this basic information inhibits our ability to make definitive conclusions--"

You could say, beyond that, "--regarding the impacts of petroleum development on sensitive canyon areas."

DR. KRAEUTER: Could you read it one more time? Just the first part?

DR. BOTHNER: Data on the rates of sediment accumulation and flux of associated contaminants is needed in the canyons of the north Atlantic area. The lack of this basic information inhibits our ability to make definitive conclusions."

DR. KRAEUTER: I am more concerned with the first part, "rates of sediment accumulation and flux of contaminant material--"

DR. BOTHNER: "--flux of associated contaminants that is associated with the sediments."

DR. KRAEUTER: Don't we need to know something about the flux of things that are not necessarily contaminants, too? Obviously, contaminants are important and that is what we are trying to deal with.

We have got sediments and contaminants and there are things--I keep thinking, what is the source of the food for all of this added biomass? Something has got to be going on there. We have not quantified either of those.

DR. BOTHNER: Shall we say "materials" instead of "contaminants?"

DR. KRAEUTER: I do not want to--what is the background for the things we are trying to do? I mean, you might have a flux of something if it was coming from a well and you would call it a contaminant, but if it was coming from a biological concentration mechanism you might not call it a contaminant. I don't know.

I would rather use "material" but I do not want to lose sight of the fact that we really are after the contaminants.

DR. GRASSLE: "Contaminants are likely to be associated with particles." Then it should be "Particulate flux and accumulation of sediments--the rate of particulate flux and accumulation of sediments is poorly known." Then the sentence that you had, Mike--

DR. BOTHNER: Let me try something else, just for fun, to see if I can get around this one. Leave the first part alone.

"Data on the rates of sediment accumulation and the flux of contaminants potentially associated with those sediments, is needed." Do you like that? Is that--we sort of step out of the--

DR. KRAEUTER: Suppose the contaminants are not associated with those sediments?

DR. BOTHNER: Well, we are really talking about contaminants that are associated with sediments. That is really the thing.

DR. KRAEUTER: Our fecal pellet sediments?

DR. BOTHNER: Sure.

DR. GRASSLE: My problem with it, Mike, is a slightly different one. That is that particle flux includes more than just accumulation on the bottom.

DR. BOTHNER: It does.

DR. GRASSLE: It is really that that is a concern for biology. I want to try to get in the whole problem--that we do not know anything about sediment dynamics and particle flux in these areas.

DR. BOTHNER: How about "data on particle dynamics?"

DR. GRASSLE: "Particle flux and sediment dynamics."

DR. BOTHNER: "Data on particle flux, sediment dynamics, the rates of sediment accumulation and the flux of potential contaminants." That is all right.

DR. GRASSLE: "The potential accumulation of contaminants."

DR. BOTHNER: Yes, that is good.

DR. GRASSLE: "Potential accumulation of contaminants."

DR. BOTHNER: All right, remind me.

DR. GRASSLE: Can you write that on the board, Mike? "Particle flux, sediment dynamics and rates of sediment accumulation." Is that what you had?

DR. KRAEUTER: "Rates of contaminant accumulation" rather than "sediment accumulation."

DR. MACIOLEK: That is why we have to write it down--we cannot remember.

DR. BOTHNER: Well, the problem is that sediment dynamics includes rates of sediment accumulation.

DR. MACIOLEK: Well, then, say "including."

DR. BOTHNER: How about "data on sediment transport?" How about that?

DR. GRASSLE: The reason I like "particle flux" is that to me that implies the vertical flux. Why don't we put in "vertical particle flux?"

DR. BOTHNER: "Vertical particle flux, sediment transport, rates of sediment accumulation--

DR. KRAEUTER: There may be a horizontal flux.

DR. GRASSLE: Yes, but that is in transport.

DR. BOTHNER: That is in transport.

DR. GRASSLE: I think a parenthesis--"(associated contaminant and potential contaminant accumulation)."

DR. BOTHNER: "Data on as needed in the canyons of the North Atlantic area." Then, "The lack of this basic information inhibits our ability to make definitive conclusions."

Then the other question I had to the group is, should we expand all kind of conclusions that that pertains to, like--

DR. GRASSLE: I think we should leave it.

DR. BOTHNER: Just leave it? Okay.

DR. GRASSLE: A suggestion for the parenthesis--why don't we say "processes which determine flux of contaminants?" Or, no, "which determine availability of contaminants to the biota."

DR. BOTHNER: How about "processes which determine the effects of contaminants?"

DR. GRASSLE: Right, "processes which determine the fate and effects of contaminants." Just "processes which determine the effects of contaminants." We have just got this sentence hammered out.

DR. MACIOLEK: One more sentence.

DR. BOTHNER: How about "which influence?"

DR. GRASSLE: Yes, "which influence." Quickly, does anyone have a problem with "C," because we are a good stopping point.

DR. KRAEUTER: I think we were talking about something to "C."

DR. GRASSLE: Okay, good, we are stopped.

(A luncheon recess was taken.)

AFTERNOON SESSION

DR. GRASSLE: Okay, will you read for the record, Mike?

DR. BOTHNER: For the record, "Data on vertical particle flux, sediment transport and rates of sediment accumulation (processes which influence the fate and effects of contaminants) are needed in all of the canyons of the North Atlantic area."

DR. KRAEUTER: No, I do not think we can say "all."

DR. BOTHNER: "The lack of this basic information inhibits our ability to make definitive conclusions."

DR. KRAEUTER: I think "all" is overkill. It would be nice.

DR. GRASSLE: There was passed out a page 25d which was our just agreed version of "H"--oil spills and blowouts. I made one tiny change in the end of the paragraph where there are two "ifs" and so I got rid of the second "if."

That brings us to "Needs" "C."

DR. VALENTINE: I have modified that. I have got a version I would like to pass out.

I tried to make this a little bit more specific--the physical information. You can see how I changed that. I expanded the needs and why we need the information.

It seems like we have somewhat of a good foundation for understanding what is happening in these canyons, primarily from two canyons, but with less information from parts of other canyons. There seem to be certain patterns and certain processes dominating certain areas.

We really need more information from a variety of the canyon types to put it together and really make it useful for predicting any future impacts.

DR. AURAND: Can I offer a suggestion for the last sentence?

DR. VALENTINE: Sure.

DR. AURAND: "Comprehensive studies in a variety of canyon types would improve the ability to predict--" or something like that.

You clearly have made predictions and what you are really talking about is the ability to improve the ability to do so. You know, you can figure out how strongly you want to word that.

DR. VALENTINE: What was your suggestion? "--would improve the ability to predict potential impacts and--"

DR. GRASSLE: Do you want to say "--to predict and avoid--"?

DR. KRAEUTER: No. Studies would not help to avoid. They would just help you to predict. It is two separate steps. Study will not help you avoid. The study will help you predict so you can design something to avoid.

(Simultaneous discussion.)

DR. VALENTINE: How does it read now?

DR. GRASSLE: "--would improve the ability to predict potential impacts." Do we want to have "--on canyon biota?" I like leaving it in.

DR. VALENTINE: That is the main thing we are talking about.

DR. GRASSLE: Good. Now we are going back to page 9 to see if there is anything that we haven't covered in A, B or C that needs to be added in.

(Simultaneous discussion.)

DR. VALENTINE: I think we ought to discuss making the suggestion that, if there is exploratory drilling in one of these canyon rims in the future, that MMS undertake an investigation before, during, and after.

DR. COOPER: Before and during.

DR. VALENTINE: Exploratory would be before, during, and after.

DR. KRAEUTER: I do not think we ought to lock them into it.

DR. GRASSLE: We are not locking them into anything, because they are the ones that make the decision. You know, we can--that would be a definite need to take that opportunity to see if anything happens.

DR. MACIOLEK: As a recommendation.

DR. GRASSLE: Yes. They do not have to take our recommendation. I would not limit it to exploratory--I would just say drilling activity.

DR. VALENTINE: Or near a canyon rim.

DR. GRASSLE: Just drilling activity near a canyon rim.

DR. VALENTINE: We are talking about the north Atlantic.

DR. KRAEUTER: I would not want to restrict them to that. I would rather say canyon rim--and if they happen to get one in the mid-Atlantic--

DR. GRASSLE: If drilling activity occurs in close proximity to the--what was that word--margin or--canyon boundary--it should be monitored.

(Simultaneous discussion.)

DR. VALENTINE: Is that what we mean? Take a few samples?

DR. GRASSLE: No.

DR. COOPER: From the fisheries point of view, I think that you would certainly want to add some process-oriented aspects to that.

DR. KRAEUTER: What would the States want?

MR. VILD: I would think so, but although we would like to see an emphasis on fisheries it would not be the only thing that would be interesting.

DR. BOTHNER: Instead of just mentioning, prior to exploratory drilling, why not say "prior to lease sales"--you know, that just precedes the drilling by a certain amount of time, which is of course of the essence.

DR. GRASSLE: If you say drilling activity, it includes leasing.

DR. KRAEUTER: Lease sales are too far in the future, you really do not know what you are talking about there.

DR. BOTHNER: They won't be drilled before they are sold.

DR. GRASSLE: Drilling activity is the only thing we looked at.

DR. KRAEUTER: You could have a lease sale and the companies may choose not to drill, and they just give it back. It is wasting our money doing studies where nothing is going to happen.

DR. VALENTINE: I think that, as far as this applying to anything, that they would fulfill the "obligation" by studying drilling near any canyon in the world doesn't really apply, because our whole discussion has been directed toward the north Atlantic submarine canyons, and they differ--you know, California, the middle Atlantic, the north Atlantic. They are different.

They have different energetics, different sediment types, that sort of thing. So if they did a study on some Gulf coast canyon and then decided to drill a hole--

DR. GRASSLE: I think it has to be qualified to the North Atlantic, because that is what our report is about.

DR. VALENTINE: Right.

DR. KRAEUTER: I am not willing to say that. I am judging from the view of the scientific committee right now. What happens is that happens on everything [sentence unclear].

Okay, so we say the north Atlantic. Then we come out, okay, what about Lydonia versus some other canyon? Because they are different? How different does it have to be before we cannot extrapolate some of the important processes?

DR. VALENTINE: I would say the Gulf coast province is radically differ--

(Simultaneous discussion.)

DR. KRAEUTER: What about the mid-Atlantic?

DR. VALENTINE: The mid Atlantic is generally similar except for the tidal velocities and the current velocities.

DR. GRASSLE: What did North Atlantic mean when we said North Atlantic in the title of our report? I was thinking of something involving the northeast.

MR. VILD: I think what--it would be the North Atlantic planning area, which very interestingly stops at the Rhode Island/Massachusetts border. We are officially a Mid-Atlantic State, and yet we are part of the North Atlantic Regional Technical Working Group, not the Mid-Atlantic working group. So there you go.

I think it is mainly to reflect where the lease sales are in the north Atlantic.

DR. GRASSLE: Maybe we should say North Atlantic and Mid-Atlantic.

DR. KRAEUTER: There is not going to be that many. We might as well get some information from somewhere. We may not do this for any other area.

DR. VALENTINE: They do not ever have to do it.

DR. KRAEUTER: We do not want to recommend just this area and then say it has not been recommended, therefore we are not going to do it anywhere.

DR. VALENTINE: This does not require them to do anything.

MR. VILD: I personally would like to see, if it is available, more information from the Hudson Canyon, because that is where they had--it was almost a commercial find, we were told, of gas.

DR. VALENTINE: If drilling occurs.

MR. VILD: If there is any sort of impetus for production in the whole Atlantic, it is going to be right there.

DR. GRASSLE: "If drilling occurs close to the boundary of a submarine canyon in the north Atlantic or middle Atlantic regions, processes associated with potential impacts should be studied."

DR. VALENTINE: How about saying "drilling is planned" instead of "if drilling occurs?" That way you can start--what we want is a study that has a before and during, at least.

DR. KRAEUTER: If they are going to study it, I can tell you scientifically they would recommend that they do it beforehand. They usually start these beforehand, that is just almost a matter of record. When they are going to do a study they get [data] before. I don't think we need to tell them that.

DR. GRASSLE: Instead of "occurs" it should be "is to occur."

DR. MACIOLEK: Or "is planned."

DR. GRASSLE: Somehow planning sounds too nebulous.

DR. KRAEUTER: "Is to occur." You have got this whole thing in California right now. They started that study and have 5 years of data now. It is costing millions and you are standing there saying, okay, now what do we do? Do we continue this for another year, figuring that they are going to--

DR. MACIOLEK: On the other hand the Georges Bank monitoring program--the first samples were taken a week before drilling started. That is too close. If you do not understand the processes you need longer than a week lead time.

DR. COOPER: Actually, the samples were taken about 14 months before.

DR. MACIOLEK: Those samples are not part of the MMS Georges Bank Monitoring Program.

DR. COOPER: They are a very important part of the whole study. Where some studies have gotten started before MMS shifted out of first gear.

DR. GRASSLE: I like the word "to occur," as well, because planning gets to have a technical meaning at times, too, and you find bigger problems.

MR. VILD: That is right, because we do talk about the planning area, and it is every single bit of federally-owned real estate out there.

DR. GRASSLE: Okay, so we have that one.

DR. KRAEUTER: Or you could put in something like "sufficient lead time to establish pre-drilling activities" or something. You could put something nebulous like that, "with sufficient lead time to establish pre-drilling conditions."

DR. VALENTINE: So how does it read now? Have we got something written down?

DR. MACIOLEK: I wrote it down.

DR. GRASSLE: "If drilling activity is to occur in proximity to a canyon boundary in the North Atlantic or Middle Atlantic areas, processes associated with potential impacts should be studied."

DR. VALENTINE: What about the lead time? Sufficient lead time?

DR. MACIOLEK: Is that clear enough about what we mean?

(Simultaneous discussion.)

DR. GRASSLE: I do not think we can do better than that. To really be sure we should to start now.

DR. KRAEUTER: As a Scientific Committee member I would not recommend it. I could not, in good conscience, looking at this limited amount of money and all the area that I have to cover. You have got to look at this as a national program, not just a regional thing.

DR. VALENTINE: Was that "D?"

DR. MACIOLEK: Yes.

DR. GRASSLE: Is that okay now?

MR. VILD: What did you decide about the lead time question?

DR. GRASSLE: We dropped it.

Okay. Are we ready to go back to page 9 to see if there is anything that is lost?

DR. MACIOLEK: What is on page 9 seems specific now, and we seem to have written--

DR. GRASSLE: I think we have these covered. My own opinion is that it is all covered. The only thing that might be left out is some of the rather specific geochemical points, whether they may need to be somehow incorporated into the new "C"--if there needs to be some specificity in the new "C" as regards geochemistry.

DR. BOTHNER: Extensive geochemistry is a given in all of these things, isn't it?

DR. GRASSLE: Geochemistry is specified in "C" at the moment.

DR. KRAEUTER: I think everything else we have left rather vague so that it could be planned properly. If you start doing that, people are going to want to go back to the other ones and say, "What did we mean by--" and we are going to get into a research proposal.

DR. MACIOLEK: We might get some comments back from the people who aren't here today, too.

DR. GRASSLE: Okay. Are there any other concerns?

DR. MACIOLEK: Any minority opinions? Yes. I guess not from this group.

DR. GRASSLE: Okay. We have done it.

DR. VALENTINE: Now, shall we fit these other pieces together? Characterization--that little thing that I wrote about -.

DR. KRAEUTER: Do we need to look at these--

DR. GRASSLE: Oh, yes. Has anyone specific comments on these?

DR. BOTHNER: On the 100 words?

DR. VALENTINE: I made some editorial changes on mine.

DR. GRASSLE: Yes. We accept those.

DR. AURAND: Do the rapporteurs understand where they go?

DR. MACIOLEK: Yes. We spoke with Jim about it.

DR. BOTHNER: I have a few more of mine.

DR. MACIOLEK: You and I had better read that.

MR. VILD: I have a couple of questions on yours, Mike. You talk about scavenging but you do not really define what it is. If this is supposed to be going out to the person who knows something about Georges Bank and submarine canyons who is not a scientist, that particular term might be unfamiliar.

DR. KRAEUTER: I think you are going to have to leave it to the wordsmiths over here, really, because there is a lot of that in many of these.

The average person is not going to understand "endemic"--there are all kinds of words in here. I was looking at Dick Cooper's. Is anyone looking at fisheries? What is the word terminal all the way down--what is a terminal shelter? I do not even know.

(Simultaneous discussion)

DR. KRAEUTER: What is the word terminal? What is a terminal shelter?

DR. MACIOLEK: I think he just means a terminal or grotto.

DR. KRAEUTER: A terminal means a place where there are more of them congregated such as a grotto?

DR. MACIOLEK: I am guessing that a terminal is--

DR. KRAEUTER: I do not know what the term means.

DR. MACIOLEK: You know, like a bus terminal.

DR. KRAEUTER: Now I understand it.

MR. VILD: Just trapping instead of scavenging? I guess my question is, what is being scavenged?

DR. MACIOLEK: "Baited hoods" is probably "baited hooks."

(Simultaneous discussion.)

DR. GRASSLE: Which one are you on now?

DR. VALENTINE: We are on Mike's.

MR. VILD: Yes, trapping would be good.

DR. GRASSLE: What line of Mike's is this?

MR. VILD: Oh, I am sorry. My only comment, if that is what you are referring to, is the word "scavenging." It is not really defined. Mike suggested that we change "scavenging" to "trapping."

DR. GRASSLE: I am asking which document?

MR. VILD: This is on sediment resuspension and potential for pollutant scavenging.

DR. BOTHNER: Actually, as I review this, I inserted some sentences that talked about sediment traps. That makes that word not so good, the word "trapping."

DR. GRASSLE: Can we include scavenging by particles? Would that be clear?

MR. VILD: Well, I guess this is the problem because, as I just mentioned to Mike, it is not readily apparent, number one, what is being scavenged and, number two, who is doing the scavenging.

If you talk about particles scavenging--

DR. GRASSLE: It is pollutants that are being scavenged and particles that are doing it.

MR. VILD: Right.

(Simultaneous discussion.)

MR. VILD: Yes, if you could just have a modifier. Scavenging is a nice, colorful word and everything, but I think the lay person has a different idea of what scavenging is.

DR. GRASSLE: The trouble is that it--

MR. VILD: You have an animal doing something, you know, and that is not really the case.

DR. BOTHNER: How about sediment resuspension and the potential for pollutant adsorption by particles?

DR. GRASSLE: Yes.

DR. BOTHNER: Do you like that better?

DR. KRAEUTER: The public is not going to understand it. They are not going to have any idea what you are talking about.

DR. GRASSLE: Oh, boy.

DR. KRAEUTER: I mean, can you give that to your local congressman and think he could read it by himself, without having his staff there?

DR. AURAND: Well, you do not read anything without having your staff there.

(Laughter)

DR. GRASSLE: How about sediment resuspension and the potential for pollutant transport on particles?

DR. KRAEUTER: Yes.

MR. VILD: I am sorry--what was that again, please?

DR. GRASSLE: The potential for pollutant transport on particles.

DR. VALENTINE: I wrote a little thing we talked about, talking about what canyons are and regarding their extent. We were going to focus mainly on the shallower parts of the canyons in this workshop.

DR. GRASSLE: As I saw it--okay, there were two statements, one that was going to appear at the beginning of this section--the day two section--and there was another one which was to be a very broad definition of canyons, to be at the beginning of the whole document.

DR. VALENTINE: Should I read it? I do not have copies of it. This is to let them know that we know about what canyons--

DR. GRASSLE: I do not think there is anyone here who is going to quibble about it, so I would just--let's identify it and give it to the rapporteurs now. This is be at the head of the whole document to define what a canyon is. Page is going to write another one.

DR. VALENTINE: I already wrote the 100 words.

DR. GRASSLE: I know, but are we aware that we have this thing on the typical canyon at the beginning of the page--here, let me give you the page.

DR. MACIOLEK: That is what he just wrote.

DR. GRASSLE: This should go on page 1 of the second day. It should go between the two paragraphs on page 1, before the first illustration.

DR. VALENTINE: Is that illustration going to remain like that?
DR. MACIOLEK: I think it would be useful to have such an illustration. It is obviously going to be a bit more professional.
DR. VALENTINE: It has got to have some labels, too.
DR. MACIOLEK: It should not be just a little cartoon. Do we have any graphics people? I guess if we want an illustration we will have to find something that could be used.
DR. VALENTINE: Maybe I can work something out.
DR. GRASSLE: Who sketched this?
DR. VALENTINE: Brad just sketched that on an overhead.
DR. GRASSLE: I think also that maybe on the 500 to a 1,000 some dash lines to indicate a little flexibility there.
DR. VALENTINE: What's that? On the 500 to a 1,000? That label, you mean?
DR. GRASSLE: I guess it is all right. It is okay.
DR. VALENTINE: I am going to put some proper labels on that continental slope and the 200-meter isobath. I will clean it up a little bit.
DR. GRASSLE: We are copying that. One of the things, Page, that I thought of was right at the beginning of the whole thing, was to have a two sentence dictionary definition of a canyon.
DR. VALENTINE: The trouble is, if you look up a canyon in the dictionary you are going to find a feature that we are not talking about box canyons out west.
DR. GRASSLE: Even if it is totally original, could you make one?
DR. VALENTINE: Okay, or I could just add that. Where would that go?
DR. GRASSLE: I think it should go right at the beginning of the document, what a submarine canyon is.
DR. VALENTINE: Okay.
DR. MACIOLEK: Maybe it is defined in the Georges Bank Atlas or something.
DR. VALENTINE: It might be.

DR. GRASSLE: Actually the first sentence of the document, "A submarine canyon is--"

DR. VALENTINE: The first sentence of the first day.

DR. GRASSLE: Yes, which is page two.

DR. VALENTINE: Just like one sentence or two sentences?

DR. GRASSLE: Yes. When we say--it needs to be something so we say later we are worried about these things that are not canyons, they will have an idea why they are not canyons.

DR. VALENTINE: All right.

DR. GRASSLE: Does anyone want more time to read the 100-word things? Any further comments?

MR. VILD: I have made a bunch of editorial changes on Dick Cooper's thing called "Submarine Canyons as Special Environments." I do not know whether I should present it to you folks now or just submit it to the rapporteurs.

DR. GRASSLE: Why don't you give it to Nancy, because she has a bunch, too.

MR. VILD: Why don't I give it to you now.

DR. GRASSLE: We can get it Xeroxed.

DR. MACIOLEK: Fred, this is the one we need to add a little bit to anyway.

DR. VALENTINE: Does anybody have any major criticism of this one? The one that is going to go on the second day? The one you just handed out?

DR. BOTHNER: We might as well get that out of the way if we are going to change it.

DR. GRASSLE: This is going to go between the first two paragraphs of the page 2 part of the document.

MR. VILD: Maybe I am stressing this too much, but again, if a lay person is reading this he is going to be a little confused about what exactly a shelf/slope break is, or an isobath.

Maybe I am just talking down to whoever is going to read this, without really realizing who is going to read it. The chances are that the person who really does read this will be somewhat familiar with these terms, but then again maybe not. I think maybe I lean too far in the direction of trying to make it simple.

DR. KRAEUTER: I have a little problem with that 200-meter isobath, because most of the time during the document we are comparing the canyon to the slope and the shelf/slope break we defined as 200 meters and we were taking everything above that, then it is all shelf that we should be comparing to and not the slope by definition.

DR. VALENTINE: I do not quite follow that.

DR. KRAEUTER: Maybe I am just confused.

DR. VALENTINE: Inside the canyons there are greater depths than 200 meters.

DR. KRAEUTER: Oh, I see what you are saying.

DR. GRASSLE: I think that that can be solved when we do that figure. We will just refer to the figure there.

DR. KRAEUTER: Okay.

MR. VILD: I take back what I just said before, because I see now. On the third line you mentioned the shelf breaking down onto the continental slope.

DR. GRASSLE: Maybe we can solve the problem of what the shelf and slope are by identifying that.

DR. VALENTINE: We do not want to get into the problem of saying the boundary--the shallow versus deep boundary is the plane that passes through the 200-meter isobath, perpendicular to the trend of the canyon.

DR. GRASSLE: For the record, Page is going to send up a figure with the appropriate labels to go with this paragraph, which will substitute for figure 1 in the text.

So then, that means that the second paragraph on page 1 will be, "The illustration presented is--" it has already been referred to in the definition.

DR. BOTHNER: Are we still on that paragraph on submarine canyons?

DR. GRASSLE: Go ahead, Mike.

DR. BOTHNER: On that particular page, the last two sentences of the first paragraph--"These studies have shown that canyons do not exhibit--"

I would say, "These studies have shown that canyons exhibit widely different sedimentary environments."

Then the next sentence--"Sediment texture, the intensity of bottom currents, and the predominant sedimentary processes are variable from canyon to canyon."

DR. VALENTINE: The point I was trying to make is that you do find similar sedimentary environments in some of these canyons in part, that no canyon is identical to another one. in other words, on the eastern rim of Oceanographer and Lydonia Canyons you have these gravel lags.

I mean, those are similar environments in two different canyons. However, there are other parts of those two canyons that vary widely. The canyon floors, for example, are much different.

So when I say "--do not exhibit identical sedimentary environments--" I mean that you cannot say that because you know what is happening in this canyon you can automatically say that the next one is the same in all aspects.

Certain processes in these canyons are the same from canyon to canyon.

DR. BOTHNER: The intensity is different. Actually, what troubled me about the last sentence I heard was that the sediment sources are really not the same. I mean, the sediment sources are the same, but the sediment characteristics are not.

DR. VALENTINE: This is a broad statement. The canyon characteristics part comes later in the document. This is just an up front explanation to let people know that canyons originate on the shelf and extend a long way off shore.

We know that they are very variable. This workshop is mainly going to focus on the shallow part that is mostly enclosed by the shelf, because that is where the impact is going to be the greatest.

DR. BOTHNER: I am in complete agreement with that.

DR. VALENTINE: So this is just to set the stage. I did not want to get into too much detail about different canyons or more energetic than others.

DR. GRASSLE: Let me try to make a suggestion about these sentences.

These studies have shown that canyons may have several kinds of sedimentary environments.

DR. VALENTINE: They all have several kinds, yes. No canyon has a uniform, homogeneous sedimentary environment throughout.

DR. GRASSLE: They may differ from one another in sedimentary environment. See, when you look there it is a problem, too. It could be within a canyon or between canyons.

DR. VALENTINE: You could say, "These studies have shown that canyons do have--do exhibit different sedimentary environments--" not just "may exhibit."

DR. GRASSLE: Okay, but how about "--do differ from one another in sedimentary environment--"

DR. VALENTINE: Sure.

DR. GRASSLE: Then, sediment sources and processes and bottom current regimes may also differ.

DR. VALENTINE: Well, leave out the "may."

DR. GRASSLE: --also differ, excuse me.

DR. BOTHNER: That is a little redundant somehow. Saying that the sedimentary environments are different--

DR. GRASSLE: I just don't like "variable." Are you comfortable with "variable?" I will withdraw it if--

DR. BOTHNER: How about this? "These studies have shown that canyons exhibit widely different sedimentary environments. These are characterized by differences in sediment texture, intensity of bottom currents, and the predominant sediment processes from canyon to canyon."

DR. GRASSLE: Sounds good to me.

DR. VALENTINE: That is fine. I was just trying to make it more general, but it is all right with me.

DR. GRASSLE: It seems critical in our later discussion to get this idea that there is not an average canyon.

DR. VALENTINE: In the characteristics section, I did not want to be redundant.

DR. GRASSLE: I think we will repeat ourselves. Do you want to read it again for the record, Mike?

DR. BOTHNER: All right. I have got to compose just a tad.

"These studies have shown that canyons exhibit widely different sedimentary environments. The characteristics which are different from canyon to canyon include sediment texture, intensity of bottom currents, and the predominant sedimentary processes." By that I mean accumulation/erosion type things.

DR. VALENTINE: Why don't you say "erosional and depositional processes."

DR. BOTHNER: All right. So "the intensity of bottom currents and erosional depositional processes."

MR. VILD: How about just erosion and deposition, and get rid of that extra word?

DR. VALENTINE: We like the word "processes" in there.

DR. KRAEUTER: That is what the study was all about, right? It was a process study. That is what you funded.

DR. WILBER: May I ask a question? Are those two sentences needed at all?

DR. GRASSLE: Yes.

DR. WILBER: Is there a final version of the last sentence?

DR. VALENTINE: Would you read the final version again, Mike, of the last sentence?

DR. BOTHNER: "The characteristics which differ from canyon to canyon include sediment texture, the intensity of bottom currents, and erosional and depositional processes." This changes with every round.

DR. GRASSLE: Okay. I think that is okay now, right?

DR. WILBER: These studies have shown that canyons do exhibit widely different sedimentary environments. The characteristics which differ from canyon to canyon include sediment texture, intensity of bottom currents, and erosional and depositional processes."

DR. GRASSLE: Good. Are there other issues that need to be raised? I think we are done, are we not?

DR. GRASSLE: Nancy, I have to write a little bit. Oh, Mike, will you have a Teal section? You will finish that before you go.

DR. AURAND: We are done. Thank you.

(Whereupon, at 2:23 p.m., the Submarine Canyons Workshop was concluded.)