Section III. The Science of and Lessons from the Case Studies

In all three case studies examined in this workshop, legal controversy has focused on uncertain scientific data and theories pertaining to the interaction of commercial fisheries with endangered marine wildlife. To reveal the limitations and complexities of the data scientists are working with, three marine conservation scientists were invited to discuss the data and models that were referred to in the litigation of the three case studies. Following their presentations, two additional conservation biologists were asked to comment on the significance of the three case studies. They were asked to address implications for the broader themes of addressing scientific uncertainty within a policy context, communicating science to policymakers, the courts, and the public, and understanding how legal terms are applied to scientific research.

Case Study I: Steller Sea Lions and Bering Sea and Gulf of Alaska Groundfisheries

Douglas P. DeMaster and Lowell Fritz, Alaska Fisheries Science Center, Seattle, WA

The following is an edited version of the presentation given by Douglas DeMaster.

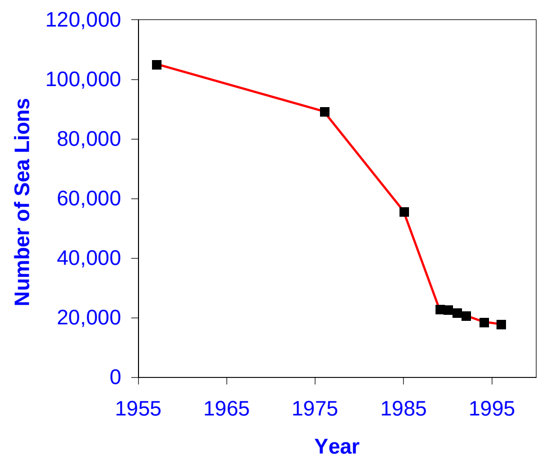
Introduction

The National Marine Fisheries Service (NMFS) is the federal agency responsible for the management and conservation of marine living resources in U.S. waters. In the U.S., the groundfish fishery in the Bering Sea and Gulf of Alaska constitutes the largest single fishery and is managed under the mandate of the Magnuson-Stevens Act. However, the NMFS is also responsible for promoting the recovery of endangered species, including the western population of the Steller sea lion, under the mandate of the Endangered Species Act (ESA) and for managing most populations of marine mammals under the Marine Mammal Protection Act (MMPA). The objective of this paper is to summarize the issues confronting the NMFS regarding the joint management of the groundfish fishery in Alaska and the recovery of Steller sea lions. Much of the information referred to in this report can be found in the 30 November Biological Opinion (NMFS 2000) regarding the potential impact of the groundfish fishery in Alaska on ESA listed species.

Background

Steller Sea Lion Population Dynamics

Fig. 1. Summary of sea lion trend data.



The western population of the Steller sea lion underwent a rapid decline of approximately 15% per year from the late 1970s through the late 1980s. While imprecise, the pre-decline population has been estimated to include at least 180,000 animals. In the 1990s the population continued to decline but at a significantly lower rate of approximately 5% per year. The estimated abundance of this population in 2000 was 34,600 animals (Ferrero et al. 2000). Because of this unprecedented decline, Steller sea lions in Alaska were listed as threatened in 1990 under the ESA and depleted under the MMPA. In 1997, the western population was listed as endangered.

There is substantial evidence that nutritional stress was one of the leading causes of the rapid decline in the 1980s. This determination was based on relatively large collections of animals from the 1970s and 1980s, as well as the results of long-term marking studies. The mechanisms by which nutritional stress led to the decline in abundance is thought to have been related to both reductions in juvenile survival and the reproductive rate of adult females. The cause of the nutritional stress is thought to be a product of large-scale environmental changes in the late 1970s (i.e., a regime shift) and competition for food with commercial fisheries. In addition, human-related removals (i.e., mortality incidental to commercial fishing, and direct mortality caused by commercial fishers and Alaska Native subsistence hunters) were an important factor in understanding the decline in the 1980s. Other factors, such as disease, adverse affects due to pollution, entanglement in marine debris, and predation, were not considered important to the dynamics of this population at this time.

Unfortunately, there are inadequate data to thoroughly evaluate and identify the leading cause or causes of the continuing decline in the 1990s. Loughlin and York (2001) reported that the current decline of approximately 5% per year is related to additional mortality of 1700 animals. They noted that only 25% to 45% of this mortality could be explained. That is, mortality associated with subsistence harvest, commercial fishing, incidental to research, and predation by sharks and killer whales, can account for only about 450 – 800 animals. The remaining mortality is unaccounted for and could be related to fishery interactions, additional predation pressure, or other unknown causes.

Steller Sea Lion Foraging Behavior and Diet

Recent reports by Sinclair and Zeppelin (2001) and Wynne (pers. comm.) indicate that Steller sea lions forage throughout the water column and consume prey from the epipelagic (e.g, herring), demersal (e.g., arrowtooth flounder), and semi-demersal (e.g., pollock, Atka mackerel) zones. The size and composition of the prey consumed by sea lions varies with their age and sex. Seasonal and regional differences in diet are significant. By region, pollock is a primary component of the diet throughout the year of sea lions in the eastern Bering Sea and Gulf of Alaska , while Atka mackerel and cephalopods are primary components of the diet year round of animals foraging along the Aleutian Islands. Pacific cod is an important component of the diet in all areas, but its importance is significantly greater in the winter than in the summer. However, smaller scale differences in diet have also been reported. For example, around Kodiak Island, sand lance is a very important prey item for sea lions.

Prey Species	*GOA*	*EBS/AI*
Pollock	73%	54%
Pacific cod	34%	21%
Atka mackerel	5%	78%
Arrowtooth flounder	22%	4%
Cephalopods	5%	16%
Herring	11%	8%
Salmon	43%	20%
Sandlance	14%	3%

The following table summarizes 9 years (1990-1998) of diet data from 31 rookeries (collection period: May – September) and 31 haulouts (collection period: December – April). Diet information was based on the frequency occurrence (FO) of identifiable hard parts in the scat of Steller sea lion (Sinclair and Zeppelin 2001). The use of FO data has been recognized as potentially biased, but given the very large number of samples and the time period and areal scope of the collections, the above authors consider these data representative of the diet of sea lions being studied.

Table 1. Summary of diet information for Steller sea lions (1990-1998). Data are presented as percent frequency of occurrence in scats. Only food items with at least a %FO of 10% are shown.

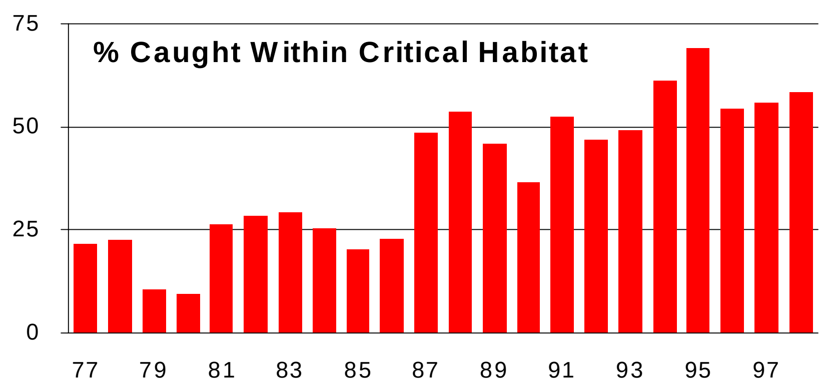
Groundfish Status in the Bering Sea and Gulf of Alaska

In the Bering Sea and Gulf of Alaska, the biomass of all species of groundfish generally have increased throughout the 1980s and remained relatively stable in the 1990s. Annual harvests during this same time period increased from the 1980s to the 1990s and have remained relatively constant over the last decade. Throughout this period the harvest rate has remained relatively constant (i.e., below 20% of exploitable biomass). This harvest strategy was designed to reduce the biomass of target species by 40-60% of equilibrium unfished levels, which is the standing stock of biomass thought to be optimally productive (i.e., net production and risk of overfishing are both at acceptable levels). In discussing the impact of commercial fisheries on the prey field of Steller sea lions, considerable confusion has resulted . That is, both of the following are true. Commercial fisheries for groundfish on average have reduced the exploitable biomass of groundfish by 40% to 50% relative to the unfished condition. And, groundfish biomass has increased between the 1980s and 1990s.

Several factors confound managers in the achievement of optimal harvest strategies. First, estimates of current stock size are imprecise and estimates of historical stock size and the stock size that would occur absent a fishery are even less precise. Therefore, significant changes in the best available estimate of current stock size relative to the theoretical stock size absent a fishery have occurred following a single year of additional survey data. In addition, the biomass of an individual species is not uniform in space; nor are removals by commercial fishers. Therefore, rates of removal in specifically fished locations are likely to be considerably greater than the target harvest rate for a species in a given management area.

For example, in the eastern Bering Sea over the last 20 years there has been a significant change in the manner in which the fishery is prosecuted, where a considerably larger fraction of the catch was taken from critical habitat in the 1990s relative to the 1980s (Fig. 2).

Figure. 2. Eastern Bering Sea pollock catch data (1977-1998).



The same phenomenon was reported to have occurred in the Gulf of Alaska during the same time period (NMFS 2000). NMFS has expressed concern regarding the potential for adverse effects of the commercial groundfish fishery on the recovery of the Steller sea lion caused by the “localized depletion” of prey important to the successful foraging by sea lions. For example, Fritz et al. (19xx) estimated that the Atka mackerel fishery in the vicinity of Sequam Pass was removing between 45% to 73% of the Atka mackerel in habitat critical to the foraging success of sea lions (Table 2). NMFS has not been able to insure that the effect of the current groundfish fisheries on the prey field of sea lions in Alaska will not appreciably reduce their survival or recoverability.

Table 2. Biomass estimates, catch data, and estimated harvest rate of Atka mackerel. Data from the Sequam Pass area.

Year	Leslie Biomass	Unbiased Biomass*	-95% CL	+95% CL	Catch	Harvest Rate (?)
1992	44,535	41,211	16,339	66,084	**29,909**	**73%**
1993	47,493	45,304	30,002	60,608	**31,704**	**70%**
1995	24,432	22,950	18,318	27,581	**12,126**	**53%**
1996		58,131	43,556	72,708	**25,993**	**45%**

* autocorrelation removed

Management of the Groundfish Fishery

The ESA requires that all Federal agencies "insure" that action taken by their agency does not jeopardize the continued existence or adversely modify critical habitat of species on the List of Endangered and Threatened Wildlife. Neither jeopardy nor adverse modification is defined in the ESA, but regulations promulgated by NMFS define the terms as:

> "Jeopardize the continued existence of means to engage in an action that reasonably would be expected, directly or indirectly, to reduce appreciably the likelihood of both the survival and recovery of a listed species in the wild by reducing the reproduction, numbers, or distribution of that species."
>
> "Destruction or adverse modification means a direct or indirect alteration that appreciably diminishes the value of critical habitat for both the survival and recovery of a listed species. Such alterations include, but are not limited to, alterations adversely modifying any of those physical or biological features that were the basis for determining the habitat to be critical."

A formal consultation with itself is required if NMFS cannot determine that a proposed action (e.g., authorization of groundfish fisheries based on the existing Fishery Management Plans) is not likely to jeopardize the continued existence or adversely modify the critical habitat of a listed species. In this case, NMFS specifically initiated a formal consultation with itself regarding the potential impact of the groundfish fishery in the Bering Sea and Gulf of Alaska on ESA listed species.

NMFS made a determination in the 30 November Biological Opinion that the groundfish fishery in Alaska would jeopardize the continued existence and would adversely modify the critical habitat of Steller sea lions. As required under the ESA, NMFS developed additional conservation measures that, if implemented, would remove jeopardy and adverse modification of critical habitat. The set of conservation measures described in the 30 November 2000 Biological Opinion are referred to as the Reasonable and Prudent Alternative (RPA).

To evaluate whether the fishery, as managed under the RPA, was adversely modifying habitat critical to Steller sea lions, NMFS developed a metric designed to index the adequacy of the prey field. To do this, NMFS first estimated the monthly biomass of cod, pollock, and Atka mackerel in critical habitat in the action area. NMFS then estimated the monthly consumption of sea lions in the action area and calculated a ratio of forage available to forage consumed (Appendix 3, NMFS 2000). The percent of forage consumed was found to vary between approximately 0.5% and 1.5%. In other words, were sea lions to only forage in critical habitat and to only consume pollock, cod, or Atka mackerel (both of which are conservative assumptions), the largest fraction of the prey based that would be consumed in a given month was 1.5%. Based on this determination, NMFS concluded that the large-scale prey field (i.e., at the critical habitat size scale) was adequate for the recovery of sea lions following implementation of the RPA. However, NMFS was also concerned with smaller-scale effects (e.g., localized depletion), but had few data regarding the appropriate size of the prey-field that needed protection to insure the critical habitat was not adversely modified. Therefore, to achieve an adequate level of protection, NMFS also imposed seasonal and spatial restrictions on fishing both inside and out of critical habitat.

To evaluate whether the fishery, as managed under the RPA, was jeopardizing the continued existence of sea lions in the action area, NMFS developed several criteria: 1) Was at least 50% of the non-pupping habitat closed to fishing for Pacific cod, pollock, or Atka mackerel, 2) Was at least 75% of pupping habitat closed to fishing for these three species of prey, 3) Was at least 50% of critical habitat closed to fishing for these three species of prey, and 4) Was the worst-case expectation of the effects of the fishery on the recovery of the western population of Steller sea lion such that the time to recovery would not be increased by more than 10%. An additional criteria involving the ability to test the efficacy of the RPA in promoting the recovery of sea lions in the action area was also considered.

Chapter 9 of the 30 November 2000 Biological Opinion describes the specifics of the RPA. Briefly, the RPA includes the following conservation measures: 1) established no transit zones within 3 nm of 37 rookeries and no fishing zones within 3 nm of all major haulouts, 2) a Global Control Rule would be applied to the catch-setting process that would not allow directed fishing for a species when the spawning biomass is estimated to be less than 20% of the projected unfished biomass, 3) 13 subareas within the action area would be established, where no directed fishing for pollock, Atka mackerel, and Pacific cod would be allowed in 8 of the 13 areas (Areas 2, 4, 6, 8, 9, 10, 11, and 13), 4) fishing for pollock, Pacific cod, and Atka mackerel would be prohibited from November 1 through January 20 inside critical habitat, 5) four seasons would be established for the open zones in critical habitat. This measure would evenly subdivide the combined winter allocation of 40% to the A and B seasons and the combined fall allocation of 60% to the C and D seasons, and 6) catch limits for pollock, Pacific cod, and Atka mackerel inside critical habitat will be established based on the proportion of biomass estimated to be in critical habitat open to fishing to the total biomass in the overall management zone.

Table 3 summarizes the percent of critical habitat open to fishing following the implementation of the RPA. Overall, 66% of critical habitat was protected, 74% of pupping habitat was protected, and 56% of non-pupping habitat was protected. In addition, NMFS assumed in evaluating the impact of the fishery on the recovery of Steller sea lions that absent a fishery the population would equilibrate at a minimum population size of 40,000 animals. It further assumed that an area closed to fishing would realize a benefit equal in magnitude to the current rate of decline of the non-pup population (i.e., 4%). NMFS, therefore, calculated that following the implementation of the RPA the downward population trend observed in the 1990s would be arrested. NMFS further determined that a worst-case impact of fishing in the sub-areas of critical habitat would result in an annual population decline of 0.77% per year over the next 8 years.

Table 3. Summary of % area of Critical Habitat open to fishing after implementing the RPA described in the 30 November 2000 Biological Opinion.

Block	*Area Closed*	*Area Open*	*Total CH Area*	*% CH closed*	*% CH open*
GOA	80,926	62,512	143,437	56	44
EBS	91,844	20,579	112,423	82	18
AI	62,570	37,530	100,100	63	37

Finally, NMFS in developing the RPA struggled with the conflicting objectives of removing jeopardy, but including a monitoring plan that could test the efficacy of the RPA. In a traditionally managed fishery, such an evaluation would likely incorporate an “adaptive management” experiment, where certain portions of the action area would be closed to fishing, while other areas would be open. However, in this case, NMFS determined that the ESA objectives of removing jeopardy and avoiding adverse modification of critical habitat had to be achieved first, and it was only in the remaining areas open to fishing could an adaptive management experiment be undertaken. It was with this in mind, that NMFS created the standards of closing 50% or more of critical habitat.

In designing the monitoring experiment, NMFS established three regional zones (Gulf of Alaska, eastern Bering Sea, and the Aleutian Islands) such that the trend in abundance of non-pup sea lions in the areas open to fishing were statistically similar to the trend in abundance in areas closed to fishing. NMFS further tested the hypothesis that a 1% or greater improvement in the non-pup trends in abundance would be detected after 4, 6, and 8 years of annual monitoring, and determined that 8 years of monitoring would be sufficient for evaluating the efficacy of the RPA in promoting the recovery of the western population of Steller sea lion.

Conclusion

The management of the groundfish fishery in Alaska in light of the ESA requirement to insure that a federally managed fishery does not jeopardize the continued existence or adversely modify the critical habitat of Steller sea lions is extremely difficult and contentious. It is made all the more difficult by the indirect nature of the interaction (i.e., competition). Only one other marine mammal – fishery interaction involving an ESA listed species is based on competitive interactions (i.e., Hawaiian monk seal – lobster fishery), and it is equally contentious. Management is further complicated by the significance of environmental forcing in contributing to the current decline of a species that by traditional expectations should be recovering.

There is no question that the western population of Steller sea lion is declining. And, there is no question that several of the target species important to the groundfish fishery in Alaska are key components of the diet of sea lions in Alaska. Also, there is no question that the commercial fisheries remove very large amounts of Steller sea lion prey from habitat known to be critical to the successful foraging of sea lions. However, this is about where the “certainty train” stops.

The currently available data are inadequate to test the nutritional stress hypothesis. Therefore, it is not possible to test for the relative importance of environmental forcing in causing sea lions to be nutritionally stressed relative to the importance of competition with commercial fishing. Interestingly, a number of independent data sets indicate that the western population of sea lions are not nutritionally stressed relative to their con-specifics to the east. Based on these studies, scientists have retreated from the conclusion that nutritional stress is the leading hypothesis for explaining the current decline, and now can only conclude that inadequate recruitment is the most likely contributing factor (DeMaster et al. 2001). Likewise, there are inadequate data to test the importance of predators in contributing to the current decline (often referred to as the “predator pit” hypothesis).

At this point, NMFS has tried to use the best available scientific information and commercial data to evaluate the nature of the interaction between the groundfish fishery and Steller sea lions in Alaska. Unfortunately, the available information sources are not entirely consistent, and the NMFS has had to evaluate which data sources have precedence over others. Without additional conservation measures, NMFS could not insure that jeopardy or adverse modification of critical habitat will be avoided were the groundfish fishery in Alaska prosecuted as it would have been under the existing FMP. Therefore, NMFS has developed an RPA and intends to implement it (or a comparable RPA) to mitigate an interaction that can not be proven to exist.

Finally, all of the stake holders involved with this issue support the necessity of determining whether a particular set of conservation measures are effective in mitigating whatever potential exists for jeopardizing the continued existence or adversely modifying critical habitat. Unfortunately, this has proven extremely difficult. And, it can not be done quickly. The likelihood of either the environment or fishery management regulations remaining constant over a period determined to be sufficiently long to test the efficacy of an RPA is very low. Nonetheless, NMFS has made a commitment to test the adequacy of conservation measures designed to mitigate potential adverse effects of the interaction. The outcome of this monitoring program will not be available for at least 5 years at a minimum. Claims otherwise are unnecessarily optimistic.

Q & A Session for Doug DeMaster

JIM ESTES: The power analysis that you did, I presume, assumes that in the areas that remain open that the level of the take remains roughly comparable to what it had been, there's going to be redistribution, so probably it will get worse in those areas.
So that would, it seems to me, make your design actually more powerful.

DOUG DEMASTER: It was conservative in that sense, since the contrast should have been bigger than we assumed here. Because we assumed that the existing trend in those blocks will continue.

JANIS SEARLES: When you did the pattern of open and closed areas, I know you used the coverage of habitat and of a certain proportion of adults, but how much work did you do with fishing effort?

DOUG DEMASTER: We looked at fishing effort because we wanted to make sure that there was adequate effort in the open areas to create a contrast with the closed areas, but we weren't looking for a certain fraction or percentage of it because we assumed that in the closed areas that it would get moved over to the open areas. Now, it turns out that wasn't a very good assumption because what the fishermen told us was, when you have those closed areas like that the processing plants in those closed areas wouldn't be getting adequate input to remain open. They'd close. Then the time it takes to take a load of fish from Point X to the next closest processing plant is such that the fish would spoil. So at least their economic analysis of our proposal was that out of a $700 million fishery, they'd be losing $500 million.

ELLIOTT NORSE: Two questions. One is, can you update us on the nutritional hypothesis, the lipids content in diet analysis and what that might have done? Two is, can you think of anything that might account for the anomalous pattern where you've got the least population decline or even barely detectable increase in a place where fishing intensity was highest?

DOUG DEMASTER: I'll start with the second one, dear to my heart. There's a couple explanations. Again, as a parallel with monk seals, there's a feeling that you could possibly have a black hole going on, where it's an area where animals are coming in from the outside so you're drawing some net recruitment from the outside into this really good area. So that's one explanation where you could have animal movement.

Juveniles move around a lot. The adults, once they start pupping, don't move around much. So if that's occurring, it's probably a juvenile phenomenon. Also, most of that fishing is taking place during the A season, the winter season. So perhaps there is some other time of year that is really benefiting the animals in that area, perhaps the summer. It's hard to know.

In terms of nutrition, a lot of work has been done on nutritional hypothesis for the different prey species, in particular a proposal by a number of scientists talk about the junk food or popcorn hypothesis, that pollock doesn't have enough fat, has enzymes that block iron absorption, a whole series of things. Initial experiments that were done in captivity suggest that if you take animals that are used to eating herring and then switch them today on great big frozen pollock, they'll lose weight. Now, for most animal trainers that's not a big surprise because most animals in captivity will just say no. Like your dog, if you have an Irish setter, they won't eat if you switch their diet. So that study suggested there was a decline related to pollock, but couldn't tease out other factors.

Similar studies are going on at the Sealife Center and another one at the Vancouver Aquarium where the original study was done, looking at a mixed diet and augmenting the diet with oil and trying to do a much finer scale. But the initial study suggested that on a pollock diet, Stellers will lose weight over a two-week period. The energetics and nutrition suggest that pollock isn't all that bad a forage relative to squid and some of the other groundfish, but isn't nearly as energetically dense as capelin, herring and some of the fatty fish. That being said, pollock during the roe season when they have eggs, a lot more calorically dense than outside the roe season. So it's not an easy question.

The other thing I think you should be aware of, as I pointed out, the eastern stock of the Steller sea lions, a separate stock, is increasing. In Alaska, it's increasing about 1.7% per year. Something like 70% of the scats contain pollock. So if pollock were really bad then you wouldn't expect that increase in southeast Alaska. So it's either a combination of diet diversity or a combination of getting pollock and access to other things that they might need. But that's kind of a complication. There's nothing inherently wrong with pollock.

The other thing to keep in mind I think is an important point. People are trying to separate nutrition from predation. There's lots of evidence that suggests the longer you're out trying to feed the more available you are to a predator. So separating predation and nutrition is probably ill advised.

JOSH EAGLE: How closely correlated in time are the oceanographic changes that you referred to in the decline of the sea lions? Are those occurring exactly at the same time?

DOUG DEMASTER: Dead on. Dead on. You couldn't have made up an experiment that did any better.

LEAH GERBER: How did you determine critical habitat, did you think about seasonal changes?

DOUG DEMASTER: I was a lot younger then and I wasn't involved in that.

LEAH GERBER: Are there efforts to compare seasonal patterns in --

DOUG DEMASTER: I know the Council is interested in re-defining it [critical habitat]. Janis, maybe you can help me out here.

JANIS SEARLES: When they designated critical habitat they went on the basis of the limited foraging data that they had, where sea lions were being entangled or where they were being caught a lot, where there were opportunistic sightings of sea lions and where there were dense aggregations, particularly in the aquatic foraging areas of spawning stocks.

There have been a series of proposals over time, particularly in the early '90s before critical habitat was designated to have closures that more closely mirrored sea lion foraging ecology during the summer and the winter. During the winter, sea lions forage larger. There have been proposals periodically to have 16-nautical mile closure zones during the winter and then 20-nautical miles in the summer.

DOUG DEMASTER: I can add a little bit, I happen to have a lot of this data with me. If you look at the platform of opportunity data, which is opportunistic sightings, Steller sea lions are seen every month all the way across the Bering Sea shelf break, way out. If you look at the satellite data, something like 90% of the sightings are inside ten miles. But we only have like 53 or 55 satellite tags and the state has got another 80 tags that they're looking at. So the satellite tagging data suggests these animals are staying pretty close to the beach, but we of course just like monk seals, took the easy way and we tagged animals during the summer when they were there and tagged adult females because we knew they were going to come back. They did not go very far offshore, but of course they were young pups. We then took the next step and tagged adults and pups in the winter. They don't go too far offshore but occasionally they make long forays, much longer than you'd expect. The animals we don't -- we can't get a hold of are the one and two and three year olds, the very animals we think are having the recruitment problem and we can't catch them to put satellite tags on them. But the satellite tagging data did support the 20-mile ban for critical habitat. Those foraging areas, I think that's where we got into incidental take and all those other things.

SCOTT ECKERT: How closely linked is the little bit of tracking data you have to the fishery bycatch data? In other words, do you see a hot spot in the fishery bycatch data, is that reflective of true abundance or is it biased by the effort that you have there?

DOUG DEMASTER: The pollock fleet is finding the spawning aggregations if we let them.

SCOTT ECKERT: Relative to your Steller sea lion population: is there a disconnect between where the fishing effort is and where the animal abundance is?

DOUG DEMASTER: No, they're going to roughly the same area.

BILL GILMARTIN: Sounds like you're saying that from the information you have, the nutritional data on pollock, you're saying that Steller sea lions shouldn't be eating pollock, that they've got some other things available to them that are better so that's what they should be eating. I mean, I can see some people arguing that at some level. That's similar with monk seals and lobsters, where monk seals eat say 40 species of things, what's the problem with taking lobsters, there's 39 other things out there. So is that used at all in that way by the council or by anyone in terms of -- saying there is other species there -- of course you're fishing for most every thing else.

DOUG DEMASTER: I think the council's analysis is that Steller sea lions are only consuming less than 1% of the total biomass of cod, pollock and Atka mackerel in critical habitat. So their feeling is there's lots of fish, the fisheries are only taking 10% and leaving 90%, so what's the problem. They're also arguing that Richard Merrick did his Ph.D. on Steller sea lions and found that as the diet gets more diverse, the populations tend to be better. That being said, the animals can only eat what's out there and they're programmed, if you will -- these animals are habitual and I think animals that get used to eating pollock off a certain rock at a certain time of year go back there every year. So I don't think they're very flexible in some of their habits. But we need to get more satellite tags on to look at that question in particular.

JOSH EAGLE: The capelin and herring that were the primary food source in the previous regime, what percentage of biomass were being removed of those species by commercial fishing during that time? Do you know?

DOUG DEMASTER: There isn’t and there hasn't been a capelin -- the forage fishing -- other than herring, they haven't had a forage fishery in that area. They fish groundfish and they fish salmon, but not capelin.

Case Study II: Sea Turtle Life Histories and the Hawaii Longline Fishery Litigation
Scott Eckert, Hubbs-Sea World Research Institute, San Diego, CA

The following is an edited version of the presentation given by Scott Eckert at the Best Available Science workshop.

I'm going to briefly describe the life history of sea turtles with a particular emphasis on two species; the loggerhead and leatherback because of their interaction with the Hawaii longline fishery.

There are seven species of sea turtles, five here in the Eastern Pacific; leatherback (*Dermochelys coriacea*), green turtle (*Chelonia mydas*), loggerhead (*Caretta caretta*), hawksbill (*Eretmochelys imbricata*), and olive ridley (*Lepidochelys olivacea*). The species that are primarily involved in the Hawaii longline fishery are olive ridley, loggerhead, green and leatherback. The green turtle can be divided into two regional populations, separated by geography, those that are found in the in the Eastern Pacific and Western Pacific. Some researchers have proposed separate taxonomic status for the eastern Pacific population, but it is not widely supported scientifically. The olive ridley, too, breaks out a little bit into regional populations, primarily those populations that nest in Costa Rica and those that nest in Mexico. Under the Endangered Species Act the Costa Rica nesting population is classified as threatened, while those from Mexico are classified as endangered. But again, for this presentation, I'm focusing primarily on leatherbacks and loggerheads because those are the species that drove the recent decision to close the Hawaii-based longline swordfish fishery.

First, some background on loggerheads. The scientific name of the loggerhead sea turtle is *Caretta caretta*. It's neritic as an adult. In other words, it's a coastal dweller. It feeds on bottom areas, primarily on mollusks. As a juvenile, it has a pelagic distribution, but as an adult it settles down to live in coastal areas and is relatively sedentary. Once an adult loggerhead settles in an area, it is likely to remain in that area for the rest of its life, except to make reproductive forays, or in areas of less optimal environmental conditions they may make seasonal migrations.

Leatherbacks, in contrast to loggerheads, are entirely pelagic. They spend their lives in the open ocean and as you'll see from some slides later, they travel great distances across open oceans. They have the widest geographic range of any reptile. They're seen from 71 degrees North to 47 degrees South latitude. They have an extremely well designed hydrodynamic form for long distance traveling, and there's a whole series of biological features that follow along with that, including the fact that they never stop swimming. They are top-of-the-food-web predators and they feed primarily on jellyfish. So they fit a very unique part of the marine ecosystem as far as what they prey on.

I will present a basic life history of sea turtles, basic because we know a lot about little parts of their lives, but not very much about their entire life history. This slide depicts the life history of a loggerhead but it is fairly typical of the other species as well. Females will find nest sites up on high, dry sandy beaches. They crawl up the beach, dig a nest with their rear flippers and deposit a clutch of about 120 eggs; leatherbacks will deposit about 80 eggs per clutch.

The general models used by animals when trying to produce optimal numbers of offspring break into two different types. The ‘R’ strategy is where an animal produces a whole lot of young who mature fast and have a low survival rate, and the ‘K’ strategy where an animal produces a few young but takes care of those young to protect them until their chances of survival are good. Animals that use a K selection strategy are usually long lived and late maturing. Whit Gibbons and others have proposed that turtles fall somewhere in between, he calls them “bet hedgers”. These are animals that have late maturity, long life span but high reproductive output, and low survival rates among their offspring. It’s a mixed strategy.

In the case of loggerheads, they average about three clutches of eggs per season with an average 120 eggs per clutch. Leatherbacks average about six clutches with an average of about eighty eggs per clutch.

Here in the Pacific we don't have a good handle on the average number of nests produced by female leatherbacks during the season. This is in part because eastern Pacific turtle leatherback nesting beaches are quite long and difficult to patrol intensively. Some investigators suggest that leatherbacks may lay fewer nests per season in the eastern Pacific than those in the better studies Atlantic/ Caribbean nesting colonies. The number I'm giving you is really for the Atlantic, and it averages six clutches per season. Turtles do not nest every year, but rather they nest every two or three years. So every second or third year a female will come back to her nesting beach and deposit another series of eggs. Here in the Pacific some data was presented a couple weeks ago at our annual sea turtle symposium suggesting that leatherbacks here in the Pacific may actually have a longer -- what we call inter-nesting period, maybe as long as five years. But that's a little bit open to debate right now. Most typically it's two or three years for most populations. Pretty much the same thing goes for loggerheads, about every two years.

Hatchlings emerge from the egg after about 60 or 70 days of incubation. Emergence is simultaneous. They come out all at once to try to overload any predators who are hanging around. They make their way down to the sea using photic cues, primarily they look at the light levels over the ocean and over the land, they use the lighter horizon as their target destination. During that crawl they imprint on their natal beach. So during that crawl they set an internal magnetic compass. That compass is used to help carry them offshore so they continue to move away from coastal areas. They will also use a number of other cues, including oncoming wave direction and maybe sound, but photic cues are the primary ones.

Once they get offshore, this period was long called the lost year of sea turtle biology. We didn't have a clue where they went. They would disappear. We really didn't see them again until they came back into the coastal areas to settle down and feed. This would be as much as 10 or15 years later. A lot of effort has gone into trying to understand this big gap in their life history. Early on, Archie Carr proposed that loggerheads hatched on the Atlantic coast of the US moved directly out into the central Atlantic Ocean after leaving the nesting beach. Following up on that, there was some very good work done in Europe looking at the distribution of young turtles. They discovered that young loggerheads were appearing off the European coast at a size class not seen elsewhere in the Atlantic Ocean, and theorized that this was because these turtles were hatching on the US Atlantic seaboard and circumnavigating the basin to end up off the coast of Europe. Then, after residing there for an unknown time, they were migrating back to the U.S.

We see a similar situation in the Pacific Ocean.   Loggerheads nest in the Japanese archipelago and. on the Australian coast. While we are unsure of the migratory cycle of the Australian loggerheads, hatchling loggerhead leave Japanese waters and traverse the Pacific Ocean. They fetch up on the West coast of North America where they stay until they reach about 50 cm in carapace length. Then they migrate back across to the Japanese archipelago where they settle in a coastal area and reside for the rest of their lives.   The entire circuit probably takes about 10 or 15 years. It's a very long scale developmental migration as these turtles come across for loggerheads.

We have an even a more significant challenge to understand where young leatherbacks go. They are hardly ever seen. I've been collecting juvenile loggerhead sightings now for about 15 years and I have less than 100, with a far majority of those sighting of turtles longer than 100 cm carapace length. They're very difficult to find at sea. We really don't know where they are. I recently assembled some of that data and we're beginning to come away with an idea of where they may are.

What you can see from this figure is that there is a strong relationship between water temperature and the size of the turtle we don't observe small leatherbacks in cold water. Until they exceed 100 cm in carapace length, they remain in waters warmer than 26 degrees centigrade. What this indicates to me is that leatherback hatchlings and youngsters up to 100 centimeters in carapace length stay in warm tropical waters above 26 degrees. That's quite significant to fisheries management, because if the young leatherbacks are less than 100 centimeters in carapace length they are likely staying out of the cooler swordfish fishing grounds. It is likely that swordfishing vessels are not catching young juveniles. They are only catching older juveniles and adult turtles.

Maturity time in sea turtles is generally well known for all species but the leatherback, though it must be noted that for turtles in general, it is highly plastic. Maturity time is heavily influenced by environmental conditions, and thus the same species in different areas may also have different ages at first reproduction. In some populations, loggerhead maturity may be as long as 35 years or as short as 22 years. So you can have quite a range in first age of reproduction. Nonetheless, it's generally somewhere in the range between 22 and 35 years. With regard to leatherbacks, there's ongoing debate in the scientific community. They seem to grow very quickly, however we have no wild growth data on them. The captive growth data we have seems to indicate a fast growth pattern. There are those who argue that this fast growth allows them to mature in less than ten years. I am not one of them. I argue the opposite. While they seem to grow fast, they have to grow proportionately larger than other species to reach maturity. I think maturity is closer to 15 or 20 years for leatherbacks.

In the Pacific, mature loggerheads and large juveniles forage in coastal waters of Japan and Australia. Juvenile loggerheads are distributed across the Pacific between about 32, 34, 35 N latitude. We see quite a large number of them across the Hawaiian Islands, and over here on the southwestern coast of the U.S. and the Mexican Baja Peninsula. One of the interesting results that has come from the high seas longline observer program, is that a frequency distribution of sizes of loggerheads captured in the fishery is bimodal). This seems to indicate that loggerheads are picked up as they move west to forage and again as they move west after their time in the eastern Pacific. Such a movement would indicate that there is a distinct corridor used by these loggerheads on their transpacific migrations. Certainly the loggerheads that I am tracking from San Diego, by satellite seem to return to Japan along a distinct route. These are animals that have been there for about 35 years and they're doing the exact same things as youngsters. So they were probably captured here and brought in as rehabs. We put them in the water even though it's been 35 years and they still know how to get right across and follow this very, very distinctive corridor. Seems to be an area, between 18 and 22 degree thermals -- water temperature, and we're hoping we get them all the way back to Japan, but they're certainly on their way.

Another possible explanation for the bi-modality in size may simply be due to residency of the turtles. Some of the work the NMFS Honolulu Lab has been doing with loggerheads here in the pelagic habitat seems to indicate they eat a lot of pelagic stuff in this area, such as salps and vellela. Possibly the turtles are including the trip across the ocean as developmental habitat, rather than a true migration route.

Leatherbacks, the story is a little more complicated -- vastly more complicated relative to looking at distribution. This figure illustrates more than 50 track lines of leatherbacks I've monitored, some for almost 2 years. Most of them are post-nesting females, and this is one of our great problems in sea turtle biology. We're beginning to learn a whole heck of a lot about reproductive females, but not a lot about the rest because it's very, very hard to get our hands on other age classes and sexes, and in the case of leatherbacks, it's the hardest. There's no question that leatherbacks are the biggest single challenge to understanding juveniles and non-reproductive animals that we've ever had.

But what I wanted you to come away with looking at this is that leatherbacks have an extraordinary geographic distribution. They are literally all over the map. Here in the Atlantic Ocean, we're getting some very distinctive movement corridors, and foraging areas. This is a foraging area up in the N. Central Atlantic, one here on the west coast of Africa and even one here off the coast of Virginia. One note of interest is that of these track lines, these are a series of individuals, three of them are males and two of them are females. We finally got the ability now to capture them at sea.

What we have seen is that male behavior is similar to the females. But one of the take-home messages from this is that this is just data that we gathered in the last year or so and it's the first time we have ever had any inkling about males. We literally never see males, we never get an opportunity to work with them. This is our first inkling that males may be traveling with the females on some of these. The other message here is that at least in the Atlantic basin there are annual north-south migrations.

Now in the Pacific, these are track lines, post-nesting females out of two beaches in Mexico. What I want you to note is the very, very distinctive pattern that is forming here. Leatherbacks seem to follow very defined migratory corridors. In this case post-nesting females migrate south along the 105 degree longitude line, across the equator and then to a large foraging area west of Chile and Peru. I think I'll mention these other two track lines as well right now. These are two turtles that Peter Dutton and I captured in Monterey Bay. Monterey Bay is an important foraging area for leatherbacks and with methods developed by a colleague in Nova Scotia, we can capture non-nesting leatherbacks to deploy transmitters and follow their movements as well. In this case the turtles are moving trans-pacific – right through area C of the Hawaii – longline fishing area. It appears that both are heading for the western Pacific nesting areas in Indonesia.

Population status of loggerheads and leatherbacks has been reviewed in the Pacific Recovery Plans and hasn't changed too significantly since. Loggerhead population status from the Japanese nesting colonies seem to be in slight to moderate decline, though we don't have very complete data on those colonies. Populations in Australia are in serious decline. Leatherback population status, is very serious. In the Eastern Pacific, the population is in free fall! This is from the nesting beach at Mexiquilla in Michoacan, Mexico at what we consider the key index beach because it's got the longest term complete coverage and because is support more than 10 % of all nesting each year. My Mexican colleague, Laura Sarti has maintained a project on this beach since 1983.

To monitor population sizes or trends, sea turtle biologists use number of turtles nesting annually. While this doesn't give us a total estimate of population size because it does not include juveniles, not breeding females or males, we believe it can serve as an index of abundance. I should note that for no species and no population of sea turtle can we report hard numbers for abundance. We simply don't know what the age or size specific composition, or even sex ratio is for a sea turtle population. Thus we are left to estimate trends based on nesting females. However as I mentioned earlier, turtles only nest every two or every three years and the proportion of females nesting each year doesn't necessarily remain constant. Thus we are uncomfortable using annual count directly to understand abundance or trend, most turtle biologists prefer to use 3 year running averages. That means to establish one data point for your population size it takes three years. So to look at this figure, you go from 1983 to 1986, that's effectively one data point. Then you add a second and 3 set of three-year averages and we can predict the trend. However it took 9 years of monitoring to detect that trend and in the case of the Eastern Pacific leatherback colonies, the population was almost gone in that 9 years!

Our ability to use nesting female counts or even nest counts as a management tool is hampered how long it takes to detect a trend. So when Laura and I were looking at this, I started working in Mexico in 1986, we had 1,000 females. We had a population that seemed to going up, it looked just dandy. We didn't begin to think there was really a problem -- and even here we weren't sure. We were just back to where we were in 1983. This didn't look so bad. It just looks like what you're going to see for a standard turtle fluctuation. So we weren't even worried really until we're way out here to 1990. So when we started talking about the management scale on these kinds of things we have to be sensitive to the fact that we as turtle biologists and conservationists working on the beach weren't even all that alarmed come 1990.

And it made it a very, very difficult sales case to the NMFS and to others to be concerned about this. (Doug can well attest to that. I was working for him back in the '90s and trying to get some attention and it wasn't happening.) It was difficult to convince NMFS of a trend with a figure that looked like this. So then we come down with another three years, and then you can see, okay, now definitely we're in trouble, and then we knew the problem was there. That's when we got the satellite tracking work going. This is the same thing, just modeling that population decline based on two different starting points. The decline was occurring very, very fast, at a rate of about 22% a year in the Eastern Pacific. This is the study we published in 1997. We got the satellite tracking work going, partly in response to that.

Now as these things often go, if you want to go out and raise money to do any research project it takes a couple years. So we saw the problem in 1990, 1991. We started pitching to try to figure out what was going on. It took us until about 1996 -- we did our first deployments in 1993. We didn't receive any federal funds until 1996 to put out our first batch of transmitters, represented here in these blue track lines. To put it mildly, we were quite startled. I had felt that the turtles were probably migrating up in the North Pacific, as you can see, they have some kind of a loop going this way. So that would be moving down here, opened a whole new can of worms, and got us looking at that particular area, what was down here that might be causing the problem.

If we look at what was going on off Chile at the time, Chile started an artisanal gillnet fishery here in the late 1980s. You can see the red line here, how that fishery grew between 1987 and 1995. It grew almost exponentially. Here we're about 4,000 days at sea, here's we're at 42,000 days at sea.

LARRY CROWDER: This fishery was targeting swordfish?

SCOTT ECKERT: This is targeting swordfish. It's an artisanal gillnet fleet. It's not necessarily high seas, they don't go that far offshore. The data we had from the early 1980’s came from a study by Jack Frazier and Jose Brite-Montero in who surveyed fisherman working from San Antonio, Chile then there's and old paper reporting on the number of leatherback carcasses that had been dumped in a landfill in Peru. We took the catch rate data from the early 1980s and applied it to the fishing effort and we came away with a very, very conservative estimate of 2,000 leatherbacks per year were being killed between the Chilean and the Peruvian fisheries. To be perfectly honest, it wouldn't surprise me in the least if the number was 5,000.

What we now can say from all this is that all of the post-nesting female turtles out of the Eastern Pacific population in Central America and Mexico head directly to South America. They forage off South America, as much as 1,000 nautical miles out, prime swordfish fishing grounds. The artisanal fishery that likely caused the eastern Pacific leatherback population to crash, is also now collapsing itself. It is my understanding that the demise of the artisanal fishery is due to increase high seas longline fishing off S. America, in which the Spanish fleet plays a large role. So the Spanish longline fleet has moved in there fairly intensively and they're now fishing fairly heavily offshore, which has tended to cause a population crash of swordfish inshore. So there's probably going to be reduction in leatherback sea turtle takes in the artisanal fleet, but undoubtedly an increased take by high sea longline swordfish boats.

Our satellite data has defined quite well the post-nesting movements of leatherbacks from Mexico and Central Americas, but what about the other stocks in the Pacific? Peter Dutton at NMFS has been analyzing DNA of Pacific leatherbacks and building a more complete picture of stock ranges for the species. One of the wonderful things about using DNA as a stock assessment tool in sea turtles is that each female returns to the beach at which she was hatched -- her natal beach. That causes each female and each turtle within that population to exhibit the same maternal signature,. So if you take a blood sample from a turtle, you can now tell what nesting colony it came from and from that we can then assess the stock, the stock it's identified with.

Peter's finding for leatherbacks is that of the 14 samples he has here from the Northern Pacific, which includes those captured in the Hawaii longline fleet and some captured here in the Northern California driftnet fleet, as well as some strandings from California, is that 13 of them are exhibiting a stock signature from the Western Pacific stocks and one from the Eastern population. That indicates to us that the animals that are probably being impacted by the Hawaiian longline fleet are probably all from Western stock. Peter's data is quite recent, and actually creates something of a problem. I think we have long assumed that leatherback stocks are mixing in the high-seas Pacific, but if Peter's results hold true and leatherback turtles are segregating, then we clearly need to be monitoring each stock separately. In other words we need to monitor Mexican and Costa Rican nesting beaches to evaluate Eastern stocks and Indonesian and SE Asian nesting beaches to evaluate Western Pacific stocks. We've done very well with the Eastern stocks, but the data on Western stocks is very weak.

We're trying to get better data. They've been collecting data there for a long time, but it's a classic case of the data is so poorly done that we can't quite figure out what is happening. So we're now working hard trying to get back in to Indonesia to help -- NMFS is doing this right now, try to get them to clean up their data collection so we can look at what the population trend has been. All this has happened just within the last two or three years. So we're learning a lot of stuff right now and it may have a tremendous amount of impact on some of the management decisions.

The message is that we are getting a handle on what the distribution of the females -- or the turtles are, all of them for leatherbacks. We're beginning to identify what stocks they came from and what areas each of those stocks are using. But that information is highly embryonic and our ability to base management decisions on it is still far off. One final note. These are the two turtles we are tracking out of the Monterey Bay. This is the Hawaii longline fishing area, Area C, A, B and D. You can see that these leatherbacks are moving right through Area C. We're beginning to figure out about leatherbacks now about their movements on an oceanic basis is that they are very corridor-oriented. So it's my expectation that when we get some more tags out here we'll actually be finding that this builds up as a very distinctive corridor.

From a management perspective, that information could be very useful. Not only do we know where the corridor lies, but it has a time scale associated with it. If we look at the rate of travel through here we find -- and this is happening globally on all the corridors right now -- is this very distinctive time period. The animals are there for a few months or a few weeks as they pass through these zones and then move out of those zones.

But that's not the whole story. If you were to look at where some of the takes have been of juvenile leatherbacks, these are these animals that are smaller -- bigger than 100 centimeters, but smaller than 140 centimeters in carapace size -- they're getting a lot of that up in here. So we've probably got two things going on. We've probably got foraging by juveniles up in here and then we've got a migratory corridor by adults and moving as they move through the waters. So that's where we are right now on leatherbacks and loggerheads in the Pacific.

Q & A Session for Scott Eckert

ELLIOTT NORSE: Why are the leatherbacks attracted to the same places that swordfish are attracted to?

SCOTT ECKERT: Very good question. The distribution of leatherbacks often seems linked to water temperature of about 14 to 16 degrees. They love areas that have convergent zones, sheer zones, those kinds of things where you've got good mixing going on. That's likely due to jellyfish concentrated in those areas. The follow-on question to that then is, why in the world do they hang out in the north of the Hawaiian Islands. Most are those are juveniles and I don't have any idea at all. We've got a ways to go before we understand that.

But I think -- well, my grad student at the university was looking at leatherback activities in Nova Scotia right now and is convinced -- and I'm in agreement with him -- that if we know where the jellyfish are we know where the leatherbacks are. One of the things that I'm finding with leatherbacks sort of on a general basis is that they are patch feeders on a global scale. I came out of the University of Georgia and there are a whole lot of people there who work on patch feeding and the interaction of the animals that feed in patches and fish in foraging patches. Only they're all doing it in a petri dish, looking at the little bitty things, feeding them little bits of other little things. When we start looking at the ocean basin on a whole, we see an animal that moves from feeding patch to feeding patch across great, great distances.

In fact, things we're beginning to see now on some of these animals we're catching up on the foraging grounds, is quite interesting because they are so much more robust than what we see on the nesting grounds. It's extraordinary. Literally, these giant rolls and rolls and rolls of fat on these creatures and we don't see that on the nesting females. I think what's probably happening is this is an animal that is used to boom or bust. They feed up real, real heavily then seasonally they move away and they go to the next feeding area and that feeding area may be three or four thousand kilometers away. It's nothing for a leatherback to decide to spend two months crossing an entire ocean before getting another bite to eat, and I think that's the kind of thing that we're seeing here, which drives their movement.

That's why I'm pushing so hard to get a better understanding of the oceanic environment out there and the way the turtles are utilizing it. I think that area off the African coast where there is good coastal upwelling is an important feeding area for them and I see this grouping up going on. I think this area off South America is also an important feeding area. It may very well be that some of those areas north of Hawaiian Islands where they've got a lot of activity going on there with currents represents the same sort of thing.

BILL RODGERS: What's your theory on the corridor behavior though, because if they're responsive to, let's say, jellyfish, why isn't it much more erratic? You have an evolutionary theory for why they seem to travel in those --

SCOTT ECKERT: Those particular corridors?

BILL RODGERS: Yes.

SCOTT ECKERT: I think it's hard-wired in. I don't think the animal's behavior is that plastic. They don't have intellectual capacity to adjust. One of the reasons I believe that is that that first year when we really got the large quantity of leatherbacks being tracked out of Mexico going down to South America, that was an El Niño year. They went right down and hung out offshore and they almost looked to me like they were milling about. I could kind of see this movement of the track lines. My first response was, well, it's an El Nino year, they're confused. They seem to go right into a foraging area, in tight to the coast and then leave and go to the next foraging area. These warmer temperatures have confused them.

But subsequent to that, every track line I've done since then, all the animals are doing the exact same thing. In fact, I've got one right now I'm tracking, we're at about 16 months on its track line, and it's staying right in that area and it's actually moving a little bit south from there. What that indicates to me is that despite bad environmental conditions for feeding, the animals don't have the plasticity they need to move away and go set themselves up in another area.

To me, that was surprising. I thought pelagic creatures, we're all used to seeing sea lion populations crash, all these different coastal animal populations come crashing down during an El Niño event and here we've got a pelagic animal. For them, it shouldn't be a problem. They should just be able to move and go someplace else where the food is still available and what we're seeing is they're not doing that. They actually seem fixed on this same thing.

Also, these corridors seem to be very distinct. They don't wander much from them. Each female year after year or different females will follow those same pathways. They're not feeding grounds. They're pathways that get to the feeding grounds. Steve Morreale has suggested that it's because they use magnetic projection to determine their location, they take the shortest path between two points and that gets them there. I think it's more complicated than that. They seem to do some very fine-scale navigational things out there to indicate they know in real-time where they are and where they want to be and so that's how they like to get there.

LES WATLING: It's possible that by crossing the Pacific in a corridor like that, if the animals are keying on jellyfish patches, you'd expect jellyfish patches to be a convergence zone, and they cross at least four convergence zones in that corridor by taking a diagonal path. So they cross four different convergent zones, at least, on each pass. So it would make a fairly efficient strategy if you're patch feeding and feeding on patches that would have moderately predictable location on a decadal scale.

SCOTT ECKERT: That's something really interesting. I haven't looked at that yet. I would like to. We're just beginning to build this data up now to be able to define these things. Because we're also getting foraging depth data on them as well, at what depth they're at, we can start looking at those kinds of things.

I know we've seen some interesting things. When the turtles reach areas where the jellyfish presence is high, their behavior changes. We see them when they're coming out of the Caribbean, they go out past Barbados and then they start hitting real deep dives. Paul Cornelius, the jellyfish scientist from Great Britain tells, me there's this species of jellyfish that lives down at two or three hundred meters and there's a high density right there. It's clear that the animals were working that.

Once we increase our sample size a little bit, we'll be able to start identifying those kind of things.

TIM GERRODETTE: Scott, you talked a lot about sources of data that you have that relate to basically habitat issues, where the turtles are feeding and their heart rate. As we heard in the presentation of the legal side this morning, though, one of the key issues facing NMFS is what is an acceptable level, if there is such a thing for such an endangered species, that could be permitted to be taken if the fishery is going to continue.

So we need other kinds of data for that, something about reproductive rates, population size and things like that. Could you comment on that?

SCOTT ECKERT: About ten years ago we talked about doing the PBR [potential biological removal level] on leatherbacks and I got my arm twisted with Tim to try to take a crack at some of this stuff because it is a critical management issue with these things, as well as the stock assessments. Now we've got stock data that indicates that the population is from the Eastern Pacific. That's a huge, huge, problem for us. We do not know anything about survival rates, wild survival rates in leatherbacks, right on up to juveniles, right into the adults. We don't know what the maturity times are. Some of these very, very basic pieces of information that the modelers need to be able to develop a population model we can then use to apply PBRs or some of these other things. That data just simply isn't there. Again, the other issue is it's always based on nesting females. It's a huge, huge problem. Why we don't have that information, we can beat that one to death if we want to. But it's basically that the effort hasn't been put into getting that type of information.

LARRY CROWDER: If I could add a comment, one of the problems is what we've measured about sea turtle vital rates were the things that are easy to measure, like adult survival, where you can get turtles that retain tags, which leatherbacks don't do. You can get a good adult survival estimate, but for leatherbacks we don't even have that because of their tag retention problem. For counting how many eggs you have per nest, that's real easy to count. They come to you, they put them on the beach, you count the eggs.

But in terms of survival rates for a population with this kind of life history, that's the stuff that probably matters the least. What matters is survival through that whole long life history when they're at sea and what their rates of survival are at various stages there and the duration of the life history. What's that age of first reproduction number. We can bound the problem in a sense that there are only certain life histories that can work. So we can rule out some possibilities, as it's impossible to have survival rates this low.

But in terms of having vital rates information for any of these turtles to build a model, what we've done so far is loggerheads, those vital rates, and Kemp’s ridleys because in that case, the whole population is censused. We know of all the individuals that nest are counted and all the hatchlings that leave the beach are counted. So if you know something about the age of first reproduction, you can do a kind of input-output model and get a fairly good idea of what's going on. With leatherbacks, we just don't have the numbers.

SCOTT ECKERT: To a large extent we can get those numbers, but it takes an initial anticipation that you're going to need them 10 to 15 years in advance of trying to gather that information.

LARRY CROWDER: Well, the reason we have a loggerhead model is that Jim Richardson started walking on the beach when he was 14 in Georgia and by the time he was a graduate Ph.D. student he had 20 years of data. Most of us don't live that long.

SCOTT ECKERT: And to compound it, these are often stock-based numbers. So you have to do it for the stock you're looking at. Because up in St. Croix we've got quite a bit of good data on leatherbacks. It's a really well covered population. We've got a huge amount of information on those animals. We're finally beginning to get up above 75% of pre-migrations on those. But that's St. Croix. That's the Atlantic Basin. Those animals are larger. They're different. We don't have that same kind of information in the Pacific. But we were talking about earlier, how do you get the management agencies to put the resources -- or somebody to put the resources into it. We have to say, we need that data but it has to be way, way in advance of the problem.

JIM ESTES: You have quite a lot of satellite data now for these animals at sea. Wouldn't it be possible to use disappearance rates to make some inferences about mortality from that? I mean you have some expectation about how long these satellite tracking devices are going to last and so forth, it seems to me there may be some useful inferences could be made on the basis of that. If they die at sea, it's going to disappear.

SCOTT ECKERT: It's really hard. When a transmitter quits, a transmitter quits. Often you don't have the foggiest idea of why, whether it is failure of battery. The only failure, I can usually determine is battery failure, because I get very diagnostic behavior of the transmitter just before it fails. Other types of failures, electronic or otherwise are impossible to deduce, as is attachment failure. To try to predict that cessation of data is due to mortality of the turtle or transmitter failure is not possible, yet.

The marine mammal folks have it a little easier because they often can get the transmitters back. So when they get them back, even though the batteries may be expired and they're beat up, they can start making some predictions about why they stopped.

DENISE ANTOLINI: Can you respond to the argument that Paul Achitoff discussed earlier this morning, which is that the impact of the Hawaii fishery is small or negligible compared to the global threats. What's the scientists' response to that? Also related to that, how do you see the international efforts to awareness of the threats and efforts to protect populations relative to what the United States and NMFS is doing? Can you give us some perspective on those two, if you can?

SCOTT ECKERT: There's no question that in my mind that the take of leatherbacks by Hawaii longline vessels is a tiny fraction of the overall Pacific take. I think that's supported. Whether the take level by the Hawaii longline fleet is enough for a jeopardy opinion, I don't want to speak for NMFS on this, but is that enough to threaten the population? Yes, I think the take levels that have been there are enough to threaten the population, and that's all the ESA requires.

But its my understanding that the ESA says that all other factors aside, is that take level enough to jeopardize recovery of the population. Considering that we have a population that has dropped 22% per year and is current take mortality levels were 25 or more leatherbacks take levels were too high. This goes way back to 1993 or 1997, one of these workshops on longlining that we had Larry Crowder sit in. He said, guys, you don't need a statistician to tell you these take levels are too high, irrespective of all the information we don't have, the levels of take were threatening the population.

Relative to the issue of domestic management issues versus international management and recovery issues, there's no question that the U.S. is leading the way. It's been slow to start, and I think in many ways the Fishery Service has not done a good job, but I also think they have been slow to publicize the good things they are also doing.. They have a very good leatherback biologist in La Jolla, Peter Dutton, and his genetics work is superb. They also have a series of negotiations going on internationally with the Chileans and the Peruvians and all these other guys, as well as with the Mexicans.

In fact, the last three or four years their directed efforts have been to bump the hatchling production up on nesting beaches. They are trying to do the same thing with Indonesia, but we're having troubles with revolutions and people shooting each other over there. So it's been hard to get in there. But there has been quite a bit of effort going on. The one place that I think we really need work is we really need to address a mechanism to reduce bycatch, whether that be time/area closures or gear technology in longline fisheries. That area where the turtles in the Eastern Pacific are feeding off South America right now is prime swordfish fishing area for the Spanish and a number of other fleets.

The same thing is happening in the Atlantic. It's happening all over the North Atlantic right now. We've got a high take level there. We've got to get a handle on this issue and that's going to take a really dedicated effort and it's going to take some time. It's going to take a few years.

If we started today -- it's actually sort of in the process now, but if this was time line one, it's going to be two or three years of a lot of dedicated effort to find a way to clean up longlining. Can we clean it up? Yes, I think longlining as a fishery technique can be adjusted to reduce leatherback and probably loggerhead bycatch down to a manageable level.

DENISE ANTOLINI: Particularly with respect to Chile, is there any governmental response to the kinds of crises that you're talking about?

SCOTT ECKERT: Apparently the Chileans -- and I haven't been a part of those discussions, but apparently the Chileans have been very cooperative. Peruvians have the door closed. They're not interested. Chileans have been very cooperative and they've put a couple of their biologists to work monitoring and putting an observer program into place and looking at what the take rates are out there. The Spanish are being approached now through the EU, but they have been uniformly pretty reticent to deal with this particular concern.

The lawsuit here in Hawaii and the ongoing biological opinions from the North Atlantic are nice tools. It's got a lot of people all of a sudden coming to the table. Certainly the Canadians have now become quite interested. My student keeps getting calls from Canadian fishery biologists saying, ‘do we have any of those leatherneck turtles in our waters?’

So all of a sudden we're getting a lot of international interest, but it's going to take a lot of effort before we can fix the problem. Here in the Pacific there are some in the community who believe it's too late in the Pacific for the leatherback anyway. But in the last couple of years the numbers have been creeping up a little bit. I think we're encouraged. In fact, Jim Spotila made a presentation at the last sea turtle symposium saying that their previous estimates -- while qualitatively accurate -- they said the extinction in ten years may be a little extreme and now they're seeing their data as -- stretching their data out a little bit. So there is some of that going on, too.

Case Study III. Hawaiian Monk Seals and the Northwest Hawaiian Islands Lobster and Bottomfish Fisheries

Bill Gilmartin, Hawaii Wildlife Fund; chair, Hawaiian Monk Seal Recovery Team

Bill Gilmartin presented background information on Hawaiian monk seals, their critical habitat the Northwest Hawaiian Islands lobster fishery.

The first counts of monk seals were done in the 1950s. Between those first counts, there were annual visits to the Northwest Hawaiian Islands. Gilmartin characterized them as very short one-day or two-day trips, where the animals were counted. There was very little in the way of a focused research effort during that 20-plus-year period. By the mid-1970s it was known from the beach counts that the numbers of animals counted had dropped by about 50%. As a result of that change NMFS listed the monk seal, with a lot of encouragement from the Marine Mammal Commission, in1976. Then in 1980 the National Marine Fisheries Service initiated a recovery program for the monk seal, which continues through today.

According to Gilmartin, the general focus of the recovery program is the National Marine Fisheries Service task force devoted to studying the monk seal and carrying out some recovery activities. There is also the monk seal recovery team, the NMFS-appointed advisory group to advise the program, and that group has met for the most part annually since 1980. The general focus of this recovery program and of the recovery team relative to monk seals has been in four major areas: population monitoring, efforts to enhance female survival, identification of habitat requirements and mitigation of human impacts.

He remarked that “population and monitoring, while it's been criticized often by the Marine Mammal Commission, is not just counting animals. It's putting people on these islands where the monk seals breed for usually a few months each year, looking at survival of animals, looking at condition of animals, injuries, entanglements, evidence of fishery interactions, those sorts of things, trying to for the most part identify problems. Sometimes all you are going to see is the change in survival. But sometimes you also see other things on the animals or just the general condition of the animal, if it's thin or something that serves to also indicate to you something may be going on. Monitoring also is important not just to identify problems, but also if you do experiment with some fix out there, you need some way to evaluate whether or not you fixed it. So you have to continue to look at those same things that indicated there was a problem and see if there was a change.”

Gilmartin described several female survival enhancement projects that had been conducted over the past 20 years. At Kure atoll, where there was a Coast Guard station and human disturbance at the beaches, a project was initiated where staff protected young females for a few months on the beaches. The recovery team saw a dramatic change in survival of animals, from 10 to 20% of the pups surviving to age one, to 80 to 90% even

100% survival of pups from birth to age one. The Team initiated a program that ran for ten years where they fed underweight female pups in captivity, kept them for a year and then returned them to the wild, releasing them at sites where population was depleted. Gilmartin reported that many those animals survived and became reproductive contributors to those populations.

He suggested that there has been and continues to be a problem with aggressive adult males killing females. In five different operations over a 15-year period, approximately 40 adult males have been removed from the Northwest Hawaiian Islands and relocated to other places. In the past few years there have also been shark control projects to increase pup survival at French Frigate Shoals.

In terms of identifying habitat requirements, Gillmartin described the Recovery Team's process: "Initially we looked at where pupping was occurring, hoping that we could add some protection to pupping beaches. We started to identify foraging areas, initially just by depth contour, having dive recorders on the animals and then going to where we could get position as well to indicate foraging and to identify prey species."

To mitigate human impacts, the Team identified prime pupping beaches and tried to add protection to those sites to keep people away from them. With respect to human caused fisheries impacts, Gilmartin said, "we know something is going on where there are interactions -- all I can say is [the recovery team] recommends some action be taken. That is something that neither the NMFS monk seal recovery program nor the monk seal recovery team can do anything about, other than recommend that the National Marine Fisheries Service or the Navy or the Coast Guard take some action to change those impacts."

Supportive research for monk seals has been funded at the level of about $250,000 to $750,000 a year, averaging about $500,000 per year during the 1980s and early 1990s. Much of the work that was done was done for the most part with at least half of the workforce being volunteers. Most of the real recovery actions that were taken, including the headstart work with the pups, rehabilitation work, and removing males were done with volunteer effort. Gilmartin pointed out that while this volunteer effort was essential, the team had limited resources, and was unable to execute some of its planned activities.

He reviewed the chronology of events leading up to the establishment of critical habitat for the endangered monk seals focusing in particular on how the available information developed versus what management actions were taken.

The monk seal was listed as 'endangered' in 1976. In 1980 NMFS issued the Draft EIS on designated critical habitat. The agency's preferred option was to set the boundary at 10 fathoms, while other proposed alternatives were 20 fathoms and three miles around the breeding atolls and islands. That same year (1980), NMFS appointed the recovery team, and it had its first meeting. Also that year the team conducted a study at Lisianski Island, which, according to Gilmartin was the first time any research was done on foraging patterns of monk seals. By attaching instruments to 9 or 10 monk seals, the team found that the animals were indeed using the area within 20 fathoms and also going beyond the 20-fathom isobath out to over 60 fathoms. Based on that information, the recovery team wrote a letter to the NMFS regional director recommending that the critical habitat designation be at 20 fathoms rather than the 10 fathoms preferred alternative, and that it include Maro Reef, where there had been sightings of monk seals that had migrated from Laysan Island to feed.

In 1983 the recovery plan was signed off by the regional director, and the recovery plan included the letter recommending that the 20-fathom isobath in the designation of critical habitat. In 1984, NMFS published a supplemental EIS with the preference for the ten-fathom isobath not including Maro Reef. In 1984, the recover team conducted another study which supported the 20-fathom isobath, and also showed animals diving deeper than had been seen in the earlier study. The recovery team met again in 1984 and wrote a letter to the regional director recommending a 20-fathom isobath and including Maro Reef. That was the last meeting of the recovery team for five years. The recovery team was disbanded after that meeting. Gilmartin remarked, “Although it was reformed a couple years later, it included 17 or 18 people and NMFS never seemed to have the travel money to get us all together so we never met.” In 1989, another recovery team was appointed. In the interim, the agency had not designated sufficient critical habitat.

In 1986, the Sierra Club Legal Defense Fund (SCLDF) sued NMFS on behalf of Greenpeace Hawaii to designate critical habitat per the recovery team and the Commission's recommendation. Gilmartin remarked, “So, of course, NMFS paying attention to all of this went ahead and designated critical habitat, but at 10 fathoms. Essentially, SCLDF just took them right back into court and pushed again for 20 fathoms, and then in 1988, NMFS reconsidered. It's interesting that no new data came in during this interim couple of years between the 10 fathom and 20 fathom isobath designation, but NMFS found that -- I don't recall what their reasoning was here-- but when they went out for public comments on this, they basically used the same information that had been developed earlier when they designated 10, now suggesting that it needs to be changed to 20.”

According to Gilmartin, there was a significant amount of diving data and movement research done during the 1990s, all of which shows that monk seals travel beyond 20 fathoms, with reports of one animal diving at 250 fathoms.

In looking at the interaction between the lobster fishery and the monk seals, Gilmartin said, “we knew monk seals ate lobster back in the 1960s and 1970s. The question has always been, how important is that in the diet.”

Much of the early monk seal diet data was summarized in the 1980 DEIS and showed that the animals did take lobster. Craig McDonald working in the Northwestern Hawaiian Islands doing lobster research observed a monk seal eating lobster, breaking the animals in half and then consuming only the tail. Gilmartin pointed out that the recovery team used scat analysis to try to determine the importance of lobster in the monk seal diet but did not find any evidence of lobster parts, but in seal spew on beaches, there were lobster parts. Gilmartin did a feeding trial with a captive adult monk seal, and fed it whole lobster. The seal broke the lobsters in half. He kept the animal for a few days, collected all the scat material, went through it and found no evidence of lobster. At that point, it became clear that scat was not a very good indicator of whether or not monk seals were eating lobster.

In the early 1990s, a Ph.D. student sorted through 940 scats from all the Northwest Hawaiian islands spanning a period of several years and found a very low percentage of what she could identify as crustacean parts. Since the recovery team had suspected that that this lack of visible crustacean parts was not necessarily representative of lobster consumption, her study reinforced the fact that there was still a big hole in knowledge of monk seal foraging habits, specifically, how much lobster they eat.

Also during the early 1990s the new technology of fatty acid analysis was developing and the monk seal program contracted with Dr. Sara Iverson to do some work with monk seal fat. At the 1998 monk seal recovery team meeting, Dr. Iveson reported that she found evidence suggesting a much higher percentage of lobster in the monk seal diet than they had been able to identify with scat material.

In the early 1990s, NMFS amended the lobster fishery management plan to force some lobster fishing to occur outside of the usual fishing commercial grounds of Necker, Maro and Gardner Pinnacles, into all of the other islands, French Frigate Shoals and points northwest. According to Gilmartin, these areas had not been fished for ten years or more. "This relocation of the fishery to much smaller areas, much smaller banks around some of the smaller atolls and banks in the northwest, where we had monk seal populations that were recovering, increasing in numbers from being fairly depleted during the 1970s, we thought this really could effect those growing populations with local depletion of lobster stocks. The other problem was that they had allowed this fishery to go and work these areas with knowing absolutely nothing about the abundance of lobster at these sites."

The recovery team made recommendations to NMFS that they should do some experimental fishing to find out about abundance of lobsters at these islands before allowing the commercial fishing effort to continue. NMFS did not respond. At their next meeting, they recommended that the lobster fishery be closed for three years to allow recovery of the stock and to allow more information to be developed on what fraction of the lobster resources monk seals may need. Gilmartin noted, that having this letter from the recovery team helped Earthjustice Legal Defense Fund in its lawsuit against the agency. He also noted that two days before the hearing, NMFS decided to close the lobster fishery for the sake of the lobster stock. "NMFS still has yet to acknowledge there is any potential interaction between monk seals and the lobster fishery."

Gilmartin described the situation with the lobster fishery: "One thing I would like to mention too, just that there's been an interesting change in the lobster resources.

Initially, the first fishery management plan was for a spiny lobster fishery. Essentially now the spiny lobster is commercially extinct. They've so fished that animal in the main commercial fishing grounds at Maro Reef they don't catch any more spiny lobster, they catch slipper lobster. The slipper lobsters have essentially moved into what was spiny lobster habitat. We don't know -- so historically we don't know, at least for the populations that may be feeding in those areas near the commercial fishery, how important that spiny lobster may have been relative to the slipper. I think we'll be able to find that out as this fatty acid technology gets used more on monk seals. We can look at some of the northwestern islands where the fishing has not occurred at the level it has near the more commercial grounds and see what the relative importance is of those two species that are being fished."

When the longline fishery expanded in 1990 from fewer than ten boats to over 50 boats, the monk seal recovery team started seeing hooked monk seals in the Northwest Hawaiian Islands at some of the breeding grounds. They saw a total of seven or eight animals that had been hooked, and obtained some video footage of the hooked seals. The video generated a lot of media attention, such that the West Pacific Regional Fishery Management Council asked for a temporary restriction on the longline fishery to outside of 50 miles around all the breeding islands. According to Gilmartin, that was later converted to a permanent restriction on that fishery because of the monk seal interactions on the breeding islands with the fishery. "Since that fishery was moved beyond 50 miles, to the best of my knowledge, there has not been a hooked animal seen on any of the breeding islands. So it was a really good move."

In January of 2001, President Clinton designated the Northwestern Hawaiian Islands as a coral reef ecosystem reserve, which, at the time of the workshop, was intended to eventual gain national marine sanctuary status for waters within the 50-nautical mile radius around the Northwestern Hawaiian Islands. The planned reserve includes some fishery restrictions, with the exception of the bottomfish fishery which was grandfathered into the reserve. The Executive Order grandfathers in the lobster fishing at year 2000 levels. Gilmartin pointed out that because of the lawsuit, there was no lobster fishing in the 2000. "It's unclear what will happen with that fishery, if it will remain closed. Certainly if it comes back, it's going to get challenged for sure."

Q & A Session for Bill Gilmartin

SCOTT ECKERT: I am impressed with how active the recovery team stayed after the recovery plan was written and turned in. How was that facilitated?

BILL GILMARTIN: Well actually, I'm surprised that after the critical habitat issue, that the agency even got us back together at all. But I pushed it. I was managing the program. I valued the recovery team input a lot and when it was disbanded I basically still used the program. I still phoned Doug; I still phoned other people on the team and had questions from time to time. So I asked that it be reformed. I was still surprised that it was. Since we were reformed in 1989 we have met annually since them. So I think it's been a real advantage to the program to have that expertise available.

BILL RODGERS: Could you just talk briefly about back in 1980, given the studies, as I read the overheads, suggested that 40% of the foraging activity occurred beyond 20 fathoms, why it was that the recovery team recommended 20 fathoms rather than a greater distance?

BILL GILMARTIN: Well, we only had the three options that were proposed to us. We discussed it but we didn't think we could offer a fourth alternative and have that get anywhere. So we decided to go with what we could see as the maximum, which is the 20 fathoms.

PAT TUMMONS: Just a question. One of the things that the West Pacific Council has been proposing is expanding the precious corals fishery, and one of the most fascinating things to have come from the tagging of the monk seals was actually the discovery of new precious coral beds. Is there any chance that the mining of precious corals might impact further the prey that's available to the monk seals?

BILL GILMARTIN: For those of you that didn't hear, when cameras were put on monk seals a few years ago, in 1996 or 1997, the animal that went to the 250 fathoms depth with a camera on his back showed that he was in a gold coral bed, which is a very slow growing, very precious corals, and no one knew that coral bed was out there anywhere.

So that brought up this issue of what's going to happen if you go in and try to harvest in that coral bed, does that disturb the habitat enough that it may basically eliminate it as a foraging site for monk seals.

Well, nothing has happened on that yet, and I think as best as I can tell, this coral reef reserve, that is within the 50 miles, that this coral reef reserve that has been established which is going to move into the sanctuary status will prohibit coral removal. So I think it's going to be dealt with that way. So I don't think it's going to be an issue.

CATHY COUSINS (Observer): I didn't hear anything about the shark predation and interaction of monk seals with sharks. As I understand, sharks not only eat pups and attack monk seals but they also compete with them for food and that the critter cam has shown that monk seals will get a prey item and the sharks will come in and take it away from them. Can you address that?

BILL GILMARTIN: Well, I don't think that's quite right. There is some footage that shows a little bit of interaction between the sharks and monk seals near some prey, and not much of the two paying much attention to each other, but both actually feeding on the same items.

There has been some concern of late of sharks taking monk seals at French Frigate Shoals where some of the food resource problems there have already been discussed. I think what we're seeing there are sharks taking monk seals that are in poor condition, pups that are starving anyway.

We've also had a problem there where we mentioned some of these adult males that have been killing pups. Of course, the males aren't consuming the pups, but that leaves a dead pup in the water nearshore and that's brought in sharks. If you do this enough times over a couple of seasons and you start to train the sharks to come in to feed on pups and then you start having them taking pups that may be in better condition.

Some limited shark control was supposed to have been done at French Frigate Shoals in 2000; I don't know yet if that happened. We'll be finding out the week after next at the upcoming recovery team meeting, and we will find out what the results of it were.

DOUG DEMASTER: Just a comment on recovery team. I think people should be aware that at least NMFS and Fish & Wildlife Service use recovery teams really differently. There are recovery teams in some cases that are established to write a recovery plan or to draft one. Other cases, like Steller sea lions, they're used to comment on research plans. In other cases like monk seals, I think through sheer guile and will, at least on Bill's part, they were asked to do everything: review biological opinions, EISs, research permits. To do that, they had to meet annually.

I think there's a real difference in a recovery team that meets regularly and scheduled the 6th of December every year and other recovery teams that essentially don't know what their longevity is or what their function is or are specifically asked not to get involved with management issues. So I think across the board there is a real disparity in how recovery teams have contributed among the different species.

BILL GILMARTIN: This also was the first NMFS recovery team.

BILL RODGERS: Just another comment on the nature of recovery teams, because you have some observations here about lawyers' behavior, and notice how opportunistic we are. The point is that these comment letters sometimes don't work in the chain of command. You feel the chain of command is not workable. Well, attorneys will grab that letter, try to open up another chain of command. So there are many opportunities where simple comments, which are very important, as you know, in the EIS process and when someone is trying to build another structure these comments that you feel constantly are rejected, and rejected and rejected, there's a measure of success in another forum.

BUZZ THOMPSON: Just a follow-up question to Bill Rodger's comment, which is when you were writing recommendations as part of the recovery team, who did you consider your audience to be?

BILL GILMARTIN: I think the first letter, we had hoped that someone actually in NMFS would pay some attention to it. But I think this recovery team quickly learned that our boss, who was the regional director -- and this person of course has changed over time – of the NMFS southwest region, I don't think there is a time up until possibly the present, this year, that the southwest region has paid much attention to anything we sent them. That's pretty easy to document.

Our recommendations each year would include not just recommendations that were directed to the recovery program, those people were right there so they heard what we had to say, but they be directed to the Navy to do something at Midway, to the Coast Guard, the Fish & Wildlife Service, and we depended on the region to take our letter and then use that information and for them to contact those other agencies and do whatever it was we recommended them to do for the monk seals, close this beach or anything else. That never happened with any of the recommendations we sent them. They never followed through with any of those recommendations in terms of just distributing those to whom they should go to. So there was very little response out of them.

DENISE ANTOLINI: Can I follow up on that and ask you what institutional changes could be made -- I guess given the current political climate, maybe none -- but how did you institutionally get the staff level scientists in the recovery teams, to have their voices heard more effectively, because I think that's another common thread that we're hearing throughout these case studies.

BILL GILMARTIN: I don't know. If the agency wants to stonewall and not pay attention, that's pretty easy for them to do. I think what Bill Rodgers suggested is right; you try to document what you think should be done and then hope that someone outside is going to pick it up and carry the ball for you.

DENISE ANTOLINI: But do you think there needs to be a statutory or regulatory change in the structure or is it just, are we always going to be caught in this --

BILL GILMARTIN: Recovery teams are really only advisory. So it's not like you can say you have to -- we have no authority to say you have to do these things. We have no authority you have to even read what we recommend.

ALISON RIESER: I would just add that under the Marine Mammal Protection Act, the Marine Mammal Commission, created by the statute, is required to advise NMFS on various specific determinations and decisions that they make under the act, and the agency is required to respond, and explain why if the recommendations are not followed. Is that right, Michael?

MICHAEL GOSLINER: They're required to respond but not to follow. They do have to provide some basis for rejecting the recommendations.

BILL GILMARTIN: Which NMFS does very well.

MICHAEL GOSLINER: They do it; I'm not sure they do it well. They do it frequently. They respond well, but I'm not sure that they're always satisfactory responses.

LESSONS FROM THE CASE STUDIES

Dr. Leah Gerber, National Center for Ecological Analysis and Synthesis, Santa Barbara, CA
Dr. Larry Crowder, Duke University Marine Laboratory

Dr. Gerber made some general observations about the agency decision making process, pointing out that decisions in all three cases were made incrementally and that the status quo for federal agencies is to do nothing unless challenged. As a result, the decisions made by these agencies are often contentious. Gerber asserted that when decisions are contentious, the science is more highly scrutinized and controversial and in some cases said to be biased.

She also pointed out that when organizations don't have well-defined goals, their preference is developed during the policy-making process, rather than acting based on predetermined objectives. NMFS by its mandate, which is to develop, use and protect resources, contains multiple objectives that are conflicting. This is essentially a result of having responsibilities under the Magnuson-Stevens Fishery Conservation and Management Act, the Endangered Species Act and the Marine Mammal Protection Act. One question she posed was when these three laws come into conflict, which one takes precedence? She offered that it ought to be the Endangered Species Act, given the nature of its mandates, but that has not happened in the three workshop case studies.

Gerber’s main observation was that advocacy groups play a major role in the policy-making process. Under Section 4 of the Endangered Species Act the agency is required to respond within a certain time frame once a petition to list a species has been filed. She acknowledged that most of the protective actions toward Steller sea lions, sea turtles and monk seals, have resulted from lawsuits or threats of lawsuits.

Scientists need to clarify a number of legal definitions for their own use, in order to be able to use science as a basis for decision-making. Gerber cited the example of species recovery. There is not currently a clear definition of when a listed species’ population can be considered legally recovered, nor are there clear criteria for classifying species as endangered or threatened. She noted that the Steller sea lion is probably one of the most abundant species that are listed under the Endangered Species Act. There is some suggestion based on population modeling that Steller sea lions perhaps should not be listed as endangered given their abundance. Additionally, she noted that definitions for critical habitat are unclear, as are criteria for determining how much habitat to protect.

There is a vast difference between ‘jeopardizing the continued existence of ‘a species and promoting its recovery. Gerber made the point that “in many cases a federal action might potentially not have a strong jeopardy on the persistence of a population but that action, if it were not taken might promote recovery.” Another definition which she argued needs clarification is population structure, what are the appropriate management units, for Steller sea lions in particular.

She emphasized the importance of educating the public as the public plays an important role in influencing the policy-making process, and defining research and management priorities. She advocated proactive management as a way to improve the prospects for endangered species, which requires that managers recognize the problem before it becomes a crisis. Graham Caughley defines this as the “small versus declining population” paradigm where in conservation biology scientists are too often focused on populations that have been driven to critically small populations and worried about how to go about recovering these populations. Instead, conservation biologists should be focusing on populations that may be relatively abundant but may also be showing signs of potential decline.

The science-policy-stakeholder community that deals with the ESA needs to develop approaches to recognize and address conflict over management practices. There are differing objectives between the stakeholders and within agencies in charge of implementing the Endangered Species Act that need to be handled more efficiently. Gerber advocated developing quantitative approaches to evaluate how effective protective measures are. A first step is to develop a modeling approach to test and develop experimental predictions for when some protective measure is going to be effective based on what is known about basic biology and the levels of exploitation or other threats to marine populations.

Finally, she discussed incorporating uncertainty. There is a need for methods that explicitly incorporate uncertainty and what we know about the biology as we translate that into policy. She called on the scientists and lawyers to be explicit about what decisions should be based on policy and which should be based on science. One important example is risk tolerance. When talking about persistence of a population, policy people need to be explicit about what is an acceptable level of risk and scientists need to develop approaches to evaluate strategies for testing the effects of protective measures given that level of risk.

Dr. Crowder agreed with Dr. Gerber that the issue of legal definitions was the most important message to take away from these case studies. Clearly, it is a challenge for scientists to apply legal standards to their research. To help other scientists grasp what these terms mean, and how they apply to their conservation biology studies, Dr. Crowder. broke down the phrase “best available scientific information” (BASI) and tried to address some of these issues.

“Best”: Crowder remarked that very often the best available science is totally unacceptable in terms of the quality of data. He challenged participants to think of ways to improve the quality and quantity of the data used before entering into decision-making processes. He encouraged them to find ways to make these improvements in a timely way, rather than after a species or population is already in crisis.

“Available”: With respect to these case studies, national or domestic fishery data are generally available from observers. However, Crowder noted, in the case of longliners, “we don't have a clue what the international fleet is doing. So acquiring the information and having the data that are out there available to analysts is something that is really a serious problem."

"Scientific": Crowder agreed with Dr. Gerber that there is a lack of accepted standards in use among scientists. For example, he cited assessing the trends in sea turtle populations where survey techniques vary widely from place to place. This discrepancy creates questions about how to combine the empirical data that's collected in a variety of locations, even if the same fishery acts on a given stock.

"Information": Crowder said "Information is an interesting term. I learned way back in graduate school, samples aren't data. I still have a room full of samples. So the very fact that somebody's been out collecting data doesn't mean the data is in a digestible or accessible form. Opinion is not information. And I think that trying to maintain our objectivity about evaluating the information we have is a bit of a problem."

A key question Crowder posed is whether conservation scientists can acquire data in a timely way to address issues brought up by commercial fisheries and endangered species interactions. Research that scientists began recommending in anticipation of an upcoming problem decades before is often initiated only once a population is at a crisis stage. Crowder offered, "I found it fairly easy to make a pretty good career out of anticipating things that NMFS needs to know five or ten years from now but they haven't gotten around to doing. If you could be two or three years into it by the time they realize they have a problem, of course they come to you for help."

Crowder also discussed what he referred to as "the tyranny of uncertainty." When he was on a sea turtle expert working group for the southeast sea turtles, he listened to scientists arguing with environmentalists and fishers about whether or not 'takes' translate into mortalities. A scientist might think of an inexpensive way to answer that question through scientific research. But often, the group would choose not to pursue the research proposal because they prefer to argue from ignorance than to have the data to resolve that conflict. In the past, Crowder noted, when federal agencies had little or no information on which to base a decision, courts would defer to the agency's expert judgment, and find that they were meeting the BASI standard.

More recently, as in these case studies, having no information is no longer a defense. Crowder asked, how can agencies provide better incentives for accumulating information? He said "it seems that there are sometimes disincentives because people can argue from ignorance what they want to argue and additional data may put their arguments at risk." However, he noted that NMFS has evolved considerably in recent years, and is doing a better job of meeting the BASI standard.

He noted the expansion of stakeholders that have occurred with respect to marine issues in recent years. Formerly, when NMFS was the Bureau of Commercial Fisheries, the agency worked only with the commercial fishermen to try to enhance catches. In the mid-1970s the agency became the National Marine Fisheries Service, still primarily serving the stakeholders, commercial fisheries and also recreational fisheries. Crowder remarked of the changing agency, “Then Flipper arrived on television and concerns about marine mammals, particularly in the Eastern Tropical Pacific tuna purse seine fishery and the associated legislation from the 1970s, really raised the bar for National Marine Fisheries Service in trying to meet a wide variety of requirements. So now they're trying to maintain sustainable fisheries, marine mammals and protected species, endangered species at the same time, and that's really a significant challenge for them.”

Over time the community interested in ocean issues is expanding rapidly. There is increasing concern by people who have never seen or never will see a Pacific leatherback, that Pacific leatherbacks continue to exist in the ocean. Moreover, Crowder explained, this new community is willing to put money behind that concern, and the concern can lead to litigation. Crowder posed the question of how long the NMFS will continue to have fisheries as the main subject in its title. Will it become or have to become more of a conservation-oriented organization? Many scientists who were trained as fishery biologists have watched fisheries and associated non-target species be depleted in a classic curve sloping downward over time, Carl Safina’s “fish curve,” and have become conservation advocates. Crowder remarked, “when does the agency need to reconfigure its mission or when do we need to encourage reconfiguring the mission to deal with doing our work sustainably versus living the fish curve through one species after another after another? What prevents us from getting out front with the science?”

He characterized the general problem as follows: It begins with a general lack of recognition that there's a problem, as Scott Eckert points out with the Pacific leatherback beach counts. At first the scientists who are probably most concerned about these species have to recognize that here may be something going on, then to talk about that problem. A key point is the importance of scientists communicating their concerns in clear, concise terminology for the public. Since management is about changing human behavior, not actually managing a sea turtle or marine mammal population, economic, social and political issues are important.

Some time in this process, there can be novel science that intervenes in a really important and influential way, such as the fatty acid work for monk seals, or genetic stock identification tools. These tools which allow scientists to say where an individual turtle taken in a fishery came from, or how much lobster can be safely removed without causing jeopardy to monk seals. The results of such studies are critically important to management.

Finally, Crowder advocated the responsible use of population models. “Suppose they take a hundred turtles or a thousand turtles or ten thousand turtles -- well, ten thousand sounds like a big number, so most people would say that's important. But does ten sound like a big number? It's only a big number in the context of all the other sources of mortality and all the other things that influence growth rates, and the only way to put that all together is in some kind of population model.”

One thing to be aware of is what Crowder the science diversion stage, where resources become available to scientists to gather information but this can lead to further delays in acting, where scientists and politicians continue to study the problem rather than act to solve it. One thing that can prevent the science diversion track is to have a very active recovery team process, such as take reductions teams mandated under the MMPA. The job of the take reduction teams, which are comprised of various stakeholders, is to reduce takes of marine mammals in a particular fishery and they just have to go at it and fishery until they make some progress.

Finally, Crowder encouraged working together with a stakeholder on solutions to end up collaborative solutions to conservation challenges. He described his experience with turtle excluder devices in trawl fisheries in the Southeast U.S. As Crowder said, "it went nowhere until the commercial fishermen became involved…we're not going to come to a solution unless we come to a solution together."