ALASKA LEGISLATURE SPECIAL COMMITTEE/SUBJECT FILE 8672
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Response: Objective A can be tested using control groups as described in the study plan. Our ability to test Objective B will depend on movement of a portion of the radio-instrumented sea otters into oiled areas. Translocation is a necessary component of the rehabilitation process. There is no basis for the statement that translocation effects will likely play a larger role in survival than oil exposure. The circumstances involved in translocation in this study are not similar to those in historical translocations, and effects thus cannot be assumed to be the same as in previous translocations. Furthermore, because of our ability to monitor the radio-instrumented sea otters, we will be able to distinguish between mortality and emigration.

Comment: Field methods of this study are inadequately described: the frequency of relocation of instrumented animals is not given; the health assessment criteria are not described; and it is not clear how females will be distinguished from males during counts of the study populations. (ESC)

Response: As stated in the study plan, it was intended to relocate the instrumented sea otters at least biweekly. However, weather conditions in the Sound can be severe, especially in winter, and movements of the otters can make it difficult to track all of them on a regular basis. Health of the surviving otters is not being directly assessed; rather, we are determining biweekly rates of survival, where feasible. In this study, there are no counts being made on study populations that require distinguishing males from females. Sex is known for each of the instrumented otters.

Comment: Analytical methods are not detailed enough. Sample sizes may be too small to be meaningful. A sample of 45 rehabilitated otters with diverse characteristics is too small to detect differences that can be extrapolated to the rehabilitated otter populations. (ESC)

Response: A sample size of 45 instrumented sea otters is sufficient to measure effects that can be extrapolated to the rehabilitated otter population. The 45 animals are generally representative of the otters at the rehabilitation centers.

Comment: Study objectives are compromised by the fact that otters were captured, maintained in captivity, stressed and translocated. Oil exposure is only one factor that has potentially impacted the otters. Several otters were judged to be unoiled by otter center workers when they were admitted for rehabilitation, so the findings will pertain primarily to the effects of captivity and translocation. (ESC)

Response: We recognize that the fate of the sea otters following release may be influenced by various factors including oiling, cleaning, and captivity. Records were kept on the degree of oiling at arrival and the clinical history of the otters while at the centers, and these will be considered in interpretation of the data. Hydrocarbon burdens in blood and fat (collected prior to release) will be available for instrumented sea otters and will provide a basis for judging the effects of oiling on the fate of the otter.

TERRESTRIAL MAMMALS

Comments on Terrestrial Mammals Studies - General

Comment: Although the study descriptions are improved over those provided in 1989, the 1990 studies are inadequately detailed to make a proper scientific evaluation. (ESC)

Response: The Trustees believe that sufficient detail was provided to allow scientific evaluation. Additional information is available from referenced publications.

Comment: The omission of results from related 1989 studies (Terrestrial Mammal 1, Terrestrial Mammal 3, Terrestrial Mammal 4, Terrestrial Mammal 6) makes it difficult to understand the justification for their continuation into 1990. Given the lack of mortality, substantial indications of injury would be necessary in order to justify these studies. (ESC)

Response: Because these studies are conducted for purposes of litigation, results of studies are confidential; they will be made public either in the course of the litigation or after final reports are prepared and authorized for release.

Comment: Terrestrial Mammals 1, 3, and 4 lack evidence of exposure of these species to oil. There are no documented mortalities of deer, river otters, or brown bears in the 1990 study descriptions, so there is no reason to continue these studies. It is extremely unlikely that these species or black bear (Terrestrial Mammals 2) or mink (Terrestrial Mammals 6) could have been significantly impacted by the spill. (ESC)

Response: All of these species use intertidal habitats that were heavily impacted by oil. Therefore, there is significant potential for contact with oil and resulting injury.

Comment: Relevant law requires that the anticipated costs of the assessment be less than the anticipated damage amount in order for the assessment costs to be reasonable. Many of the studies, notably those regarding Terrestrial Mammals, violate this requirement. (ESC)

Response: The NRDA regulations, which are optional, indicate that the anticipated cost of the assessment should be less than the anticipated amount of damages. See 43 C.F.R. § 11.14(ee). There is no requirement that each individual element of the assessment meet this test. Rather, individual elements need only lead to an increase in accuracy and precision in the assessment that outweighs their costs. See Id. The Trustees believe that the assessment meets these standards.

Comment: The Plan contains studies designed to do scientific research that will not lead to identification of injuries, and the costs of such research cannot be recovered under the NRDA regulations, but should be funded by alternate means. Examples include Terrestrial Mammal 6, the toxicity study of which is an inappropriate laboratory simulation of actual environmental conditions. (API, ESC)

Response: Terrestrial Mammal 6 is very clearly focused on determination of injury from ingestion of sublethal doses of oil. The use of a laboratory simulation is a reliable, scientifically accepted technique that will produce results that can be extrapolated to mink and other related species impacted by EVOS.

Comment: The need for any studies of terrestrial mammals should have been motivated by 1989 data wherein injury to mammal populations, sufficient to merit further study to define restoration needs, was documented. Since it is very unlikely any such injury to terrestrial mammal populations was documented, the justification for these studies is highly unquestionable. (API, ESC)

Response: Terrestrial mammal studies are expected to provide information on injury and to support restoration planning. Specific results of 1989 studies are confidential.

Comment: The natural variability of species is not adequately addressed. (API)

Response: Studies are designed to account for major sources of natural variation.

Comment: The studies concerning Sitka black-tailed deer and black and brown bear disregard the fact that these animal populations are in good health and abundant, evidenced by the fact that the State still permits hunting of these animals. Had there been a sizable mortality of terrestrial mammals or a significant exposure potential to petroleum hydrocarbons, these species would have been investigated under the joint NOAA, ADF&G, and Exxon subsistence program. (ESC)

Response: The presence of a huntable surplus does not mean that significant injury did not occur as a result of EVOS. The joint subsistence program investigated the possible impact of hydrocarbon contamination on the health of humans who consume various species. It did not consider impact on the animals themselves.

Comments on Terrestrial Mammal Studies - Specific

Terrestrial Mammal Study No. 3 - River Otter and Mink

Comment: The study's overall objective, that of determining if the EVOS will have measurable effects on river otter populations, cannot be achieved given the absence of valid pre-spill population data. (ESC)

Response: Comparison of total numbers and survivorship between oiled and unoiled areas over several years will allow an assessment of injury to populations. Other information being collected on direct effects, food habits, and habitat use will be a valuable aid in interpreting population data.

Comment: Some of the specific objectives associated with food habits and habitat use may be achievable. However, observations of differences in certain parameters cannot be related to potential impacts from the EVOS. (ESC)

Response: All objectives were formulated with the expectation that differences can be related to EVOS. The study was designed to account for significant natural variation that could influence results, and, therefore, it is expected to detect injury from oil contamination.

Comment: The "food habitats" and "habitat use" sections of this study will not show any negative impacts on the population of otters. They may show otters are adjusting to new habitats. (API)

Response: It is reasonable to examine closely both food habits and habitat use because both are expected to show injury given the heavy contamination of intertidal areas that are critical for otter survival. Oil contamination does not create "new habitat". Any adjustments detected will be the result of otters responding to habitat injury.

Comment: This study will provide data that may be useful for long-term goals of managing Prince William Sound but not directly related to oil spill impacts. It will provide much information about the habitat use and movement patterns of this species, but it will not measure any population impacts. (ESC)

Response: This study focuses directly on investigation of injury from EVOS. Comparison of oiled versus unoiled areas is a key study design feature.

Comment: The Plan does not make clear that sampling programs will produce information necessary to prove that a statistically significant portion of the expected biological variability is a function of hydrocarbon contamination as opposed to other natural factors. Terrestrial Mammal 1, Terrestrial Mammal 3 and Terrestrial Mammal 4 suffer from this defect, such as severe winters, predator/prey relationships, and disease, which clearly affect key life cycle events of various species. (ESC)

Response: The study plan provides sufficient detail to allow evaluation of the statistical validity of the design. Additional details can be found in referenced publications.

Comment: This study will not identify avenues of oil contamination. It does not distinguish between contamination via digestion and contamination via thermal absorption or grooming. This study should provide for the coordination and integration of data from river otter food habits and from studies of the species on which they prey. This study will not detect simultaneous reductions in the populations of river otters and their prey species. (NWF)

Response: Identification of specific avenues of contamination is not part of this study. It will rely on other NRDA projects for information about contamination of the river otter food chain. A full suite of coordinated environmental studies is being conducted in the oiled area.

Comment: There is insufficient evidence that river otters were exposed to oil. (API)

Response: River otters were exposed to oil. Intertidal habitat critical to this species' survival was heavily impacted by oil.

Comment: "Direct effects" and "population change" parameters can show biological effects which cannot be quantified. (API)

Response: The study design is expected to allow quantification of both direct effects and population change resulting from EVOS.

Comment: It is inappropriate to compare an impacted site to a reference site for density comparisons when, in all probability, neither site has any valid pre-spill data on population trends or variance. (ESC)

Response: The lack of historical data on populations does not invalidate comparison of density between oiled and unoiled areas.

These two areas will be monitored for several years and they are similar enough that differences can likely be attributed to injury from the EVOS.

Comment: This study is not cost effective and will only assess short-term impact. There will be a quick recovery from any short term impact on otter density. River otters mature rapidly and have relatively large litters. The "takes" planned in this study will probably result in more otter fatalities than have been observed since the EVOS. Very little of this study will relate to natural resource injury. (ESC)

Response: It is anticipated that this study will be cost effective. The ability to detect injury over time is a function of the duration of the study. The design will produce reliable results and is focused on assessment of injury. Recovery rates for the otter population are difficult to predict. Plans to collect otters were canceled.

Comment: Study locations are not described well. (ESC)

Response: Sufficient detail concerning study locations was provided to allow evaluation of the project; more detailed descriptions might have jeopardized the study.

Comment : The radio transmitter and radioisotope implant techniques are not described adequately. (ESC)

Response: Sufficient detail concerning transmitter and radioisotope implants were provided to allow evaluation of these techniques. Additional detail is available in referenced publications.

Comment: Statistical procedures are vaguely defined. It is not clear how the effects of the oil spill are to be estimated and tested statistically. The level of effect being tested and the effort (number of samples, replicate subsamples, etc.) needed to detect that effect were not given. The sampling effort does not appear to be appropriate to meet objectives. The probability of declaring an effect when there really is not one (Type 1 error) is not given. The probability of declaring an effect when there really is one (Type II error) is not given. It will be difficult to determine if a statistically significant effect was due to the oil spill or to natural variation. (ESC)

Response: Sufficient detail concerning statistical procedures was provided to allow evaluation of tests. Additional detail is

available in referenced publications. The study is undergoing rigorous statistical analysis.

Terrestrial Mammal Study No. 4 - Brown Bear

Comment: Objectives A-C are concerned with possible physiological effects and mortalities of brown bear due to the EVOS, and cannot be achieved primarily because no direct exposure pathway to spilled oil is outlined. Also, there is insufficient information on how tissue and feces analyses are to be related with mortality. (ESC)

Response: Intertidal foraging is the likely exposure pathway. Other studies will provide details on contamination of forage species.

Comment: Objective D's estimation of the adult population density of the study area has nothing to do with natural resource damage assessment, particularly since no historical database exists. (ESC)

Response: Estimation of population impacts of the EVOS is an important part of damage assessment. Estimates over several years will provide trend information necessary to quantify injury.

Comment: Population estimates for only two years, 1990/1992, cannot be used to predict any trend or identify any impact from EVOS on brown bear populations on the Alaskan Peninsula. (ESC)

Response: Estimates will be obtained for several additional years if the study is continued. In addition, monitoring radio- collared bears will provide the opportunity to identify bear mortality and determine whether it is oil-related.

Comment: Two assumptions used in the model to estimate adult population levels are very weak: (1) the brown bear population is geographically and demographically isolated; and (2) all brown bear have equal capture probabilities that are constant over time. (ESC)

Response: Potential difficulties with these two assumptions are acknowledged in the study plan and details concerning how they will be addressed are presented.

Comment: The significance of hydrocarbons in fecal samples, particularly as it relates to ingestion, is not discussed in sufficient detail to determine its validity. No literature is

cited as to how this technique has been used with previous spills. (ESC)

Response: The presence of hydrocarbons in fecal samples will be an indication of exposure through ingestion of contaminated food. Other NRDA studies will provide information on contamination of individual food items. We know of no studies, other than EVOS, where fecal analysis has been used.

Comment: No explanation is given why blood is to be analyzed for packed cell volume and percent hemoglobin. These measurements are not likely to establish any impact on the bear from spilled oil. (ESC)

Response: Analysis of samples has been delayed pending additional review of the range of blood parameters that can be the most useful indicators of injury.

Comment: This study will provide data that may be useful for long-term goals of managing PWS but not data that is directly related to oil spill impacts. (ESC)

Response: This study is designed to focus specifically on assessment of injury from the EVOS, but does not relate directly to PWS populations.

Comment: Study areas in the Katmai National Park, on Kodiak Island, and near Black Lake are not described as to exact location and study area size. (ESC)

Response: Sufficient detail on study areas is provided to allow evaluation of the work.

Comment: The spill area site in the Katmai National Park is not a good choice for determining injury to brown bear from the EVOS. The bear population age structure, particularly for old males, would be quite different in Katmai because the bears are protected, not hunted. This contrasts with the control areas where hunting is permitted. As a result, some population difference might be improperly assigned to oil spill effects. (ESC)

Response: Age structure difference between the study areas is recognized and is considered in the study design. We expect it will be possible in analysis of the data to isolate this variable and, therefore, avoid improperly assigning differences to impacts of EVOS.

Comment: The Plan does not make clear that sampling programs will produce information necessary to prove that a statistically significant portion of the expected biological variability is a function of hydrocarbon contamination as opposed to other natural factors. Terrestrial Mammals 1, 3 and 4 suffer from this defect, such as severe winters, predator/prey relationships and disease, clearly affect key like cycle events of various species. (ESC)

Response: Sufficient detail is presented to allow evaluation of the statistical design and sampling programs. Natural variation is considered in selection of study sites and appropriate statistical tests.

Comment: The Trustees should consider collection of tissue samples from denning females and their cubs, as well as tissue samples from fetuses of necropsied adult females. (NWF)

Response: Collection of tissue from denning animals will be considered. A full suite of appropriate samples is planned for collection from any animals found dead.

Comment: The stress caused by the capture of live bears, the implantation of radio transmitters, and the drawing of blood has not been adequately considered. (API)

Response: Stresses caused by capture of bears are well understood and will be considered in interpretation of results. No transmitters are being implanted by this study. It is highly unlikely that drawing of a small amount of blood will cause any significant stress.

BIRDS

Comments on Bird Studies - General

Comment: The bird studies ignore current scientific literature on the effect of oil on birds, which indicates that bird populations recover extremely rapidly after an oil spill. They also ignore evidence of recovery of bird populations in PWS and the Gulf of Alaska, which confirms a healthy density and diversity of resident and migratory species there. Failure to rely on the existing literature, which would have enabled the Trustees to narrow the field studies, is contrary to the NRDA regulations. (API, ESC)

Response: The bird studies were developed with full cognizance of information available in the current scientific literature. There is considerable information in that body of literature to indicate that bird populations do not always recover rapidly after an oil spill. This is especially true of long-lived species with low reproductive rates. Further, it is essential to measure the impact resulting from individual environmental calamities to account for unique circumstances which may have a bearing on impacts to birds. The EVOS was unique in its size as was the biological richness of the area where it occurred. Observation that there are still birds in the spill zone does not constitute scientifically objective information that ". . . confirms a healthy density and diversity of resident and migratory bird species there." Studies are essential before any such conclusion can be drawn.

Comment: The Plan contains studies, such as Birds 4 and 5, that are designed to do scientific research that will not lead to identification of injuries. The costs of such research cannot be recovered under the NRDA regulations, but should be funded by alternate means. (API, ESC)

Response: All bird studies were designed specifically to provide information that would be used in assessing injury. Although some objectives, in and of themselves, are not able to define injury, they provide information that, in conjunction with other data, do support injury determination.

Comment: A disproportionate number of bird studies were discontinued with little or no explanation. The reference to cost-effectiveness in the introductory section to the bird studies suggests that the Trustees made a value judgment as to the relative costs of studying damages to multiple species and the injuries to those species without explaining their conclusions. Impacts to certain species cannot be ignored simply because they are more costly to study than other species. The impacts on one species are integral to understanding the impacts on other parts of the ecosystem. (NWF)

Response: The practicality of conducting scientifically credible injury assessment studies was a critical factor in determining whether studies should be conducted. Some studies were discontinued because it was determined that continuation would not provide substantially more information on injuries than had already been gathered. In a number of cases, elements of discontinued studies were incorporated into continuing studies or into base agency programs.

Comment: Birds 2, 3, 4, and 5 are not well integrated, as there is no apparent attempt to correlate census or distribution data with factors other than the presence of EVOS oil. (ESC)

Response: Efforts are being made to correlate survey data with factors other than the presence of oil. Information on water temperature, weather, and other factors are being considered.

Comment: The bird studies involve invasive procedures, including the killing of birds to determine the potential destination of birds that did not wash up on shore after the spill. (API)

Response: Certain bird studies involved the killing of specimens to gather information needed to assess injury, such as histopathology, condition, and hydrocarbon uptake. In all cases, studies were thoroughly reviewed by leading experts and agency representatives to insure that their methods were required, that the take was kept to a minimum, and that the number of birds taken was insignificant to the overall population.

Comment: Studies that disrupt breeding grounds during the nesting season or require the handling of birds to take blood samples should not be undertaken, unless the studies are clearly necessary. (API)

Response: No bird studies were conducted that involved disruption of breeding grounds. Surveys and censuses were conducted utilizing standard methodologies. It is necessary to gather blood samples to measure differences in blood chemistry parameters between populations in different areas.

Comment: Most of the Bird studies do not adequately account for the fact that there was substantial variability in resource levels before the spill or the fact that there is no reliable baseline data. Thus, the statistical detection of differences due to oiling will not be possible. These facts also make it impossible to develop sufficient data to describe the subtleties of historical populations dynamics or to relate any potential response to

extremely low hydrocarbon levels. (ESC)

Response: We recognize that there is a possibility of substantial variability in resource levels before the spill and have designed our program to consider this variability. For example, it is unlikely that variability between years would consistently be different between oiled and unoiled areas. The best way to address this variability for seabirds is to look at long term studies or monitoring efforts; these are available for murres at the Farallons, Semidi Islands, Pribilof Islands, and Bluff colonies in the Bering Sea. Some aspects of murre biology vary, but other aspects such as productivity vary very little. It appears that changes in numbers of murres may vary from one year to the next, but a long term change of any magnitude does not generally occur without some complicating factors of mortality such as oil spills or gill netting interferences.

Comment: The 1990 Plan still does not contain an adequate description of the studies for review purposes: survey techniques are not described in Birds 2, 3, 5, and 13 in sufficient detail to permit the reviewer to determine whether the stated objectives can be met; sampling approaches are defined only in general terms; and the descriptions of the application of statistical models to data obtained from the studies is brief and incomplete. (ESC)

Response: Efforts were made to provide sufficient information in the 1990 Plan to enable the public to understand how studies were to be conducted and how data would be analyzed.

Comment: Except for Birds 1, the studies will not render injury estimates. Birds 2, 3, 4, 5, and 13 fail to identify and consider variables, such as severe seasonal weather, food supply, disease, and commercial fishing activities, that could affect bird populations. Thus, any changes in population size cannot be linked to the spill or to other significant environmental conditions. (ESC)

Response: By utilizing appropriate comparisons (e.g., oiled vs. unoiled, pre-spill vs. post-spill) it is possible to account for these other variables. It is highly unlikely that these variables would consistently affect bird populations in oiled areas and not in unoiled areas. By looking at several species over a number of years at a large enough number of sites, these other causes can be evaluated. If any of these other causes are influential, then it should affect species and sites outside of the oil spill.

Comments on Bird Studies - Specific

Bird Study No. 1 - Beached Birds

Comment: The tracking of birds killed by researchers is an unnecessary study. It is inappropriate when considering the potential injury to the birds and the economic damages to be recovered. Other methods were available to test the accuracy of the count of the dead birds at the time of the spill. (API)

Response: Careful review by expert scientists concluded that, in order to calculate a more reliable estimate of the number of birds killed by the EVOS, it was necessary to kill and radio-track a small number of birds. Other methods were considered and determined to be inadequate to achieve a more reliable bird mortality estimate.

Comment: The killing of birds was not mentioned as part of the experiment, and this portion of the study was not subject to public comment. (API) Bird 1 would have been canceled by the Trustees if it had been made public prior to publication of the Plan in September of 1990. (APSC)

Response: Although detailed information on the collection of birds was not provided in the 1990 Plan, it was specifically noted that carcasses would be radio-tracked to determine recovery rates. The study was approved after thorough review. The number of birds killed was kept to an absolute minimum and the birds were taken from populations not affected by the EVOS.

Comment: The model to be used in Birds 1 is only vaguely referenced and cannot be evaluated without more detail, such as information regarding application of the model, the model's input parameters and its underlying assumptions, and the source and nature of the historical bird density data to be used in the trajectory modeling effort. Although two options for pursuing model sensitivity analysis are presented (but not well described), there is no mention of the criteria for choosing between them. (ESC)

Response: The findings of field studies conducted for Bird Study 1 affected the structure of the model to be used; therefore, it was not possible to describe the model in greater detail before field studies were conducted. General methodologies used may be found in the following references:

Ford, R.G., G. W. Page, and H.R. Carter. 1987. Estimating mortality of seabirds from oil spills. Pp. 848-751. In Proc. 1987 Oil Spill Conference, American Petroleum Institute, Washington, D.C.

Page, G.W. and H.R. Carter (eds.). 1990. Numbers of seabirds killed or debilitated in the 1986 Apex Houston oil spill in central California. Studies in Avian Biology. In press.

The two options discussed for pursuing model sensitivity analysis were presented in order for the reader to be able to evaluate the accuracy of the model results. It was not a matter of choosing between the two options.

Comment: Objectives B and D are not distinguishable as written. (ESC)

Response: Objective B addressed the portion of the study that would look at 10% of the birds recovered on beaches after the spill. Seabirds die for a variety of reasons and some of the birds collected could have died of natural causes and been oiled secondarily.

Objective D refers to assessing mortality of birds by adapting existing bird damage assessment models to estimate total seabird mortality.

Comment: Any mortality estimate rendered by Birds 1 will only be an order of magnitude approximation given the assumptions and uncertainties that modeling will require. (ESC)

Response: The estimate of the total number of birds killed by the EVOS that will result from the model will be significantly more precise than any of the current estimates. Current estimates do not consider the number of uncertainties such as sinking, scavenging, floating out to sea, and failure to recover that are analyzed in the model used in this study.

Comment: Radio-tagging of drifting carcasses may not yield useful information other than sinking rates because trajectories followed by floating birds can be controlled predominantly by weather patterns. (ESC)

Response: This comment is correct in stating that radio-tagging of drifting carcasses may not yield useful information other than sinking rates. The primary intent, however, was to determine sinking rates rather than deposition patterns. Information on decomposition and scavenging rates was also gathered.

Comment: There is insufficient information provided in the description of the carcass drift study for critical review. Lacking are: the source of carcasses for the drift study; the source of information describing the initial state of oiling and decomposition of the carcasses; the locations of carcass releases; the number of samples to be used; and the nature of the transmitters used. (The assumption that the transmitters will remain upright and exposed may be weak, depending on the sea state.) (ESC)

Response: The source of carcasses for the drift study was not mentioned because availability of birds, permits, and logistics of getting to various sources had not yet been fully determined. Carcasses were oiled using weathered Prudhoe Bay crude oil. Carcasses were moderately and heavily oiled. Great care was taken to ensure that carcasses were as fresh as possible. Location of carcass release was not discussed in advance of the release because winds, currents, and sea states had to be taken into careful consideration at the time of the release. Availability of aircraft and boats were also critical factors in deciding the locations of carcass release. Sample sizes were selected that would permit differentiation between effects of various species used, the degree of oiling of the carcass, and the release sites as sources of variation. It was not assumed that the transmitters used would remain upright. Identical transmitters were used in previous experiments with marbled murrelets and did, in fact, remain upright.

Comment: The use of decoys as a calibration tool has several weaknesses, e.g., that they do not match birds in profile and that decoys not found have drifted out of range. (ESC)

Response: Decoys were used in a previous experiment. They were weighted to simulate bird carcasses being used. They do, in fact, act as a reasonable control. Experimental results indicate that the decoys do float similarly to bird carcasses and they did not float out of range.

Comment: As described, the study does not take into account the sensitivity of eagle nests, seal and sea lion haulout areas, and seabird colonies. The 500-foot ASL flight altitude in flights "near the beach" could violate the 1,000-foot ceiling and the 1/4 to 3-mile buffer zones established by the Fish and Wildlife Service, the National Marine Fisheries Service, and the Alaska Department of Fish and Game. (ESC)

Response: Every effort was made to minimize these disturbances as a result of flights conducted for this study. The slight activity

generated by this study was minimal when compared to that generated by spill cleanup activities.

Comment: The assumption that the "average lineal density of carcasses for a given beach type in the unsampled area was the same as that in the sampled area in a given sector" is unsubstantiated. (ESC)

Response: The Trustees recognize the validity of this comment. However, this assumption can be tested by using ESI data (Sensitivity of Coastal Environments & Wildlife to spilled oil, Atlas of Coastal Resources) and comparing this data with oil deposition data. It will be corrected if a relationship is shown between the two.

Comment: The intended use of a 10% sample of freezer-stored birds to reflect the oiled/non-oiled distribution of birds on unsearched beaches may not be appropriate. (ESC)

Response: A 10% sample would give some indication of oiling trends, however, there are plans to look at a larger sample size during the 1991 season.

Comment: The intended use of bird carcass notes and logbooks to indicate the level of effort is improper since it may not indicate the intensity of effort applied at other locations where birds were not found. (ESC)

Response: Logbooks and bird carcass notes may not indicate the level of intensity of effort applied at other locations where birds were not found or they could, in fact, indicate intensity of effort in locations where birds were and were not found. Until logbooks and other sources of information were reviewed it was not possible to know what they would reveal. The model is not dependent upon any one piece of information such as effort.

Comment: There are no necropsies planned for oiled birds, which implies that any bird with oil on it expired as a result of contact with oil. This ignores natural mortality and post-mortem oiling. (ESC)

Response: A number of oiled birds were necropsied during the spill. Estimates of mortality will consider natural mortality. Numbers of dead birds found during 1990 response activities, as well as those found during 1990 damage assessment studies and other activities, will provide useful information in estimating natural mortality.

Comment: The assumption that carcass disappearance rates increase as the birds enter the nearshore environment is questionable. (ESC)

Response: This was simply a description of an understanding, at the time, of the physical process of carcass sinking. This assumption is not critical in any sense to the model.

Comment: This study should take into account the effects of the intensive search effort. (ESC)

Response: This study makes every effort to include all pertinent information on recovery of bird carcasses during the spill, including the intensive search effort.

Bird Study No. 2 - Censuses

Comment: Comparison of the 1990 and 1971 aerial data will not produce a valid determination of injury. This study does not explain how effects of oil spill will be determined. (API, ESC)

Response: Although more recent pre-spill survey information would have been preferable, the 1971 survey data are still useful and provide important information about distribution of birds throughout the Sound. The survey was designed as an index to migratory bird populations and was not designed to provide a total population of the study area. Because this study provides only an index to migratory bird populations, it will not measure actual numbers of birds lost to the oil spill, but will document displacement of birds and loss of habitat use.

Comment: Use of aerial surveys to provide census and seasonal distribution information for comparison with historical data from 1971 is inappropriate. Without knowing data from the surrounding years and being able to compare survey techniques used, it is not likely that the 1971 data are representative of the earlier time period and therefore an appropriate set of baseline data. Population status could have been affected by environmental and other unaccounted-for changes in the intervening years, which cannot be segregated from the effects of the spill. (ESC)

Response: We agree that a direct comparison of post-spill aerial survey data with 1971 aerial survey data may present problems in identifying other intervening events or changes that may have affected bird distribution and population. The aerial survey data provide valuable information on these parameters in 1989 and 1990 and will be used accordingly. Survey dates and methodologies between 1971 and 1989/90 are similar.

Comment: It may be impossible to establish a causal relationship between observed changes and the spill. Objectives A.3 and B.3 cannot be met without long-term studies; they will be compromised on account of the natural variability in waterbird and waterfowl populations. (ESC)

Response: Information from this study [illegible] considered in conjunction with other information to de[illegible]usality of any documented changes. Additionally, this st[illegible]es data between oiled and unoiled areas.

Comment: The study does not indicate whether the level of effort, observer experience, or other critical factors affecting the accuracy of the boat surveys will match those of earlier surveys or whether a similar protocol will be used for collecting these survey data. Further, there is no way to evaluate the reliability or the methodologies of the earlier surveys since they are contained in unpublished reports. (ESC)

Response: Similar protocols to previous studies are used in current surveys, and in all cases, through training and selection of observers, a high level of observer accuracy is assured.

Comment: There is no discussion of count replication or any other survey strategy for achieving a 95% confidence limit, and it appears that the sampling effort will not take into account natural variability, thereby potentially precluding comparisons with historical data. (ESC)

Response: The plans for this study provide information on the number and timing of counts and the statistical tests to be applied.

Comment: The sampling design is not sufficiently described for review purposes. (ESC)

Response: The sampling design is described in detail in the study plan.

Comment: The methodology used to identify the presence or absence of oil during the boat surveys is not disclosed and it is not clear that other variables that can influence bird distribution and densities are being recorded. (ESC)

Response: The presence or absence of oil is being determined by use of available data sets on oil on water (ADEC and NOAA data) and oil on shorelines.

Comment: The assumption in the aerial surveys that visibility bias affecting surveys in different years with different conditions and different observers is similar probably is not correct. (ESC)

Response: The assumption by the reviewer that aerial surveys were conducted during 1989 and 1990 under different conditions and using different observers is incorrect. The visibility bias is similar because the same observers, pilot/observers, and aircraft were utilized for both years. Conditions (weather) were also similar in that minimum weather conditions for conducting the surveys were strictly adhered to. The timing was also similar since each survey in 1990 was accomplished within a few days of the date that survey was done during the previous year.

Comment: As described, the study does not take into account the sensitivity of eagle nests, seal and sea lion haulout areas, and seabird colonies. The 150-foot ASL flight altitude in flights 200 meters offshore appears to be in conflict with the 1,000-foot ceiling and the 1/4 to 3-mile buffer zones established by the Fish and Wildlife Service, the National Marine Fisheries Service, and the Alaska Department of Fish and Game. (ESC)

Response: Every effort was made to minimize the impact of these flights to bald eagle nests, seabird colonies, and seal and sea lion haulouts. The frequency of these flights resulted in substantially less potential disturbance than the more frequent flights and other human activity associated with EVOS cleanup activities. Flight 150 ft above [illegible] level is necessary for accurate species identification. [illegible]ter offshore distance was necessary for survey width [illegible] duplicate Haddock's aerial survey done in 1971 and a[illegible] the same area being surveyed by the boat crews. Once [illegible]er survey width (200 meters off each side of the surv[illegible] was started, it was continued for continuity of the s[illegible]hroughout the study. During 1990, no surveys were done [illegible]the eagle and seabird nesting period, as surveys were accomplished during the spring (May) and fall (October). Seal and sea lion haulouts were gi[illegible]en a wider berth when possible. Very little disturbance was observed on the seal and sea lion haulout areas during the aerial surveys as the aircraft were operated at a reduced power setting (i.e., noise abatement procedures) in order to reduce the airspeed for survey purposes.

Comment: The Plan appears to put more emphasis, unjustifiably, on the more variable spring and fall surveys. (ESC)

Response: Greater emphasis is not placed upon the spring and fall surveys than on the winter survey which is considered to include

the most stable migratory bird population. If any emphasis is placed upon one survey over the other, it would be the winter and spring surveys because those cover the time period of the initial oil spill. The fall survey usually only covered PWS because inclement weather encountered on the Kenai Peninsula portion of the study area prevented complete survey of the Peninsula during both 1989 and 1990.

Comment: The statistical procedures for data comparisons are vaguely defined; it is not clear how spill effects will be estimated and tested. (ESC)

Response: As was previously stated, this survey is intended only as an population index to cover the entire shoreline of the study area. It was not a survey of selected sample areas that would then be extrapolated out into a total population of the survey area. All surveys were done using time proven, standardized aerial survey techniques used throughout the FWS for surveying migratory bird populations.

Comment: It is not clear from the Plan how studies at Naked Island reflect on injury elsewhere. While these studies may estimate changes in local density, they will not establish a causal relationship between such a change and the spill. (ESC)

Response: The availability of pre-spill information on certain species, such as pigeon guillemots, on Naked Island provided a valuable opportunity to compare pre-spill and post-spill information.

Comment: The ability to measure a change in marbled murrelets is debatable; the available historical data from the 1979-81 era may be less useful because of their age. (ESC)

Response: The boat surveys replicate counts made in previous years and will provide information on changes in marbled murrelets populations from previous years.

Comment: The proposed Kodiak Island transect surveys do not appear to have any baseline data for comparison. If this is the case, this work cannot be used to determine injury; it can only further research. (ESC)

Response: The Kodiak Island survey transects (winter) have 10 years of pre-spill data. There were no pre-spill summer surveys at Kodiak and these surveys have been discontinued.

Bird Study No. 3 - Seabird Colony Surveys

Comment: It is unclear, given the apparent similarities of Birds 2 and 3, why these two studies were not combined. (API)

Response: Bird Study 2 deals with distribution and abundance of many waterfowl and seabird species in all habitats throughout the year while Study 3 concentrated on seabird colony numbers and reproduction. They require very different methods and efforts.

Comment: Comparison of the 1990 and 1971 aerial data may not indicate injury. It is unclear how effects of oil spill will be determined in this study. (API, ESC)

Response: Bird Study #3 did not utilize information from any aerial surveys.

Comment: The use of the Semidi Islands as a control site is inappropriate because these islands are not representative of the habitat in the spill area. They are relatively far removed from many of the study sites and are affected by different oceanographic conditions and environmental influences. (ESC)

Response: Evidence suggests otherwise. If a control site such as the Pribilof Islands in the Bering Sea had been chosen, these comments might have had some validity. However, the Semidis are right on the edge of the spill, contain the closest and most comparable large murre colonies, are positioned on the continental shelf in a similar fashion as the Barrens, have exhibited similar sea water temperatures, and are in the same oceanographic regime as the Barren Islands, if not that of PWS. In addition, there have been feeding studies of diving alcids and the food they bring to their young which support this comparison. The Semidis are one of the few close sites in the Gulf of Alaska where we have a more continuous string of yearly data on both murres and their reproduction.

Comment: It is unclear whether the proposed census study properly accounts for the diurnal variability in nest attendance of the various species that occurs even during the stated study hours. (ESC)

Response: A number of studies have demonstrated that day-to-day variability is more of a concern when determining an adequate degree of precision than hourly or diurnal variation, provided that the hourly variation by censusing at certain hours is minimized. The stated study hours are the standards used at this time in Alaska.

Comment: The use of boat- and land-based surveys is problematic; the different levels of reliability of these surveys may make comparisons questionable. (ESC)

Response: Boat-based surveys are being compared to boat-based surveys and land-based are compared with land-based at particular sites, thereby coming up with consistent comparisons of relative indices. One type of survey may be preferable if choices are available since precision of estimates will be easier to refine, but standardized methods and enough replicate counts on different days will refine precision enough for either method to evaluate the large degree of change that is apparent at this time.

Comment: Much of the historical data is too dated to be [illegible] Its use will limit the Trustees' ability to measure pop[illegible] change for some species and will make difficult any link [illegible] change in population status and the spill. (ESC)

Response: If Bird Study 3 only concentrated its effort on one [illegible] where there were gaps in the historical baseline data, there would be more concern with this issue. Instead, the approach has been to look at many different sites and several different species, some of which have a much better historical baseline of data, with the idea that any effect caused by something other than the oil spill should show up at other sites and with other species in some consistent pattern. This certainly would be true for any large degree of change, if not small changes.

Comment: Statistical models are too vaguely defined. It is not clear how the effects of EVOS will be determined, particularly given the natural variation due to time and locations. The probabilities of Types I and II errors are not given. (ESC)

Response: This project has done no modeling. This study has relied on the advice of highly regarded professional statisticians who have looked at our data and treated them appropriately with programs such as SAS. Fieldwork has attempted to refine precision of estimates with as many standardized replicates as field conditions will allow.

Bird Study No. 4 - Bald Eagles

Comment: Bird surveys indicate that species density and diversity are returning to pre-spill norms; eagle surveys show more than 1,000 active nests in previously oiled areas with normal numbers of live chicks, and the subsistence study indicates that eagles' food supply and habitat are no longer in danger. This evidence should have been taken into account in the Plan. (ESC)

Response: Pre-spill norms for bald eagle density or of density, distribution, and diversity of prey are essentially unknown. FWS surveys found something less than 1300 bald eagle nests in the area of PWS that were surveyed. The survey area included portions of eastern PWS and areas not oiled in western PWS. Many of these nests were not active and many of the nests that were active in the early part of the breeding season were not successful. To say that there were 1,000 active nest in previously oiled areas does not consider all relevant data.

Comment: The eagle studies should be discontinued given the results of the 1990 surveys indicating the rapid recovery of this species and the health of its habitat. (ESC)

Response: Although 1990 surveys indicate eagle populations may be recovering from losses suffered in 1989, complete recovery may take several years. Long-term effects of losses of breeding and sub-adult eagles, the 1989 reproduction, and the progeny of these birds that were lost due to the spill will not be evident for at least one generation of eagles or 5-6 years (the time it would have taken for the lost progeny to be recruited into the population as breeding adults).

Comment: It is not clear that this study has addressed potential impact of radio tagging and the taking of blood from bald eagles. (API)

Response: Radio-tagging of bald eagles with transmitters similar to the ones used in this project has never been shown to have detrimental effects on study birds. Transmitters weighing up to 90g have been used in other studies, while those used in PWS weigh only 60g. To date no transmitter or harness-related mortalities have been documented. During this study a maximum of 13cc of blood was drawn from captured eagles. Studies by Dein (1986) and Cooper and Eley (1979) indicate up to 47cc may be taken without harmful effects to eagles.

Comment: Bird 4's use of eagles from the Copper River Basin in the survival and productivity research is inappropriate since eagle demographic data from that area (differences in habitat, food supply, and the timing of egg-laying), which is well outside the zone of the spill's impact, is not relevant to the determination of injury. (ESC)

Response: It is currently unknown where all eagles from the Copper River Basin winter. One objective of this study was to determine if birds from the Copper River were potentially impacted by the oil spill. Only one eagle from the Copper River was radioed in 1990.

This eagle is currently wintering in PWS. It is important to study areas adjacent to PWS for potential oil-related impacts since eagles range widely.

Comment: The population survey, radio-tracking, and productivity survey components of this study are research-oriented and should not be included in the Plan. (ESC)

Response: Without studying the population and productivity trends of bald eagles in PWS it is impossible to identify impacts of the spill. By comparing trends between oiled and unoiled areas within a similar geographic region, variation in environmental and prey base conditions are minimized. In addition, without radioing individuals, it would be unknown which areas have been impacted. For example, oiled material was found in one nest distant from oil impacted beaches. It is also important to identify how far eagles range to determine which areas may have been affected by oil or if eagles from oiled areas are moving to areas with cleaner shorelines. Radio-tagging also yields information on survival of eagles from oiled areas vs. eagles from unoiled areas within a similar geographic region. The location of radio-tagged eagles found dead is useful in calculating how many eagles may actually have been killed as result of oil spill. In addition, radio telemetry is useful in validating population surveys by helping to evaluate potential biases such as seasonal movements of eagles.

Comment: It is not clear whether Objectives A, B, and C can be met to the degree of accuracy and certainty stated in the Plan. Even if these objectives can be met, the poor understanding of the baseline may hamper injury determination efforts. (ESC)

Response: The survey of resident bald eagles as well as objectives B and C were met in 1990 by following the procedures outlined in the proposal. Wintering bald eagle surveys were determined to be unfeasible due to weather conditions. We believe comparisons of surveys conducted in 1982 to those from the current study are valid. Procedures established in 1982 were followed in 1989 and 1990. All population plots randomly selected in 1982 were flown in 1989 and 1990. In addition, the shorelines of all islands in PWS were surveyed in 1989 and 1990. By surveying all shorelines, populations can be analyzed on a micro scale (i.e. eagles/km of oiled vs. unoiled shoreline) as well as on a macro scale (east vs. west PWS in 1982, '89, '90). Continuation of these surveys will provide documentation of trends since 1989. Since 1982, with the exception of the oil spill, we see no reason for a decline in productivity or densities of bald eagles in PWS as data from other parts of coastal Alaska indicate bald eagle numbers have been increasing. Populations have undoubtedly been increasing in PWS since the bounty years, and prey species such as pink salmon have

increased in abundance due to growing hatchery production.

Comment: The highly weathered and non-toxic state of EVOS oil in 1990 suggests that costs and capturing activities associated with Objective D are not warranted. Short-term reductions in productivity have little affect on eagle populations. (ESC)

Response: There has never been an oil spill of this magnitude in an ecosystem similar to that of PWS. The long term sublethal effects of North Slope crude oil are unknown. Currently, too little is known about the survival of all age classes of eagles to create an accurate population model. Therefore, the effects of short term reductions in productivity in this long-lived species are unknown.

Comment: The 1982 baseline data is too dated to be valid and may not reflect pre-spill conditions. There is no assurance in the Plan that the data to be obtained in this study will be collected in a fashion similar to that of 1982. Other environmental factors may have affected eagle populations since 1982, so the study may not be able to demonstrate that a change in eagle populations between the two sets of data is spill-related. (ESC)

Response: Population surveys conducted during this study have been conducted in the same manner as those in 1982 (Hodges et al. 1984) as described in the review plan. Surveys plots, aircraft, and experience levels of personnel were similar. There has been no cause for a major decline in eagle populations between 1982 and 1989 in PWS, and the population should be stable or expanding. We are also comparing areas of oiled and unoiled shoreline within similar geographic regions to examine relative changes in the numbers and densities of eagles attributable to the spill.

Comment: The locations of the oiled and control sampling areas for the population surveys are not described and there is no indication of criteria that will be used to distinguish these areas. (ESC)

Response: Sampling areas vary depending on what is being sampled. Some sampling areas for this study are based upon the location of nest sites, and the amount of oiling along shorelines in the immediate vicinity of the nest can be determined by reference to maps developed by ADEC. Sampling areas for free flying birds is also based upon where they are found. The oiling status of the trapping site is, of necessity, a more or less well quantified measure due to the mobility of the eagles. Eagles trapped in eastern PWS were considered to have been from unoiled areas. Eagles from areas such as Northwest Bay were considered to have come from oiled areas. Eagles from locations such as the southern

shore of Green Island were identified as coming from unoiled shorelines as most shoreline in the area was untouched by oil. This conservative approach raises the likelihood that eagles exposed to oil were included in the pool of birds considered to be from unoiled areas, but this strengthens the significance of differences that may be observed.

Comment: Plot selection criteria are not described adequately; plot selection methodology and number of plots are needed for a proper review. (ESC)

Response: For population surveys in PWS, all island shoreline was surveyed along with 23 randomly selected mainland plots. All of Kachemak Bay and 14 random plots were counted on the Kenai Peninsula coast. Methods were those described in Hodges et al. (1984).

Comment: The inclusion of areas well outside the spill area, such as Malaspina Glacier, is questionable. Acquisition of such data is more in the nature of research than damage assessment. (ESC)

Response: As stated previously, it is unclear how large an area was impacted by the spill as eagles may range widely. Therefore, it is necessary to sample areas with historical data distant from the immediate spill area.

Comment: Comparison of eagle productivity in widely separated areas such as PWS and Southeast Alaska is invalid. (ESC)

Response: Habitat and environmental conditions in southeast Alaska are similar to PWS, and long-term research in southeast provides a useful body of information for comparison. In addition, effects of the spill in areas adjacent to PWS were unknown, and southeast Alaska provided a safe control for some aspects of this study.

Comment: The application of "home range" implies two assumptions that may not be correct: one is that the level of use of the shoreline is constant throughout the home range; the second is that eagles lack the ability to avoid oil. (ESC)

Response: We defined a biologically meaningful "core use area" around active nests as the average length of shoreline in front of each nest used by the resident radio tagged eagles during the nesting season. Telemetry indicated that eagles with oiled shoreline in their core use areas continued to utilize this same area. Relative use of oiled and clean areas within a core use area is not possible to determine. Assuming acquisition of food is

the primary motivation for coming into contact with oiled beaches, eagles are not likely to differentiate between clean and oiled beaches or prey.

Comment: The comparison of survival between 15 adult eagles from oiled and 15 adult eagles from unoiled areas employs too small a sample to ensure that random samples across the age structure of the population are obtained. (ESC)

Response: Adult eagles are of indeterminate age and nothing is known about the age structure in adult bald eagle populations. With no knowledge of age structure in the population, it cannot be stated that the sample is too small. Individuals in a given age class have an equal probability of being captured, while younger year classes have a higher probability of being sampled given consistent annual mortality. Age structure is unlikely to be different in eagles trapped in east vs. west PWS.

Comment: The radio-tagging program does not account for the natural dispersal of immature eagles and could potentially increase the risk of mortality to fledglings, thereby creating a bias in the study. Also, there is no explanation of how the failure of radio tags will be taken into account. (ESC)

Response: It is undoubtedly true that the dispersal of juvenile eagles increases their risk of mortality compared to adults, but the risk is not associated with the natal area in normal situations. We are comparing survival of juveniles from oiled and unoiled areas that are subject to the same risks with the exception of the pollutants in the oiled area. Radio failures are equally likely for either group. Literature on the Kaplan-Meier procedure cited in the study plan (Pollack, 1989) discusses how missing transmitters are "taken into account".

Comment: The oiled and non-oiled sampling areas for the survival study are not described adequately to allow proper review. (ESC)

Response: Sampling areas are based on the occurrence of successful nests and where adults are found. Obviously, these cannot be determined until they are observed in the field. Of the 71 chicks radioed, 41 were from areas potentially impacted by oil (west PWS) and 30 were from known clean areas (east PWS). 37 adults were captured in west PWS while 29 were captured in east PWS. Specific oiling status is determined as described above. Survival estimates will be calculated on macro (east vs. west PWS) and micro (oil in vicinity vs. no oil in vicinity) levels.

Comment: Neither the number of blood samples nor the means of selection of the individual eagles to be tested was provided. (ESC)

Response: The number of blood samples taken was equal to the number of adult eagles trapped and from which a sample of blood was obtained. Fifteen individuals were selected from dispersed areas in west and east PWS. We trapped eagles in west PWS from areas with oiled shorelines. Subsequent radio telemetry of trapped and released eagles further defined potential exposure to oiling.

Comment: The post-mortem changes evident in dead eagles may invalidate the results of hydrocarbon analyses performed on recovered carcasses. (ESC)

Response: Post-mortem changes in eagles from oiled and unoiled areas are unlikely to be different. Determination of causes of mortality are being made by highly qualified professionals who are eminently qualified to determine whether the analyses performed provide reliable data.

Comment: The oiled and control sites for the toxic/sublethal effects portion of this study are not described sufficiently to permit review. (ESC)

Response: See response to similar comments above vis-a-vis the population surveys and survival study components of this study.

Comment: Statistical models are too vaguely defined. Study sites are neither disclosed nor described adequately, and the probabilities of Types I and II errors are not given. (ESC)

Response: Statistical models mentioned in the plan are routine tests presented in statistical texts. Study sites are discussed above. It is accepted statistical practice to set the Type I error at 5%, termed "significant", or at 1%, termed "highly significant". Type II error will be evaluated in consultation with statisticians.

Bird Study No. 11 - Sea Ducks

Comment: Better explanation is needed of the correlation between hydrocarbon intake and increased mortality and reproductive failure. There will be no real measurable data on effects without this. Objective A seems unattainable given the scope and design of the program. (API, ESC)

Response: Increased information on the correlation between hydrocarbon intake and increased mortality and reproductive failure is needed and will be gathered in 1991. Objective A is attainable

and is well underway. Food items from collected ducks have all been identified and the information entered into database files awaiting statistical analyses.

Comment: The statement in the Plan that seaduck collection will be integrated with other data to demonstrate that seaducks feed on contaminated prey reveals a bias in the study. (ESC)

Response: Seaduck collection was integrated with sample sites of blue mussels in exposed and unexposed areas of PWS to test the hypothesis that seaducks are exposed to petroleum contamination through their prey base.

Comment: Given the number of ducks to be collected in this study in 1990 and the fact that waterfowl hunting is still permitted, seaduck populations must be healthy. Thus, there is a question about the cost-effectiveness and reasonableness of this study. (ESC)

Response: The presence of a huntable surplus of ducks does not mean that significant mortality did not occur and, therefore, that injury from EVOS did not occur.

Comment: The study and control sites within PWS are not defined, the methodology used in selecting the individual seaducks to be collected at each site is not described, and the number of samples to be collected at the control sites is not given. The relatively small sample sizes will not justify the planned statistical work. (ESC)

Response: The exposed study site is the oil spill area of western PWS. The PWS control site is a series of bays and inlets north of Cordova in the unoiled area of the eastern Sound. Ten individual seaducks of each species per site is considered the optimal sample size. It is adequate to obtain information needed but will not exert undue population pressure. Statistical interpretation of small sample sizes has made considerable progress in recent years.

Comment: The use of a control site in Southeast Alaska is inappropriate since that area is not representative of the spill zone. (ESC)

Response: Use of a control site in southeastern Alaska is appropriate because intertidal habitats, where these ducks forage, are similar to PWS. Intervening sites along the North Gulf Coast

(Icy Bay, Yakutat Bay) had naturally occurring oil seeps and were deleted from consideration.

Comment: The predictive models for estimating the effect of oil on morbidity, mortality, and reproductive potential are not described. It will be subject to a large degree of uncertainty on account of the ranges of reasonable variables used for input. The use of a model in this fashion represents use of a non-standard technique for injury determination that is not widely accepted. This violates the NRDA regulations. (ESC)

Response: The study has evolved away from the predictive model postulated at early stages of the investigation into actual documentation of morbidity, population decline, and reproductive failure.

Comment: Integration of data from other studies likely will not be possible due to the high degree of spatial variation present even on a small scale. (ESC)

Response: Integration of data from a study of petroleum hydrocarbon levels in blue mussels and other intertidal forage species is possible and is proceeding.

FISH/SHELLFISH

Comments on Fish/Shellfish Studies - General

Comment: Fish/Shellfish studies 2, 5, 7, 8, 10, 17, 23, 27, and 28 will provide data that may be useful for long-term goals of managing Prince William Sound, but are not directly related to oil spill impacts. (ESC)

Response: These studies have components that will demonstrate exposure of fish and shellfish to oil (observations of oiling), bioavailability (hydrocarbon and mixed function oxidase analysis), or injury to individual organisms (histopathology). Taken together they are structured to determine damage to fish stocks. Damage due to the oil spill may require management actions that would otherwise not have been necessary and ancillary benefits improving the management of stocks in general may result from these studies as there are ancillary benefits for many kinds of scientific studies; however that is not the primary reason for conducting these studies.

Comment: Despite more complete descriptions for the 1990 studies, there are still problems with the changes these studies seek to measure. (API) Details are inadequate to support a comprehensive review of study design, field methods, or interpretation of results. (ESC)

Response: It is impossible to respond to unidentified problems or inadequacies. Each study is followed by a reference section of literature in the public domain from which the study designs, methods, and likely avenues of interpretation were derived.

Comment: Results of the 1989-90 subsistence program conducted by NOAA, ADF&G, and Exxon provide evidence that fish from the spill-impacted area do not contain hydrocarbons above background levels. (ESC)

Response: This statement is not entirely consistent with the results obtained to date. The subsistence tests focussed on hydrocarbons in edible fish tissues; however, fish (unlike clams) have physiological pathways by which they process hydrocarbons into forms soon unidentifiable as having originated from the EVOS (the fish themselves are largely composed of hydrocarbons). This does not mean that the fish have not been harmed by exposure to oil in either a short or a long term fashion nor that this harm cannot be assayed by means other than hydrocarbon analysis.

Comment: Except for those shellfish collected from the few obviously oiled areas, there are no problems. Even those few present extremely low risk for consumption. (ESC)

Response: It is unclear whether the first statement applies also to the general health of the shellfish or only to the question of human consumption. Shellfish containing hydrocarbon levels that would pose a risk if consumed were identified in certain areas. The NRDA shellfish studies are designed to assess impacts of the EVOS on the mortality rates and general health of certain shellfish. It is well documented that the *Amoco Cadiz* oil spill had a severe impact on shellfish in Brittany, France, and similar impacts could be expected here.

Comment: It is not apparent how the results of the fish/shellfish studies will provide data useful for the restoration effort. The lack of significant injury as shown by the record fishing season and the population management focus of many of the studies will not guide a reasonable restoration strategy if restoration is warranted. (ESC)

Response: It is impossible to restore anything until it is understood what has been lost. The fish/shellfish damage assessment studies are designed to identify injuries to these resources and provide the basis for restoration actions. The record fishing season was largely a function of good hatchery management and does not necessarily reflect returns to any particular oil-impacted stock, particularly wild stocks. Complex population management of the fisheries was in place prior to the oil spill, and management changes needed for restoration as revealed by these studies will have a very solid historical basis on which to build.

Comment: No information is provided describing the reasons certain studies were discontinued. Without access to the data generated during 1989, it is impossible to determine whether or not these decisions were justified. (UM)

Response: Some studies were completed and some did not show sufficient likelihood of demonstrating injury to justify continued investigation.

Comment: Other than analysis of gut contents, almost none of these studies (the rockfish studies being a notable exception) address key predator/prey interactions, and many do not even assess reproductive status of the adults. (UM)

Response: Fish/Shellfish studies being conducted were designed to get the most information for determining injury with the available funding. Fish/Shellfish 4 addresses predator/prey relationships between juvenile salmon and harpacticoid copepods. Other studies address broader predator/prey relationships. Histopathology in

some studies will touch upon reproductive status of the adults even though this is not the primary focus of the study. Observed recruitment will define ultimate reproductive effects in some studies.

Comment: Many of the studies have inappropriate reference lists. Some studies list almost no references, some have reasonable lists, and some list large numbers of reports, many only tangentially related to the particular study. (UM)

Response: Some of the areas being investigated received considerable attention before the oil spill and some were essentially new ground. Thus, there were many references appropriate for some studies and few for others. In accordance with accepted scientific publication practice, a reference section is only a list of all of the authors and their associated papers mentioned in the preceding text. Those must be listed if mentioned in the text whether the paper has a great deal of significance to the study or not.

Comment: There is convincing evidence that fishery resources are vital and productive: no fish kills were reported in 1989 and commercial herring catch rates and pink salmon harvest reached record levels in PWS in 1990. This evidence should be taken into account in the Plan. (ESC, API)

Response: Though dead adult rockfish were reported in 1989, many fish suffering oil-induced acute mortality, particularly small fish, would probably sink or be consumed by predators. Ultimate injury to the herring population will not be evident until the 1989 year class becomes susceptible to fishing or enters the spawning population in 1992. As noted above, the record pink salmon harvest was largely a hatchery phenomenon and was not reflected in wild stock returns in 1990.

Comment: Many fish and shellfish studies are too general for damage assessment purposes. (API)

Response: The Trustees disagree. The studies have been carefully peer reviewed and tailored to address injury assessment.

Comment: Studies are uncoordinated and perhaps repetitive. (API)

Response: The Trustees disagree. Every effort has been made to assure that the studies are complementary; the studies have undergone extensive peer review and synthesis processes.

Comment: The tagging of juvenile fish will cause stress that will result in physiological changes and increased mortality. The invasive techniques will create injury to the resources. (API)

Response: The tagging process has been exhaustively studied and reported in the fisheries literature for many years. Coded wire tagging possibly has the least effect on fish of any method employed. CWT associated mortalities are extremely small. This technique is so well understood and successful that many millions of salmon and steelhead are tagged by this method every year along the Pacific coast of North America. Nevertheless, the methods for all of the studies employing tagging include techniques for estimating tagging associated mortalities. Impact on the resource is negligible.

General - Sublethal and Chronic Effects

Comment: Expensive studies focusing on sublethal and chronic effects are problematic. Results found may not be the result of oil, and may be due to natural causes or the capture and handling of the fish. (API)

Response: Many of the individual assays may not demonstrate changes pathognomonic for *Exxon Valdez* oil. However, the combination of the results from these several types of assays may lead to the conclusion that the injuries observed were due to the EVOS. Changes due to capture and handling are distinguishable from oil-induced changes.

Comment: Many of the 1990 studies rely on non-specific or non-standard indicators to correlate evidence of hydrocarbon exposure to presume population impacts, which will not bear technically conclusive results. Examples of this include: biochemical measurements of bile fluorescent aromatic hydrocarbon concentrations and enzyme level changes in fish, which are highly variable in nature (Fish/Shellfish 18 and 24). (ESC)

Response: Assays of this nature employ appropriate and adequate controls in order to produce meaningful results. The Trustees believe that the results of the assays will be conclusive.

Comment: The Plan contains studies designed to do scientific research that will not lead to identification of injuries. The costs of such research should be funded by alternate means since they cannot be recovered under the NRDA regulations. Examples of this include: the use of mixed function oxidase levels in fish tissues to assess hydrocarbon exposure - this is an unproven technique that has yielded greatly varying results between

different life cycles, seasonal factors, and food sources; and Fish/Shellfish 13's use of mussel tissue to assess hydrocarbon contamination - in particular, to determine hydrocarbon concentrations, pathways, and effects. (ESC)

Response: The Trustees disagree with the first statement, particularly in reference to the examples cited. MFO and hydrocarbon analyses are recognized scientific techniques for demonstrating hydrocarbon contamination. When combined with other analyses in the studies these produce reliable evidence of injury. It is well established in the toxicological literature that MFO's are produced as a response to hydrocarbon exposure. MFO's, though a defensive mechanism, often convert these hydrocarbons into more toxic substances than the original compound and create ultimate carcinogens. Thus, these hydrocarbons can have acute toxic effects and long term mutagenic effects. Assaying for MFO's and hydrocarbons is clearly appropriate for identification of injuries.

General - Oiling Levels

Comment: The Fish/shellfish studies contain a fundamental flaw: their designs are based on detecting differences between oiled and non-oiled areas that cannot necessarily be attributed to the oil spill. Many of the study designs have statistical problems in identifying the effect of oiling, physical location, and timing. Too little consideration has been given in these studies to distinguishing effects of the spill from natural factors that can influence population sizes, productivity, or physiology. (ESC, API)

Response: Statisticians have reviewed the studies and have determined that the methods will take into account variables due to natural variation and other factors.

Comment: It is unlikely that the sampling design of the Fish/Shellfish 1, 3, 8, 18, 22, and 28 will be able to relate observed biological responses to any particular hydrocarbon concentrations since the areas to be sampled represent a wide range of hydrocarbon exposures levels. (ESC)

Response: These studies are not intended to be carefully controlled laboratory experiments. They are not bioassays to estimate a given concentration of oil in water. There are other tests by which we are attempting to do that. The Fish/Shellfish studies were designed to determine the injury done to fish as a result of the EVOS.

Comment: Many studies are based on the development of data from

oiled and control sites. Inadequate information is provided to document that control sites are ecologically similar to test sites to establish baseline information. (ESC)

Response: The control sites were carefully selected in order to accurately determine the differences between oiled and unoiled sties. Their use is not intended to provide pre-spill baseline information.

Comment: The criteria used to select stream and sampling sites for the Fish/Shellfish studies do not include the evaluation of the level of oiling. Some quantitative method of assessing the degree of oiling should be included in the site selection criteria since these studies are designed to evaluate the effects of oil on egg-to-fry survival. (ESC, API)

Response: Quantitative assessment of oiling was not possible when most sample sites were selected. Criteria were usually based on visual observations followed by quantitative assessment of the levels of oiling. In some of the streams involved in the egg to fry survival studies, oil has penetrated deep into the substrate. In some cases, fry incubating in the gravel may be adversely affected by chronic exposure to low levels of oil leaching out of the gravel.

Comment: A large number of salmon spawning areas have been retained for additional study, yet evaluation of the level of hydrocarbon contamination is limited to the visual presence of oil and the hydrocarbon content of bivalves at the mouth of these streams and rivers. (UM)

Response: As noted above, great care is being taken to accurately quantify levels of hydrocarbon contamination. Whenever possible, several means of estimating the presence of oil are compared. Bivalves are good indicator organisms because, unlike fish, they do not possess an MFO-like system for ridding themselves of hydrocarbons. Thus the presence of hydrocarbons in bivalves provides an additional indicator on the level of oiling to which other organisms were exposed.

General - Variable Methodology

Comment: The level of detail and justification provided in the Fish/Shellfish studies is extremely variable. Frequently great detail is given on the methods employed without any discussion as to the significance of the measurements to be made. (UM)

Response: Specific questions about the methods or the significance of the measurements should be directed to a particular study.

Comment: The methodologies used in the various studies seem highly variable. Analysis of parent hydrocarbons in tissues of organisms capable of rapid hydrocarbon metabolism is of limited value. Most fish and many crustacean species at developmental stages from larvae through adults fall into this group. (UM)

Response: Hydrocarbon analysis of organisms that can quickly metabolize them is of limited utility unless samples are collected very soon after exposure or there is a potential for continued exposure. Where neither of these is the case, other methods for establishing injury from exposure to oil are employed. The variety of methods reflects these considerations, among others.

Comment: Several methods are available to assess metabolite body burdens and effects. In some studies these are mentioned but in others they are not, and there is no explanation given for this apparent inconsistency. (UM)

Response: See response to comment above.

Comment: Different methods are being used to assess a biochemical measure of hydrocarbon exposure (induction of cytochrome P4501a1). In some cases analyses will be done on formalin fixed samples, in others on subcellular fractions of fresh tissue. No information is given that would allow an evaluation of whether theses methods will produce comparable results. (UM)

Response: The appropriate controls should allow for comparable results. Because the method of sample preservation limits the kinds of assays performed, the particular method chosen for each study that is assaying cytochrome P450 is somewhat dependent upon the other types of assays that will also be performed on the same samples.

Comment: Testing procedures are inconsistent. Mixed, fixed, and nested effects models are planned for data from the same field sampling protocols. In most cases the appropriate testing model was not determined before taking samples. (ESC)

Response: While general comments like this are difficult to address, the Trustees do not believe that testing procedures are incorrect nor do they need to be consistent from one study to another if the goals for each study or analysis are different.

Comment: Error terms for testing are infrequently documented. Often the proposed error term is incorrect. (ESC)

Response: Without specific examples, this comment cannot be addressed adequately.

Comment: Procedures for estimating the total effect of oil over the area impacted are not described. Estimates are likely to be biased and highly variable. (ESC)

Response: To the extent that this is possible, the combined results of all NRDA projects, not just the Fish/Shellfish studies, will achieve this. Because a variety of life forms over an immense area were affected, only an approximation will be possible. Every effort has been made to provide consistency among the NRDA studies.

Comment: Clark and Bernards' procedures, as planned for the tagging studies, are inappropriate and will reject the hypothesis of no effect too often. This is true for Fish/Shellfish studies 1, 2, 4, 5, and 11. (ESC)

Response: Clark and Bernards' procedures are not planned for any of the studies indicated. Where these procedures are being used, we believe they are appropriately applied.

General - Natural Variability

Comment: The Plan does not make clear that sampling programs will produce information necessary to prove that a statistically significant portion of the expected biological variability is a function of hydrocarbon contamination as opposed to other natural factors. Fish/Shellfish 1, 3, 8, 13, and 17 suffer from this defect. (ESC)

Response: Appropriate controls and continued sampling in post-spill years will address this concern. However, the premise is not that the observed biological variability is a function of hydrocarbon contamination in each case; rather, if biological variability is a function of hydrocarbon contamination, our methods will allow us to distinguish this from variability caused by natural factors. The studies are observational rather than experimental.

Comment: Most of the Fish/Shellfish studies do not adequately account for the fact that there was substantial variability in resource levels before the spill or the fact that there is no reliable baseline data. Thus, the statistical detection of

differences due to oiling will not be possible. This is particularly true of Fish/Shellfish 3, 8, 15, and 17. (ESC, API)

Response: For any study in which substantial random variability is expected, an increase in the number of sample sites and sizes improves the statistical power of the test. For those studies in which variability is expected to be large, statisticians have determined the number of sites and the number of samples necessary to demonstrate oil-related variability. Some of the observed pre-spill variability for which there is baseline data was harmonic area-wide. An absence of harmonic variation would point to EVOS effects. A return to harmonic variation would be expected as the area recovers.

Comment: It will be impossible to develop sufficient data to describe the subtleties of historical population dynamics or to relate any potential response to extremely low hydrocarbon levels, as these factors are not well understood by fisheries managers. This is true in Fish/Shellfish 3, 4, 5, 13, 17, and 27. (ESC, API)

Response: Some of the populations being examined lend themselves very well to the study of subtleties of historical dynamics. There will be some level of hydrocarbon exposure that will not produce observable effects. Examining fish with low levels of exposure as well as those fish with hydrocarbon levels that produce observable effects will help estimate the total injury to fish and shellfish as a result of the EVOS.

Comments on Fish/Shellfish Studies - Specific

Salmon Studies

Comment: The 1990 adult pink salmon catch consisted of fish present in PWS as sensitive juveniles in April 1989. The record catch in 1990 provides convincing information regarding the lack of injury to this population. The need for extensive study of potential oil impacts has been obviated. (ESC)

Response: The record catch was largely a hatchery phenomenon not necessarily paralleled by wild returns. Among other factors, hatcheries' net pens were shielded from the oil by booms or were outside the spill area and their fish were able to spend a month or longer in this protected saltwater environment before they were released. Wild fish did not have those options. PWS pink salmon are not a single population and, therefore, hatchery successes and failures are not the only criteria by which to determine the effect of the EVOS.

Fish/Shellfish Study No. 1 - Salmon Spawning

Comment: It is unclear whether studies are presuming organisms were exposed to oil in areas where oil was only visible on the water surface. DOI regulations require confirmation of exposure. It is not clear how exposure pathways will be verified. (API)

Response: Fish/Shellfish 1 collects mussels from the immediate vicinity of each stream in studies 1 and 2. These mussels will be analyzed for hydrocarbon uptake (bioaccumulation) to indicate oil impact. Adults will be collected from 22 streams (12 oiled, 10 not oiled) and analyzed for histopathological abnormalities as well as mixed-function oxidases (MFO's). Eggs and fry will be collected during study 2 and also examined for hydrocarbons, histopathological abnormalities, and MFO's.

Comment: Studies do not address issues such as error margins in egg counts and the impact of sampling frequency on fish migration. (API)

Response: All eggs and fry are individually enumerated not estimated; consequently, if an egg or fry is removed from the gravel, it is counted. The counting of adults during study 1 has very little or no impact on spawning. Egg and fry sampling takes place in mid-September to mid-October and again during mid-March to mid-April. This is after the fall spawning migration and before the fry emerge from the gravel to begin their spring migration.

Comment: The methodologies used for objective A, visual observation, aerial photography, and hydrocarbon analysis of tissue samples from intertidal mussels at stream mouths, are inappropriate for determining hydrocarbon concentrations, pathways, or their effects. (ESC)

Response: Objective A reads: "To determine the presence or absence of oil in the intertidal habitat used for salmon spawning." The collection of photos, visual observation data, and mussel hydrocarbon data will show presence or absence of oil.

An objective was added in the 1991 studies which reads: "Document the presence or absence of hydrocarbons from the EVOS in the tissues of adult salmon." This objective will be met by the collection of tissues from spawning adults in 12 oiled streams and 10 unoiled streams. These tissues will be analyzed for histopathological abnormalities and MFO's.

Comment: For objective B, documenting the physical extent of oil distribution on intertidal spawning areas is insufficient to

determine hydrocarbon concentrations, exposure pathways, or their effects. (ESC)

Response: See response to comment above.

Comment: Regarding objective D, no selection criteria ensuring that streams selected will be representative of PWS streams are presented. (ESC)

Response: Four streams were weired in 1990. One on the east side of PWS and the other three in southwest PWS (2 unoiled, 1 oiled). Stream selection was made by biologists familiar with the area and based on subjective criteria.

Comment: In objective E, the correction factor for one stream does not include the variables which would allow that correction to be applied to 138 or 218 other streams. (ESC)

Response: Objective E estimates bias between aerial and ground surveys for 138 streams and estimates accuracy of the 2 methods for 4 weired streams. A correction factor can be estimated for the weired streams.

Comment: Objective F will likely be compromised through biased criteria used in determining in-stream residence time of young salmon. (ESC)

Response: Average stream life in this case is the number of days that adult pink or chum salmon spend in the stream from the time they enter the stream during their spawning migration until they die.

Comment: Objective J, the recalculation of historical escapement from 1961 to 1988, is of little relevance to impact assessment for a 1989 spill. The assumption that survey and environmental parameter estimates based on the conditions of the past 2 years can be applied as a correction to the past 30 years is invalid. (ESC)

Response: This comment apparently addresses objective G. Historically, escapement estimations in PWS have been more of an index program than an actual escapement estimate. Basically the same survey methodology using only 3 or 4 different observers has been used to build the historic database. Once a satisfactory methodology has been obtained for all 138 streams, estimates of historical escapements will be made. Knowledge of salmon total run (catch + escapement) is essential if a loss is detected during any of the life stages (studies 1, 2, 3, 4, and 28).

Comment: For objective H, no attempt is made to identify or measure other variables that may affect available spawning habitat. (ESC)

Response: The incorporation of instream flow models to obtain refined spawning area estimates for 138 streams would be too costly (both in terms of manpower and money). The stream area provides an accurate estimate of relative stream size.

Comment: Regarding objective I, a catalog of aerial photographs and detailed maps of spawner distribution is not necessary for use in designing sampling transects. (ESC)

Response: The catalog makes it more efficient to locate and plan the egg/fry sampling for study 2 and provides reproducible maps for stream surveyors to use in recording counts by stream zone. This data will be useful for detecting a change in spawner distribution (such as a decrease in intertidal spawners).

Comment: The relationship between hydrocarbon data from mussels taken from the intertidal area and salmon exposures in the stream is questionable. (ESC)

Response: Up to 75% of the PWS pink salmon spawn intertidally. The mussel data will indicate if the intertidal areas of the stream were impacted by oil.

Comment: The study plan does not identify the selection criteria or a plan for developing criteria to select the appropriate technique for evaluation of stream life. (ESC)

Response: The stream life estimates will be tested against what is observed at the weirs. Weirs provide known escapements based upon daily stream walks to provide numbers of live and dead fish.

Comment: The number of fish tagged weekly (80) to determine stream life is static and may be inappropriate for streams where the weekly escapement can vary by several thousand fish. (ESC)

Response: It is assumed that fish are randomly tagged (each fish present has an equal chance of being tagged). The number of fish tagged needs is based upon the need to have sufficient recoveries to provide a meaningful estimate. It was determined that 30-40 recoveries would provide such an estimate.

Comment: The visual observations used to define the levels of hydrocarbon contamination and to categorize stream zones is an inappropriate methodology. (ESC)

Response: Visual observations only provide the presence or absence of oil. Levels are determined by analysis of mussels (this study) and sediments (other studies).

Comment: The criteria used for separation of streams based on their exposure to oil is unclear. In one section the plan says it will be based on visual inspection, in another on levels of hydrocarbons in mussel tissue sampled near each stream. Both methods have weaknesses that will affect the basic categorization of streams. This categorization is the basis upon which the data will be evaluated. (ESC)

Response: Data from both methods as well as other sources will be used for the final oiling categorization of streams.

Comment: Statistical procedures are vaguely defined. It is unclear how the effects of EVOS are to be estimated and tested. The sampling effort may not be appropriate to meet objectives. (ESC)

Response: Study 1 provides stream oiling information, effects of oiling on adults that incubated in oiled substrate, and an estimate of adult escapement. These data will be used by the other salmon projects (Fish/Shellfish 2, 3, 4, & 28) to determine injury to the population. Type 1 and 2 power calculations will be made as the studies progress.

Comment: The probabilities of type 1 and type 2 errors, finding an effect when there is none and failing to find an effect when there is one, are not given. (ESC)

Response: Study 1 provides stream oiling information, effects of oiling on adults that incubated in oiled substrate, and an estimation of adult escapement. This information will be used by the other Fish/Shellfish projects dealing with pink and chum salmon.

Comment: Criteria for selecting treatment and control sites are not given. (ESC)

Response: This study is not designed as a traditional treatment-control experiment. The oiling data will go into the determination of "treatments" and "controls" for study 2.

Comment: The type of data resulting from this study has large variances among sites and times. It will be difficult to determine if a statistically significant effect is due to EVOS or natural variations. The study methods and analytical approach do not address or control for these potential problems. It is not clear that the sampling program will provide the information to prove that a significant portion of variability in escapement is from oil contamination. (ESC)

Response: This study is not intended to show statistically significant impacts due to oiling. It is a source of supporting data for studies 2, 3, 4, & 28. The combination of these studies will determine the impacts.

Comment: The number of streams used in this study (138) seems quite high considering that only 41 appear to be in the affected area. A reasonable evaluation of potential damage to spawning areas could have been done on a much smaller sample size with good statistical sampling design at a lower cost. (ESC)

Response: This study was designed to estimate escapement throughout PWS. Sound wide estimates are required in conjunction with coded-wire tag results (Fish/Shellfish 3) to obtain wild catch contribution for use in run modeling (Fish/Shellfish 28) to estimate potential numbers of fish impacted for the 41 streams.

Fish/Shellfish Study No. 2 - Egg/Fry

Comment: It is unclear whether studies are presuming organisms were exposed to oil in areas where oil was only visible on the water surface. DOI regulations require confirmation of exposure, and it is not clear how exposure pathways will be verified. (API)

Response: Samples of eggs and fry will be collected from each stream zone for each stream. These samples will be examined for histopathological aberrations as well as mixed-function oxidases (MFO's).

Comment: Studies do not address issues such as error margins in egg counts and the impact of sampling frequency on fish migration. (API)

Response: All eggs and fry are individually enumerated, not estimated; consequently, if an egg or fry is removed from the gravel, it is counted. The counting of adults during study 1 has very little to no impact on spawning. Egg and fry sampling takes place in mid-September to mid-October and again during mid-March to

mid-April. This is after the fall spawning migration and before the fry emerge from the gravel and begin their spring migration.

Comment: For objective 1, no effort is made to identify the level of accuracy expected from density estimates, or to determine if the damage resulting from sampling is greater than that attributable to EVOS. (ESC)

Response: The level of sampling for density is a tradeoff between what is required to achieve a specified level of precision, what is practical with regards to time spent sampling, and loss of eggs and fry from the salmon population. The Trustees believe that this study provides a proper balance of these considerations.

Comment: For objective 2, no effort is made to identify the level of error in mortality estimates, or to identify factors other than oil that may lead to over-winter mortality of eggs. (ESC)

Response: This study is a "control-treatment" experiment. Error rates were estimated using data collected during the mid 1970's. This information was used to estimate the number of streams required for the experiment. Because of the "control-treatment" design, sources of mortality other than oil should be common to both groups; consequently, an increase or decrease in mortality can be linked to oiling.

Comment: Objective 3 reflects an assumed increase in over-winter mortality in oiled streams. The significance of this cannot be determined as no methods are given for estimating adult returns. (ESC)

Response: Adult returns will be estimated for studies 1, 3, and 28.

Comment: Regarding objective 4, the use of mixed function oxidase levels in eggs and alevins as a means of assessing hydrocarbon contamination is an unproven technique and clearly research. The MFO technique is greatly variable with different life stages, seasonal factors and food sources. The use of mussel tissue to assess hydrocarbon contamination is not appropriate for determining hydrocarbon concentrations, exposure pathways, or their effects on the salmon species being studies. (ESC)

Response: Peer reviewers and experts in biochemistry, toxicology, and pathology have recommended proceeding with the MFO analysis as well as examination for histopathological abnormalities.

Comment: There are no controls for the comparison of alevin samples for tissue analysis. The method of collection does not preclude contamination, so no accurate control values can be expected. (ESC)

Response: Alevins collected for hydrocarbon analysis were collected using a rake and strainer pre-rinsed in dimethylchloride. The fry were stored in pre-rinsed glass jars. Alevins collected for MFO and histopathological analysis were not affected by the sampling methods.

Comment: Although a sample of mussels near the stream bed will be used to determine the amount of hydrocarbon impacting the stream, there is no attempt to test the assumption that hydrocarbon levels in those mussels are representative of fish exposures to hydrocarbons in the stream bed. This methodology is inappropriate to measure hydrocarbon contamination and undermines the basis upon which the data are being evaluated. (ESC)

Response: Levels of hydrocarbon in mussels near the stream bed are indicators of impact upon the spawning gravel and the eggs and fry in the gravel. Eggs and fry are collected for further analysis for MFO and histopathological abnormalities.

Comment: Visual assessment to determine degree of oiling is inappropriate to define levels of hydrocarbon contamination for impact assessment. (ESC)

Response: Visual assessment provides a starting point for categorization. This information is refined with mussel hydrocarbon data, fry hydrocarbon data, egg and fry MFO data, and histopathological data.

Comment: Statistical procedures are vaguely defined. It is unclear how the effects of EVOS are to be estimated and tested. This study does not identify the effect due to EVOS and the effort needed to detect that effect. The sampling may not be appropriate to meet objectives. (ESC)

Response: A nested analysis of variance will be used to estimate effects on egg and fry mortality as well as over-winter survival. Power of the study was estimated using similar data collected during the mid-1970's.

Comment: The methodology for injury determination is not clearly stated. (ESC)

Response: See response to comment above.

Comment: MFO analysis of eggs and fry is an experimental methodology and is inappropriate for injury assessment. (ESC)

Response: It is well established in the toxicological literature that MFO's are produced as a response to hydrocarbon exposure. MFO's often convert these hydrocarbons into more toxic substances than the original compound and create carcinogens. Thus, these hydrocarbons can have acute toxic effects and long-term mutagenic effects. Assaying for MFO's and hydrocarbons is clearly involved with identification of injuries. Peer reviewers and experts in biochemistry, toxicology, and pathology have recommended proceeding with the MFO analysis as well as examination for histopathological abnormalities.

Comment: There is no evidence that sufficient parameters are being considered by which to identify major aspects of variability in egg to fry mortality. It is not clear that the study will obtain information that will enable an accurate assessment of oil effects versus other environmental factors. (ESC)

Response: Since the oil spill was not anticipated, few pre-spill samples existed. The study will sample both oiled and unoiled areas. As differences between these areas begin to disappear as the environment recovers, it will become more apparent that the initial differences were due to oiling rather than other "natural factors." In the absence of known parameters that identify variability, our statisticians have identified those parameters from the literature and from their experience that most likely indicate variability in egg and fry mortality versus other environmental factors.

Comment: Generalization of results from this study to all 900 anadromous streams in PWS is inappropriate as the streams used in this study are the better, more consistent salmon producing streams. (ESC)

Response: Not all of the streams selected for this study are the better, more consistent salmon producers. Selection criteria included "sufficiently large adult salmon returns to indicate a high probability of success in egg/fry digging." F/S-1 allows us to estimate the stream life for streams that might otherwise fall below the level of practical egg/fry sampling. Using streams ranging from the high end producers to the very low end will allow extrapolation of results to the remainder of the streams.

Fish/Shellfish Study No. 3 - Wire Tagging

Comment: It is unclear whether studies are presuming organisms were exposed to oil in areas where oil was only visible on the water surface. DOI regulations require confirmation of exposure. It is not clear how exposure pathways will be verified. (API)

Response: Fish Studies 1 through 4 provide a comprehensive approach to assessing the impact of oil exposure on pink salmon. Samples of pink salmon eggs, fry, and juveniles have been collected for confirmation of oil exposure using the best available methods. The presence of tagged fish in oiled areas will allow stock specific confirmation of oil exposure for fish from both oiled and unoiled areas. Extensive collections of mussel samples from the intertidal zones of spawning streams will provide confirmation of the presence of oil at specific sites.

Comment: The study does not address issues such as error margins in egg counts and the impact of sampling frequency on fish migration. (API)

Response: Egg counts are used by hatchery operators to properly load incubators with salmon eggs. Outmigrations of fry from the incubators are monitored with electronic fry counters. Fish/Shellfish Study 3 uses fry outmigration counts to determine release numbers and the proportion of tagged fish in the release.

Comment: It is unclear how objective 1 will be used to evaluate effects of the spill on hatchery-released salmon. The data will be of use to hatchery managers, but they will not be valid for injury assessment. The Plan's statement that "outmigrating smolt and returning adults from these hatcheries are exposed to oil at varying degrees" is a nebulous tie to oil effects. (ESC)

Response: Determining the catch and survival rates for wild salmon and salmon released from five hatcheries in PWS will allow investigators to estimate differential survival based on their exposure to contaminated waters. A history of exposure to oil can be confirmed with results from Fish Studies 2 and 4 and used to tie in the effects of oil to losses in production.

Comment: Using the tag results as indicated in objective 2 may obtain a rough estimate of the catch of wild stock pink salmon, but is unlikely to produce information on spill-related effects. (ESC)

Response: Adult returns to the pink salmon streams will be determined by operating weirs at each site rather than by using stream surveys. Knowing the catch and escapement, differential

survival of salmon between oiled and unoiled areas can be investigated. Returning tagged fish that stray to streams other than their natal streams may provide evidence of lost populations and habitat.

Comment: Field methods are not detailed enough in Objective 3 to evaluate the validity of this study. (ESC)

Response: Detailed methodologies for coded-wire tag application to wild and hatchery fish is readily available in the literature. Weir operations also have well established methodologies and standard operating procedures.

Comment: The results for Objective 5 will provide little insight into the effects of the spill on any detected differential in survival rates. There is no measurement of exposure to oil, nor is there adequate baseline data for historical comparison. (ESC)

Response: Objective 5 calls for the identification of relevant injuries for which methods of restoring lost use, populations, and habitat must be developed. This objective summarizes the long term goal of all damage assessment studies to identify injured populations. Identifying these populations can be accomplished by satisfying objectives 1 through 4 of this study. These results, along with those of other salmon studies, will identify where and how restoration of injured populations can best proceed. There are other NRDA studies addressing the oil exposure of salmon. These studies rely on coded-wire tagged fish to verify the origin of captured fish. There is a great deal of historical data relevant to the salmon populations of PWS as well as information on the effects of exposure to crude oil on salmon.

Comment: The tagging methods have little application for evaluating effects of the oil spill. (ESC)

Response: The presence of marked fish will allow investigators to confirm stock specific exposure to oil as well as to estimate catch and survival rates for returning adults. Tagging allows identification of fish that stray from their natal streams, which is important to document true returns to streams.

Comment: The methods used to capture sensitive wild stock pink salmon fry are not specified and could have significant effects on the success of the program. (ESC)

Response: The method used to capture wild pink fry in the intertidal zone employed a modified fyke, a common gear type used to collect juvenile salmon.

Comment: The study proposes tagging coho from Valdez, Esther, and Fort Richardson (for release at Whittier and Cordova) hatcheries, and chinook from Esther Hatchery. These releases are not in oil-affected areas and it will be difficult to link results to oil. (ESC)

Response: These releases will provide a control for releases in oiled areas.

Comment: The evaluation of tag return data uses standard analytical methods, but has little application for determining the effects of the spill. (ESC)

Response: The data obtained from tag returns will allow the catch and escapement of hatchery and wild fish to be determined. This information is needed to evaluate the potential numbers of salmon lost to oil contamination. The results obtained from meeting the objectives of Fish Studies, 1, 2, 3, and 4 all apply to the effects of oil exposure on the same specific stocks of fish.

Comment: Inter-annual and inter-facility survival variation for hatchery stocks has been so large that observed differences will be difficult to interpret. It is unlikely that those differences in survival could be linked to the spill and used for damage assessment, even though the observed survival fits a pattern based on the possibility of effects. (ESC)

Response: The use of multiple tag codes at each hatchery and wild stream allows for information from individual release groups to be compared between hatcheries or within the same hatchery. Observed survival differences can therefore be examined more closely than simply between years or facilities. The use of multiple tag codes verifies the presence of fish in contaminated waters during their early marine life and will also help identify injuries and further link the exposure history to losses in production.

Fish/Shellfish Study No. 4 - Early Marine Salmon

Comment: It is unclear whether studies are presuming organisms were exposed to oil in areas where oil was only visible on the water surface. DOI regulations require confirmation of exposure, and it is not clear how exposure pathways will be verified. (API)

Response: It is not assumed that juvenile salmon captured in oiled areas were exposed to oil. Mixed-function oxidase and hydrocarbon analyses are being performed to determine if organisms were exposed to oil. Experiments will be conducted by the NMFS in 1991 to obtain more information about pathways of exposure and effects on growth.

Comment: The definition of technical terms is inadequate. (API)

Response: This comment does not provide enough information for a response.

Comment: This study does not address issues such as error margins in egg counts and the impact of sampling frequency on fish migration. (API)

Response: Egg counts from the hatcheries are used to estimate the numbers of fry released. This issue is addressed in Fish/Shellfish 3. The applicability of a Maximum Likelihood Estimator of fish migrations will be determined. The effect of sampling frequency will be determined at that time.

Part I

Comment: For objective A, the study assumes all fry released together remain so and grow at a proportional rate. This may be biased if groups of fish from a release move to different habitats or grow at different rates. Nonrandom distribution of sizes along the migration corridor, which is likely, can cause bias. Sampling one segment in an oiled area, and one in a non-oiled area, could lead to incorrect conclusions on relative growth rates. (ESC)

Response: It is not assumed that fry released together remain so and grow at a proportional rate. Fry have been sampled at many sites in both oiled and unoiled areas to minimize any bias resulting from local effects. Analyses of stomach fullness will be used to assess differences in feeding rate in oiled and non-oiled areas.

Comment: Regarding objective B, the study is unlikely to give precise enough data to detect differences in migration speed and patterns caused by oil-related effects. (ESC)

Response: The applicability of a Maximum Likelihood Estimator of fish migrations will be determined. Questions relating to migration speeds and patterns will be addressed at that time.

Comment: No information is given on the method for determining hydrocarbon content in tagged fry captured in 1989. (ESC)

Response: Individual tagged fry were placed in clean glass vials immediately after capture. They remained in these vials until shipment for hydrocarbon analysis. Mass spectrophotometry is used to estimate hydrocarbon content. The ratio of the metals vanadium and nickel is used to identify oil from the *Exxon Valdez*.

Comment: The methods used to determine growth, migration paths, and migration speeds are imprecise and based on assumptions regarding behavior and swimming speed that are likely to be invalid. The variability of these estimates will be too great to determine changes caused by EVOS. (ESC)

Response: More precise estimates of fry growth will be obtained from otolith microstructure analysis. The applicability of a Maximum Likelihood Estimator of fish migrations will be determined. Questions relating to migration speeds and paths will be addressed at that time.

Comment: Differences in migration distance and patterns will be analyzed with ANOVA, but no information on how these parameters will be quantified is given. They are likely to be highly variable and of marginal use for evaluating spill-related effects. (ESC)

Response: The applicability of a Maximum Likelihood Estimator of fish migrations will be determined. Questions relating to migration distance and patterns will be addressed at that time.

Comment: Migration rate estimates will be influenced by the sampling frequency. Insufficient information is provided to evaluate the appropriateness of this method. (ESC)

Response: The applicability of a Maximum Likelihood Estimator of fish migrations will be determined. Questions relating to migration distance and patterns will be addressed at that time.

Comment: There is no indication of how differences from geographical effects will be separated from effects due to presence of oil, where the primary definition of oiled and unoiled is based on geography. As most of the oiled areas occur in one part of the Sound and the unoiled in another, factors other than history of oil exposure would affect the variables measures. (ESC)

Response: Fry have been sampled at many sites in both oiled and non-oiled areas to minimize any bias resulting from local effects.

However, it is recognized that geographic effects confound oil effects. A bioenergetics model, multiple regression techniques, and analyses of stomach fullness will be used to evaluate growth conditions in oiled and non-oiled areas. All available data on environmental conditions in oiled and non-oiled areas will be used in these analyses.

Comment: The study design introduces a stock-related bias that is not controlled or tested. There are potential stock-area interactions that are not controlled or tested. (ESC)

Response: Repeated measures analysis of variance will be used to determine if there are differences in fry growth among tag lots. Data from tag lots that are not significantly different will be pooled into groups. Comparisons of growth between oiled and non-oiled areas will be made within these groups. Refer to the previous comment/response for a discussion of area effects.

Comment: The methods for injury determination are weak: they consist of testing for growth differences, migration speed, migration distance, and migration patterns between oiled and unoiled areas. The criteria used to define oiled and unoiled are not given. The definitions of growth, migration speed, distance, and pattern are not given or are deficient. (ESC)

Response: Results from mixed-function oxidase and hydrocarbon analyses of fry will be used to determine the level of oil exposure of fry in different areas. This information will be compared with all available data on hydrocarbon contamination in the environment. Cumulative growth will be estimated by regression of final body weight on time for fish within tag lots or groups. Growth of individual fry over short time periods will also be estimated from otolith microstructure analysis. The applicability of a Maximum Likelihood Estimator of fish migrations will be determined. Questions relating to migration speed, distance, and patterns will be addressed at that time.

Part II

Comment: The detailed measurements and analyses to evaluate effects on abundance, distribution, habitat utilization, size, growth rate, feeding habits and migratory behavior are governed by the appropriateness of the field sampling program. This program may not have adequately included geographic effects or other natural variabilities. (ESC)

Response: The investigators considered geographic variation and other natural variabilities. Study sites were chosen to minimize

geographic differences while maintaining the treatment difference of oiled versus unoiled sites. Environmental parameters were monitored at each site to determine the extent of physical differences.

Comment: The abundance and distribution of copepods is dependent on factors other than oil, which are not addressed sufficiently to determine species abundance or distribution in a statistically significant manner. (ESC)

Response: Sampling to evaluate the effects of sediment contamination on harpacticoid copepods in 1990 was designed to reduce geographic variability by comparing heavily oiled and lightly oiled beaches within the same contaminated embayment. Substrate composition and algal coverage, as well as sediment hydrocarbon contamination, were quantified at each transect to determine their effects on the observed numbers and species composition of copepods. Procedures for collection were consistent across all sampling locations.

Comment: The abundance and distribution of meiofauna is dependent on factors other than oil, which are not addressed sufficiently to determine species abundance or distribution in a statistically significant manner. (ESC)

Response: The meiofauna recolonization experiment is a field-manipulative study involving azoic sediments that have been quantitatively contaminated with crude oil. Control and treatment sediments were randomly placed along the same beaches. Any differences observed in recolonization among treatment levels can thus be directly attributed to oil contamination.

Comment: Insufficient information is given to evaluate the appropriateness of the sampling frequency, which will influence the estimates of migration rates. (ESC)

Response: Coded-wire tagged juveniles represent groups of fish released at specific locales and times. Recoveries of these fish yield information on average rate of movement and general direction of movement within and between habitat types.

Comment: The comments made above in Part I regarding stock and location effects that are not controlled by this study design also apply to Part II. (ESC)

Response: The investigators considered geographic variation and other natural variables. Study sites were chosen to minimize

geographic differences while maintaining the treatment difference of oiled versus unoiled sites.

Comment: The study design does not indicate that variables that affect biota or biological endpoints are being considered when establishing a cause and effect attributable to oil. (ESC)

Response: The investigators considered natural variables. Environmental parameters are monitored at each site to determine the extent of physical differences. The study design selected oil and control sites in comparable habitat areas to minimize the effects of other variables.

Fish/Shellfish Study No. 5 - Dolly Varden Char and Cutthroat Trout

Comment: The Trustees will not be able to attribute differences in survival or growth to oil. The assumption that the differences in average growth rate are attributable to an external disturbance assumes that pre-spill growth and survival rates were similar in both control and treatment streams and any differences are entirely caused by spill-related effects. Natural variability or geographical differences are not considered. (ESC)

Response: It is important to point out that fish were sampled before any potential exposure to an oiled marine environment since the Dolly Varden and cutthroat trout were overwintering in freshwater when the oil spill occurred. Given this, the first sample from each stream (the emigration during 1989) provides the baseline data for stocks in control and oiled groups. These data indicate mean-length-at-age was similar among control and oiled groups which indicate that fish of the same size grow at the same rate regardless of their overwintering location. Since overwintering populations of Dolly Varden and cutthroat trout are composed of many genetic stocks and the ambient climates in the experimental areas of PWS are similar, differences in average growth rates were not expected. Therefore, large differences in average growth rates between control and oiled groups would be attributed to some external disturbance so long as initial size of fish is corrected for.

We do not have a direct measurement of pre-spill survival rates among the treatment groups but since the mean-length-at-age were similar between control and oiled groups this would indicate that survival rates were probably similar. If one of the treatment groups had higher survival rates a greater difference in the mean-length-at-age would be expected and in particular in the older age classes (age 4 and older) but this was not the case.

The experimental design, which includes replicate sites in both

treatment groups, does take into consideration both natural variability and geographical differences. The study tests for differences in growth and survival between treatment groups only with tagged fish. The analysis tests to see if the differences in growth and survival are greater between control and oiled groups than differences within each treatment group.

Comment: There are no baseline data to show populations in all study areas had equal growth and survival rates prior to the spill. (ESC)

Response: See the preceding response.

Comment: Differences in survival and growth rates are likely due to natural differences. There is no indication of how results will be analyzed to show a link between the oil spill and survival or growth differences. Data are not being gathered to analyze for spill-related effects. (ESC)

Response: See the preceding response.

Fish/Shellfish Study No. 11 - Herring Injury

Comment: It is necessary to account for natural variables that would give the same results when estimating the proportions of dead herring eggs from oiled areas. (API)

Response: It is because of the natural variables that control sites are included with the same treatment (depth and replicate level) parameters as the oiled sites.

Comment: Estimates of biomass often have sampling errors. (API)

Response: Sampling errors can be measured using the sampling techniques employed in the study and an accuracy goal (± 25% of the true mean, 95% of the time) has been met for the 1989 and 1990 biomass estimates.

Comment: The herring season yielded over 8300 tons in a 20-minute season, a record catch rate. Since herring do not die after spawning, living to spawn in several successive years, most of the 1990 spawning population was present in PWS in April 1989 when potential exposure to oil was greatest. As the 1989 herring season was closed, the harvestable surplus was not taken, resulting in a

larger fish population in 1990. This should have decreased fears of an impact on population, and resulted in reductions to the study program. (ESC)

Response: Much of the study is designed to measure the sublethal impacts (effects on egg production, larval survival, possible metabolic affects due to ingestion of oil, etc.), that proved to be significant in 1989. The sublethal impacts occurring from exposure of eggs, larvae, juveniles, and adults could affect populations for many years and affect egg survival of the 1990 production year. As a result, the study was continued to enable researchers to further define the potential impacts (complicated by sublethal impacts) and to further refine results analyzed from 1989 and 1990.

Comment: It is very unlikely that the determination of biomass to within ± 25% of the true value, a goal of objective 1, will provide the sensitivity to determine the impact of EVOS. (ESC)

Response: The biomass estimate may be utilized to examine the effects of EVOS on a population level, and if this were the only tool available to determine impacts, the above statement would be true; natural variability may mask some subtle impacts. However, this is not the only tool being employed to measure impact and, as stated in previously released results, sublethal impacts have been observed in early life stages of herring.

Comment: The oil level information from maps and analyses of mussel tissues does not represent hydrocarbon exposure of herring in PWS, and will not be useful in determining any impact from EVOS. (ESC)

Response: The Trustees disagree. Mussels are recognized in scientific literature as indicators of oil contamination. Additional analyses on herring will confirm effects of oil exposure of herring.

Comment: Mortality of eggs in the field is a function of density dependent survival and natural factors. The goal of objective 4 appears to be the development of egg loss information for better management of herring. (ESC)

Response: One of the goals in the study is to insure the accuracy of the spawn deposition biomass estimate. Since the egg loss factor is directly tied to biomass improvements in the egg loss factor and development of an associated error will result directly

in improving the accuracy of the biomass estimate. The fact that it will improve herring management is incidental to the determination of injury and may aid in future restoration of the herring stock.

Comment: The evaluation of embryonic and larval tissue for MFO is an experimental technique which is variable depending upon season, life stage, food type, etc. Impact assessment should not be used to develop experimental techniques. In 1989 NOAA found that DNA/RNA ratios did not provide significant endpoints, and there was no need to repeat this effort in 1990. (ESC)

Response: Use of MFO analysis has been demonstrated in the literature as a viable technique to document hydrocarbon exposure. Use of this technique has been recommended by expert peer reviewers.

Comment: The field methods indicate that the unacceptable ± 25% biomass estimation could be compromised by logistical problems. The Plan indicates that this work augments the ability to manage the resource so that EVOS damage can be predicted. This does not fall within the purview of NRDA damage assessment. (ESC)

Response: The Trustees disagree with this comment. Management of a potentially injured resources can only be done wisely with improved resource knowledge. As management of the resource hinges on the injury that may have occurred, augmentations of management techniques to aid in determination of injury are not only essential, but fall within CERCLA guidelines, and are economical.

Comment: The biomass to be estimated in 1990 will not include the fish that are the product of 1989 egg production. The plan provides that there were no significant 1989 adult mortalities. Thus, biomass estimation is necessary for herring resource management but has little to do with EVOS impact determination. (ESC)

Response: While adult mortalities may not have been documented, there may have been significant sublethal injuries to adults including: reduced reproductive potential, lower egg survival, and genetic aberrations to adults' tissues, including reproductive tissues. Further injury may be minimized through management adjustments, which is why accuracy in the biomass estimate and forecasted biomass is essential.

Comment: An estimation of fecundity is included, but the literature provides no evidence of fecundity effects on adult fish from one acute exposure to hydrocarbons. (ESC)

Response: The Trustees believe that there is a substantial scientific basis for estimation of fecundity. Some Literature shows that not only are eggs resorbed in exposed adult females, but oocyte-loss can be measured and quantified. In addition, there is no evidence that adult herring in PWS received "one acute exposure;" for example, Knight Island was heavily oiled, and it is possible that in 1989, adult and juvenile herring in this area were repeatedly exposed to various levels of toxic hydrocarbons. Samples of adult tissues, including sac roe collected in 1989 and 1990, may reveal sublethal injury.

Comment: The measurement of growth will be unable to discriminate differences with regard to EVOS. Growth is simply a parameter necessary for better management of herring. (ESC)

Response: Exposure of herring larvae to oil in the laboratory impedes growth which is a quantifiable injury. Whether differences could be detected in the field can only be answered by processing samples RNA/DNA. The RNA/DNA analyses have been run and future analyses to determine growth are not necessary. Knowledge of larval growth, using RNA/DNA techniques is not necessary for better management; growth in adults is measured during standard AWL sampling in the spring which is not funded by NRDA monies.

Comment: Herring exhibit density dependent survival. There is no relationship between herring spawning biomass and later recruitment, so the death of eggs is meaningless in this study. (ESC)

Response: The Trustees disagree. In a year where density dependent factors do not interfere and a cohort from a hatch will become a large part of the biomass in four years, oil exposure has the potential to inflict significant injury to that cohort and resulting returning biomass. There is no proof that the 1989 cohort would not have contributed significantly to the future biomass whether or not the stock was impacted. Therefore, the study was necessary to discover the potential impacts and to aid in possible restoration planning. In addition, death of eggs, whether from a strong recruiting cohort or not, is significant to PWS food chains, because herring are a prime link in the ecological chain in the Sound every year.

Comment: The statistics appear to be aimed toward the development of models with which to manage herring, rather than to detect impact attributable to EVOS. (ESC)

Response: The Trustees disagree. Statistical models are designed to address population injuries. Any management benefits are serendipitous.

Comment: The methodology focuses on modeling the population based on the number of eggs laid. The size of the 1989 year class will be estimated and compared with what it might have been based on measurement of 1989 egg loss. This may be impossible, as egg numbers do not equate directly to fish numbers. Number of eggs spawned will explain only a portion of the variation in abundance among brood-years, due to density dependent survival. (ESC)

Response: Estimation of eggs laid is used to back-calculate the year's spawning adults, not to predict the return from the eggs laid. Since fecundity, density of spawn, and extent of spawn are all well known, numbers of adults can be back-calculated using an estimated egg loss component, with relative accuracy. It is from the spawn biomass estimate that the next year forecast is made employing age dependent natural and fishery mortality components in the model. In addition, other models, such as catch-age analysis, are currently being employed to compare various estimation models and to provide an index of accuracy.

Fish/Shellfish Study No. 13 - Clam Injury

Comment: It is unlikely that objectives A-D will be attained, as the study design greatly underestimates the natural variability in all the biological and chemical parameters that will be measured, although the available literature on the effects of oil on intertidal clam populations is considered. (ESC)

Response: Incremental growth data (both pre-and post-spill) will be available for comparison of the growth rate by site.

Comment: The field sampling strategy is flawed. Hydrocarbon analysis is done on sediments and clams collected from the lower intertidal zone along transects oriented perpendicular to the shore. As samples are composited into single samples, gradients of chemistry and biological response at different shore levels are obscured, and sample variance is increased. (ESC)

Response: Due to the need to take samples in triplicate, it was deemed prohibitive to take triplicate sediment samples at each tide height that was sampled.

Comment: Except for the largest site differences, the amount of sample replication at each site may be insufficient to detect statistical differences. Differences due to natural causes will be difficult to distinguish from those due to oiling. (ESC)

Response: Stepwise regression using level of oiling, tide height and incremental growth will help distinguish natural effects from oil effects.

Comment: Necropsy analysis is improperly applied to mean histopathological examination. Necropsy would be unlikely to yield useful information. (ESC)

Response: The term "necropsy" will be changed to "histopathology" in appropriate studies.

Comment: Methods used to count live and dead clams are invalid. As it is usually impossible to estimate accurately how long dead shells have been in sediments, the presence of dead shells cannot be used to estimate the number of clams killed by the oil spill or later cleanup effort. (ESC)

Response: The enumeration of dead shells is only one method for possible differences between oil impact levels.

Comment: As the parameters measured are variable over small temporal and spatial scales, it will be difficult to characterize the baseline condition. Quantification of injury from the spill or later cleanup efforts may be difficult. (ESC)

Response: Characterization of the baseline growth rate in clams is accomplished by measuring annuli which are retained.

Comment: As background histopathology is poorly understood, it will not be possible to link any observed effects with EVOS. Relationships have not been established between observed histopathology and oil-related effects on the survival potential of natural mollusk populations. The significance of observed effects is questionable. (ESC)

Response: Histopathology results must be linked with data on the level of oiling in sediments, growth rate prior to the spill and growth rate after the spill.

Fish/Shellfish Study No. 15 - Spot Shrimp Injury

Comment: Estimates of biomass often have sampling errors. (API)

Response: This study does not attempt to estimate biomass.

Comment: The shrimp study focuses on the changes in individual organisms, rather than in the population. For this type of organism, without a demonstrated effect on a population, no injury should have occurred. (API)

Response: The study in 1991 is focusing on potential differences in the 1991 year class, not individuals.

Comment: Objective D are the study will test the hypothesis that hydrocarbon levels are not related to site contamination levels. The methods do not give procedures for collecting water/sediment samples to define the level of contamination at a site. (ESC)

Response: The two oiling categories are based on the initial path of the oil spill. The study has not continued to document oiling in the environment because the project assumes only an impact to the 1989 year class, which was in the water column at the time of the oil spill. The 1989 year class settled to the bottom in mid 1989.

Comment: For objective E, the methods do not describe what is meant by injury to tissues, what tissues will be studied, or how injury will be determined. (ESC)

Response: If hydrocarbon testing indicates exposure, then whole shrimp samples will be submitted for histopathology.

Comment: There is no documentation that selected control sites are sufficiently similar to test sites for baseline production of shrimp, and other environmental factors that could affect the results of the study. Aside from CTD water column profiles, there is no indication that environmental data will be collected. (ESC)

Response: The major focus of this study for 1991 is to determine if a difference can be detected between the two oiling categories for the 1989 year class. CTD water profiles are collected during each sampling period. The bottom salinity, temperature, and dissolved oxygen will be compared between oiling levels.

Comment: The oiled test sites have varying degrees of exposure to floating and stranded oil. The criteria given for selecting impact and control sites, and how those sites chosen will be documented for specific levels of oiling or exposure, is insufficiently explained. (ESC)

Response: The path of the oil spill was initially documented by aerial surveys. For the purpose of this study, sites were chosen as either oiled or unoiled based on surface oiling. No attempt was made to document the degree of oiling.

Comment: The shrimp pots described are designed to catch adult shrimp of commercial market size and are inadequate for the objective of determining if the 1989 year class had a high mortality rate in areas of high oil impact. (ESC)

Response: The shrimp pots utilized in this study capture male, transitional, and female spot shrimp. Occasionally juvenile shrimp are captured, however they are not fully recruited to the sampling gear. To determine if the 1989 year class was impacted, the year classes are separated using modal analysis. For the purpose of this study there is a lag between the time of capture and full recruitment for the 1989 year class. Alternative methods to capture shrimp smaller than 20 millimeters have not been developed.

Comment: How samples for hydrocarbons and fecundity will be handled and preserved in the field to ensure sample quality and integrity are maintained until analysis in the laboratory is not revealed. Chain of custody and QA/QC procedures are not discussed. (ESC)

Response: Samples for fecundity are frozen on board the research vessel. Each fecundity sample has a unique sample label, which accompanies the sample. The label identifies the location where the shrimp was captured. Upon return to port, the samples are transferred to a freezer at the Alaska Department of Fish and Game facility in Cordova. All hydrocarbon samples are maintained under strict QA/QC guidelines as established by the management ream. This includes chain of custody.

Comment: Except for tissue hydrocarbon measurements, no information is given as to what criteria will be applied for attributing differences to oil and what level of effects will be tested. (ESC)

Response: In addition to tissue hydrocarbon measurements, histopathology may be conducted if hydrocarbons are present. Significant differences in fecundity, egg mortality, catch per unit

effort, and year class strength will be compared between the levels of oil impact.

Comment: The probabilities of statistical Type I and Type II errors are not given. The sampling effort may not be appropriate to meet statistical analysis objectives. (ESC)

Response: The probabilities of 0.05 and 0.10 for Type I and Type II errors respectively are given in the "Methods" section of this study plan. Technical experts and statisticians have determined the sampling level that is appropriate and adequate to meet this study's objectives.

Comment: The number of individuals required per sample, as well as the interpretation of the results, will vary greatly depending upon what is sampled for tissue hydrocarbon analysis, which is not sufficiently described. (ESC)

Response: The objective of this comment is not clearly stated. The types of tissues being tested are muscle and egg. The types of hydrocarbons for which these tissues are being examined are listed in Appendix A. The number of specimens to be examined was determined, as noted above, by technical experts and statisticians.

Comment: There is insufficient information regarding the method for treating composited samples for hydrocarbon analyses in the analysis of results. (ESC)

Response: The tissues contributing to a composite sample are pooled prior to processing, thus the resulting values obtained refer to the entire composite sample rather than to any component of it.

Comment: Inadequate information is provided to determine what statistically significant differences will be detectable within the study design. (ESC)

Response: The differences the study is designed to detect, if they exist, are listed in the objectives of this study plan.

Comment: The objectives and methods do not indicate the study will lead to a quantification of the baseline condition, the level of injury, the variance of degree of injury in space, the length of time over which injury will persist, the likelihood and rate of recovery, or the link between EVOS and the injury. There is no indication that an exposure pathway will be documented. (ESC)

Response: Quantification of the baseline condition of the population was not an objective of this study. However, the study does attempt to assess the survival of the 1989 year class of spot shrimp that was in the water column as zoea larvae at the time of the oil spill and to view survival of this 1989 year class in the context of year class survival both pre- and post-spill. The study will also assess the survival of the 1989 year class in oiled and unoiled areas.

Fish/Shellfish Study No. 17 - Rockfish

Comment: This study is not consistent with the exposure requirements for conducting natural resource studies. (API)

Response: The Trustees disagree.

Comment: Ocean floor studies should be done only when there is data showing high concentrations of oil and a long residence time. If such data exists, it has not been made available. (API)

Response: Hydrocarbons were found in rockfish species that dwell on or near the bottom. Therefore these studies are appropriate. The aspects of the study involving sediments and sessile organisms were warranted to help determine route of contamination based on presence of hydrocarbons in bile from demersal rockfish.

Comment: The presence of hydrocarbons does not presumptively indicate injury. Evidence of the causality of the oil to injury must be shown. (API)

Response: Presence of hydrocarbons in organisms in treatment and not in control sites warranted efforts to determine presence or absence of injury. Further studies, specifically histopathological evaluation and MFO sampling, will help in establishing injury.

Comment: The study methods and data analysis sections do not describe how objective C, toxicological analyses of effects on growth and reproduction, will be conducted. (ESC)

Response: This portion of the study has been discontinued for 1991.

Comment: Objective D, the determination of the feasibility of using microstructure to evaluate depressed growth from oil contamination, implies an experimental technique of a research nature. (ESC)

Response: Use of otolith microstructure to show environmental stress is a proven technique. The feasibility aspect of this study is to ascertain its applicability to showing stress relative to the oil spill.

Comment: The reference to Rosenthal which cites seasonal variations in abundance in nearshore habitats contradicts the premise that demersal rockfish complexes have a high degree of fidelity to their habitat and are relatively sedentary. The study of reef habitats for histopathological and other long term effects may be invalidated by mixing of populations. (ESC)

Response: Rosenthal's study involved species counts using underwater transects. In that reference Rosenthal attributes changes in abundance and species composition species disappearing or becoming more secretive. This, in light of other studies, (especially Carlson and Barr, 1977), indicates that demersal species go into hiding during the winter months rather than leaving the area. Pelagic species will move into deeper waters.

Comment: Sampling locations are not identified, and the criteria for choosing sites does not indicate adequate scientific control or baseline determination. It is not clear whether the test sites are representative of the entire resource. The appropriateness of sampling sites as controls and test sites cannot be evaluated adequately, particularly regarding other important variables, such as other sources of petrocarbons. (ESC)

Response: Sample sites and selection techniques were identified in the methods section.

Comment: Sampling design is inadequately addressed and biased to improperly sample target fish species. (ESC)

Response: Demersal rockfish, specifically yelloweye rockfish, were being specifically targeted because during 1989 they were the species which were found dead immediately after the spill and also showed elevated hydrocarbon levels in the bile. The sampling design was not designed to get a random sample but directed to get a representative sample of the demersal fish at each site.

Comment: The level of effect from EVOS which will be tested for, and the probabilities of making Type I and Type II errors are not specified with respect to experimental design, sampling strategies and statistical significance. (ESC)

Response: The level of effect for our hydrocarbon analysis with a sample size of 40 fish in the treatment group will allow us to detect a difference of 20% with the probabilities of type I and type II errors of .05 and .2 respectively. The histopathological samples with a sample size of 60 fish will allow us to detect a difference of 15%.

Comment: The appropriateness of sample sizes specified cannot be evaluated. (ESC)

Response: See answer to previous comment.

Comment: How the different levels of variability (geographic, oiling, and reef communities) will be handled in the analysis is not explained. (ESC)

Response: The Trustees are not trying to account for different levels of oiling, but determining the presence or absence of hydrocarbons.

Comment: How samples for hydrocarbons and fecundity will be handled and preserved in the field to ensure sample quality and integrity are maintained until analysis in the laboratory is not revealed. Chain of custody and QA/QC procedures are not discussed. (ESC)

Response: Sample handling and chain-of-custody procedures were discussed in the methods section.

Comment: Bile, which is non-specific to hydrocarbon source and may be subject to interference by exogenous and endogenous compounds, cannot be analyzed to determine whether EVOS hydrocarbons are present in demersal rockfish. (ESC)

Response: The hydrocarbon analysis may be non-specific, however the presence of hydrocarbons in bile, in concert with other results, may lead to the conclusion of contamination by EVOS.

Comment: Tissue analysis to detect EVOS hydrocarbons is questionable due to the efficient, and possibly selective, metabolic functions in fish. (ESC)

Response: The primary indicator of exposure to hydrocarbons is bile. Other tissues will be analyzed only if results of bile analysis indicate further investigation is necessary.

Comment: Specific techniques for determination of hydrocarbons in sediments and tissues are inadequately described. How contamination will be defined and determined is not stated. (ESC)

Response: All samples for hydrocarbon analysis were sent to Auke Bay NMFS laboratory for analysis on site or through sub-contractors.

Comment: It is unclear how descriptions of otoliths are to be interpreted. It is not possible to determine how otolith-derived age composition and mean length-at-age data are to be used. (ESC)

Response: Otoliths and length data are collected as ancillary information to better describe the organisms being sampled.

Comment: The objectives and methods do not indicate that this study will result in a quantification of injury to resources. The objectives are split between documenting exposure and identifying aspects of injury. There is no indication that damage will be assessed beyond testing the statistical significance of its occurrence, or will be related to EVOS. (ESC)

Response: This study is designed to produce a qualitative assessment, rather than quantitative assessment.

Fish/Shellfish Study No. 27 - Sockeye

Comment: Studies appear to penalize potentially responsible parties for the increase in pink salmon population to the level which would occur without commercial fishing. That more fish is an injury defies common sense, and appears to be utilizing damage assessment in a manner inconsistent with Congress's intent. No evidence is cited which proves injury to spawning habitats, or elsewhere, resulted from the elevated population. (API)

Response: This is a sockeye directed project and the intent is to investigate the potential damage of sockeye overescapements to future sockeye production and to lake (freshwater) ecosystems where spawning and rearing occur. Given the sockeye salmon life-history (5-7 years), the evaluation of injury will be derived from studies being conducted in this and subsequent years.

Comment: This study does not account for the State's management of this ecosystem. Overescapement is the result of fishery management practices. Thus, this is not an EVOS impact assessment study. Measures could have been taken in anticipation of injury caused by the closure of commercial fisheries, such as the use of weirs, to

minimize effects. These measures would have been less invasive than those found in other fish studies. Studies should evaluate the impacts, if any, of historic management practices. (API, ESC)

Response: Overescapement is the result of not being able to harvest adult sockeye in traditional fisheries because of the presence of EVOS oil on the fishing grounds.

Comment: Evidence may exist that shows that salmon population levels have been artificially deflated by commercial fishing. The increase in salmon population should not be assumed to be injury, as positive effects are seldom injurious. (API)

Response: A basic tenet in the management of pacific salmon (all species) is that escapements beyond a specific level results will decrease numbers of adults in future populations. It may be true that sockeye production in some systems has been limited by heavy fishing pressure. However, the evidence for the sockeye systems being studied here is that escapement levels consistent with established goals will produce the greatest yields.

Comment: The study methodology does not provide data useful for correlating oil exposure with any potential observed fishery effects. (ESC)

Response: Sockeye salmon overescapements were caused by the lack of fishing pressure on the stocks due to the presence of oil on the fishing grounds. With the inclusion of non-impacted study sites, potential fishery effects can be factored out.

Comment: The determination of number, age, and size of sockeye salmon juveniles is selected freshwater systems is of marginal use in determining EVOS injury as no oil reached this freshwater spawning habitat. (ESC)

Response: Oil did not directly reach this spawning habitat, but may have reached the spawning grounds through contamination of adult fish. However, the main objective of this study is to document injury as the result of overescapements. These freshwater ecosystems are very oligotrophic and large numbers of predatory sockeye juveniles can disrupt the entire food chain of these lakes. Once disrupted, the food chain is difficult to restore, e.g., lost species of zooplankton.

Comment: The field methods were developed to perform fisheries research unrelated to EVOS. (ESC)

Response: The field methods employed in the EVOS related fisheries projects were, in large part, developed in research projects totally unrelated to the EVOS. This project uses those methods proven to be scientifically accepted.

Comment: The determination of injury in this study has no relation to EVOS. (ESC)

Response: The Trustees disagree. The injury to the freshwater ecosystem as the result of sockeye salmon overescapements may be linked to the EVOS.

Fish/Shellfish Study No. 28 - Run Reconstruction

Comment: There should be a modeling effort comparable to the one proposed in this study to assess damage to other components of the ecosystem. (UM)

Response: Pacific salmon support immense commercial, sport, and subsistence fisheries, and, for this reason, their life histories and population dynamics have been exhaustively studied for the past century. It is only because of this historical information that the run reconstruction study is possible. The governments intend to determine injury to the rest of the ecosystem and to restore these other components.

Comment: The salmon population dynamics in PWS indicate that the status quo is not stable, but transitional. Since even obvious factors affecting salmon population dynamics are not fully understood by area fisheries managers, it will not be possible to provide the input to describe the subtleties of historical population dynamics. (ESC) The effects noted as oiling values for parameters consider only negative values. (ESC)

Response: This comment is not clear. However, using the run reconstruction model to estimate catch and escapement counts in the absence of the oil contamination, the effects of the spill will be determined by the difference between these estimates and the observed catch and escapement counts.

Comment: The comprehensive timing model of Schnute and Silbert (1983) may not represent the salmon dynamics of Prince William Sound. (ESC)

Response: The timing model of Schnute and Silbert was developed for a terminal fishery. The PWS study deals with several stocks, which will add another dimension to the model. Using the

historical tagging data, the study will obtain estimates of district-to-district transition probabilities for each stock, allowing the addition of the Markovian exchange process to the timing model (Hilborn, 1989).

Comment: Testing the model parameters against a single year class will not be adequate to prove that the model works. (ESC)

Response: There exist thirty years of catch, fishing effort, and escapement data that can be used not only to fit the model, but also to evaluate its effectiveness. Moreover, simulation is also a commonly used technique in evaluating a model's effectiveness.

Comment: The model and the input data are not sufficiently described to determine if this modeling procedure is technically sound. It is necessary to know what EVOS effect the model is intended to detect and the Type I and Type II errors expected. (ESC)

Response: The references of Schnute and Silbert (1983) and Hilborn (1990) outline the timing model and procedures for obtaining maximum likelihood transition probabilities from tag recoveries. The study plan outlines how the two techniques will be combined for the PWS run reconstruction.

The model will provide estimates of the catch and escapement counts in the absence of the spill. Obtaining any probablistic confidence in these estimates (type I and type II errors of a hypothesis test, for example) will have to be done through simulation. Only until we have developed the model can we evaluate it.

Comment: The utility of models such as this is to provide a range of possible future conditions. Such models lack precision. Managers have had the data necessary to construct similar models for years, and have not done so due to their limited validity and application. (ESC)

Response: On the contrary, these models can be effective in environmental assessment and decision making. The models require an enormous data base source, a comprehensive understanding of the dynamics of the phenomenon to be modeled, and the latest in numerical software and computer hardware. This modeling effort will use publications of this past decade for its mathematical foundation and will utilize the thirty years of catch, fishing effort, escapement, and tag recovery data that is available.

Comment: The Plan states that these models will be useful for establishing harvest policies and for allocating fishing activities among areas and times. The investigators' approach appears to focus on the development of data for guiding fish allocation policy decisions and not on NRDA impact assessment. (ESC)

Response: The inspiration for the development of this model is for NRDA impact assessment. Once the model is built and the database developed, it can be used for future management decisions.

Fish/Shellfish Study No. 30 - Salmon Database Management

Comment: There is no timetable for accomplishing this study, and no explanation of the need for it in light of the salmon database management tasks being undertaken in Technical Services 3. (UM)

Response: Technical Services 3 provides a geographic information system. This project develops a biological database, and should terminate within one year after completion of field data collection and completion of laboratory analysis.

Comment: Although described as a study, the objective of this program is to develop the computing capacity and facilities to manage historic and spill-related data for the Trustee Council's efforts in the Fish/Shellfish area. The construction of a database system to maintain both historical and spill-related data does not fall within the purview of the NRDA regulations, nor does the structural facility to house that database system. (ESC)

Response: Development of the database is necessary for analysis of data collected in NRDA field projects. The hardware required by this project is limited to that necessary to accomplish the work.

COASTAL HABITAT

Comments on Coastal Habitat Studies

Phase I

Comment: The description of Phase I does not include enough information to ascertain whether the study has been designed to be statistically valid or how extrapolation from specific stratified random sample sites to all possible sites in a given category can be accomplished. (ESC)

Response: The objective of the 1990 plan was to provide summary information on individual studies, adequate for reviewers to understand the scope of the study. Statisticians have been consulted during the plan development to ensure that appropriate statistical designs are followed to allow for extrapolation.

Comment: No criteria for understanding how potential study sites were ground-thruthed have been included in Phase I. (ESC)

Response: As stated on pages 11-12 of the 1990 plan, the potential study sites were visited by coastal habitat personnel examining the sites' physical and biological attributes to verify their appropriateness as a matched pair to respective oiled or control sites.

Phase II, Part A

Comment: The study design of Coastal Habitat 1 does not permit the Trustees to estimate chronic or sublethal effects, particularly in fish. (NWF)

Response: The coastal habitat study is designed to measure sublethal effects to the intertidal fauna and flora considering such factors as mussel reproduction, intertidal fish parasitism, respiration, and growth.

Comment: This is one of the most important studies of the whole plan, yet it has improved only marginally in detail from 1989. The description of the study plan indicates that analysis of the 1989 samples was not sufficiently complete to be used to modify the 1990 sampling plan. Considering the funds expended, this is reprehensible. (UM)

Response: Sufficient analysis of the 1989 field samples was completed before the 1990 field work began. The study was modified to reflect the results of this analysis, including the pairing of sites, the selection of new study sites, and the addition of site-specific biological experiments.

Comment: Hydrocarbon analyses of plant and algal materials is still lacking. It is impossible to tell if productivity of subtidal plants and algae is being assessed. (UM)

Response: Due to the large number of field samples, the process of hydrocarbon analysis is taking a long time to complete. This information is being analyzed together with field measurements to determine the direct and indirect effects of oil, and the effects of cleaning on plant survival and productivity.

Comment: Although in the study description it is stated that an "integrated ecosystem approach" will be stressed, only lip service is given to assessment of the functioning of the ecosystem and potential for trophic transfer of contaminants. From the information presented it is impossible to tell if this will be accomplished. (UM)

Response: The coastal habitat study is founded upon an ecosystem approach to determining injury. By examining biotic and abiotic links within coastal habitat zones, and by providing information to those responsible for other damage assessment studies, it is expected that a comprehensive, ecosystem-wide determination of injuries may be established. A synthesis process integrating appropriate resources has been initiated.

Comment: The bibliography supplied is very dated in most cases, and the relevancy of the references chosen is somewhat questionable. (UM)

Response: The 1990 study plans included bibliographies of only selected literature; moreover, the quantity of literature pertinent to cold northern climates is sparse. A complete listing of appropriate literature is being assembled and likely will be made public.

Comment: This study may be continued in an attempt to document recovery of areas where significant effects are observed. Clearly, this and other studies where significant effects of the EVOS are observed should be continued at least until some estimate of the recovery period can be made. (UM)

Response: Coastal habitat data collection is scheduled to be conducted over a three-year period that began in 1989. Several samplings per year are being collected to assess potential injuries and recolonization (recovery) rates of intertidal flora and fauna.

Comment: Some measure of both aerobic and anaerobic carbon cycling, such as respiration and sulfate reduction, should be made to assess potential effects of the EVOS on energy flow in these systems. (UM)

Response: These measures are beyond the scope of the coastal habitat study.

Comment: There is no way to determine whether the study's objectives will be met. Without adequate description, it is impossible to tell whether results can be extrapolated to other sites exposed to oil. (API)

Response: The objective of the 1990 plan was to provide summary information on individual studies, adequate for reviewers to understand the scope of the study and the interrelationships between studies, and the scope of the overall damage assessment program. The Trustees believe there is sufficient information provided for that type of review.

Comment: Although the Coastal Habitat Study claims to be following an ecosystem approach, the level of detail provided makes it impossible to determine how well this will be accomplished, and to what extent community structure or function will be addressed. (UM)

Comment: The damage assessment section has been significantly expanded from the 1989 plan, but the information is still inadequate to determine how well injury will be assessed. (UM)

Comment: The Coastal Habitat study fails to recognize the findings of the Net Environmental Benefit Analysis, which indicates that oil-impacted areas are recovering. (ESC)

Response: The 1990 study has again been reviewed extensively by appropriate experts for design and cost-effectiveness and, where appropriate, has been revised accordingly. We have reviewed the findings of the Net Environmental Benefit Analysis and have incorporated any applicable findings into the study design.

Comment: Much of this year's work will be the "analysis of samples obtained in 1989." In 1989 "specific methods" were developed for each component of the study, but are listed by title only. Most of these are very procedural, ("locating quadrants, sample identification and chain of custody, sample storage and identification", etc.), somewhat generic, and necessary for any study. Usually, this information would appear in the QA/QC plan, which were not, other than the analytical chemistry and histology

groups, submitted as part of either the 1989 or 1990 plans. Only a few titles in this list indicate what type of data they are generating. (UM)

Response: The work performed in 1990 involved the completion of a limited amount of sorting and analysis of 1989 samples, and the collection and initiation of sorting and analysis of 1990 field samples. A detailed study plan of data analysis, collection techniques, and QA/QC standards was not included because the plan is aimed at providing a summary of individual studies adequate for reviewers to understand the general scope of the studies, and the interrelationships between studies.

Comments: To be reviewed adequately, the "Specific Methods" developed for this study would have to be available to qualified experts. (UM)

Response: The specific methods in the coastal habitat study may produce results used in litigation. They therefore constitute confidential information unavailable during the study process. A less detailed review version is provided to allow reviewers to understand the scope of this study and its interrelationship with other studies.

Comment: The "Specific Methods" section should be expanded to include measures of both primary and secondary productivity in matched oiled and unoiled habitats in the supratidal, intertidal and subtidal zones. (UM)

Response: Due to the extent of the spill-affected area and the study's primary objectives of estimating the quantity, quality, and composition of critical trophic levels in moderate-heavily oiled sites relative to non-oiled sites, an estimate of community function cannot be directly determined. The study, however, does take an integrated ecosystem approach to assessing the interrelationships between and within the intertidal community.

Comment: The flora and fauna of the intertidal communities of Prince William Sound and the Gulf of Alaska are thriving. Thus, the justification for the Coastal Habitat study is questionable. (ESC)

Response: Our data do not support the conclusion that these communities are thriving. The data to date have been extensively reviewed by experts, resulting in the continuation of the coastal habitat project.

Comment: The Plan does not address QA/QC or chain-of-custody issues. (ESC)

Response: The QA/QC standards were established by the analytical chemistry portion of the damage assessment process and are appropriately included in that section of the plan. There is no need to duplicate these standards in the coastal habitat study plan.

Comment: The Coastal Habitat study does not describe how information gained from subtidal sites can be related to the stratified random sample sties since subtidal sites were chosen independently of the intertidal and supratidal sites. (ESC)

Response: The subtidal portion of the study is not directly linked to the stratified random sampling design of the intertidal and supratidal portions of the study. The subtidal study is now incorporated into the Subtidal study 2, to which it was linked last year.

Comment: It is unlikely that the Coastal Habitat study will yield an objective quantification of injury and recovery since lightly oiled shorelines were eliminated from study and moderately and heavily oiled shoreline were combined into one category for the stratified random sample study. (ESC)

Response: Detailed hydrocarbon analysis is being performed on samples from each location that will yield a range of precise levels of hydrocarbons. This range in levels will provide the means for extrapolating injury.

Comment: There is no indication in the Coastal Habitat study of the total number of sites samples or their distribution between control and oiled sites; among Prince William Sound, the Gulf of Alaska, or Kodiak Island; or among habitat or the five shoreline types. Nor are the five shoreline types identified. There is no mention of whether any of the sites were sampled more than once in 1990 or the number of sites that were sampled in both 1989 and 1990. It is not known from the write-up of this study whether the same criteria were used to select sites in both years. (API, ESC)

Response: Page 10 of the 1990 plan identifies that there were 102 sites to be studied in 1990. These are distributed equally between control and oiled sites with 42 sites in PWS, 27 in CI/KP and 33 in KAP representing the five following habitat types: (1) exposed rocky shores; (2) fine textured beaches; (3) coarse textured beaches; (4) sheltered rocky shores; and (5) sheltered estuarine shores. Seasonal and annual collection of data has been integrated

into the study design integrating 1989 and 1990 sampling sites. A discussion of the site selection process is given on pages 10-12 of the plan.

Comment: Estimation of recovery rate in Phase II - Part A requires several sites visits. The study does not explain how seasonal changes will be integrated into the estimation of impact/recovery or what parameters will be used to predict recovery rate and potential for restoration. The habitat types examined in the stratified random sample are not provided. And the degree of oiling is not clearly defined. (ESC)

Response: Coastal habitat data collection is scheduled to be conducted over a three-year period that began in 1989. Several samplings per year are being collected to determine potential injuries and recolonization (recovery) rates of intertidal flora and fauna.

Comment: Methods are not given for random selection of sites in Phase II - Part A. Inclusion of nonrandom sites, chosen in 1990 for Coastal Habitat 1, may make the whole sampling design nonrandom. Thus, it may be difficult to extrapolate impacts to the entire spill-affected area. None of the statistical procedures for detecting differences are described. (ESC)

Response: The selection process is explained in the August 1989 and the 1990 State/Federal Natural Resource Damage Assessment and Restoration Plans. As explained in the 1990 plan, the additional 1990 sampling sites were deductively selected to provide additional spatial and habitat distribution thus providing a paired comparison that maintains the statistical validity of the design. Statisticians were consulted during the development of the 1989 and 1990 study plans to ensure that appropriate statistical designs were met.

Comment: The Coastal Habitat study contains no reference to the determination of appropriate restoration techniques or the assessment of the effectiveness of natural recovery. (ESC)

Response: Coastal habitat data collection is scheduled to be conducted over a three-year period that began in 1989. Several samplings per year are being collected to assess potential injuries and recolonization rates of intertidal flora and fauna. This information will be used to determine appropriate restoration techniques.

Comment: None of the methods used to determine hydrocarbon concentrations in sediment and soil are given in Phase II - Part A. Objective B of this study should be to "measure" rather than "estimate". (ESC)

Response: Hydrocarbon analysis of sediments and soil samples collected by the coastal habitat project will be analyzed in Technical Services study 1. The analysis will measure concentrations of petroleum hydrocarbons and their metabolites.

Phase II, Part B

Comment: The use of historical data in Coastal Habitat 1 may not be relevant because there is no information provided on the location of the ten historical sites from which mussel and sediment contamination data have been collected. If they are from low-energy, low gradient beaches at the head of embayments, they are not typical of most of the oiled sites in Prince William Sound. It is unclear from the Plan whether the methods for detection of petroleum hydrocarbons in the mussel tissues is the same for the historical and the post-spill samples. The presence of hydrocarbons in mussel tissues should not be considered an injury unless it can be shown that these oil residues are causing biological damage. (ESC)

Response: The historical sites and sites established just prior to the spill were placed at low energy sites where fine sediments are available. Although some of these sites received light oiling, they are not typical of the exposed areas that received moderate to heavy oiling. The information from these sites will provide a basis for evaluating the extent of oiling and impact in protected areas adjacent to high energy oiled areas. Analytical methods for the historical samples and post-spill samples are the same. The presence of hydrocarbons in mussel tissues are used as an indicator of availability to organisms of hydrocarbons in the water column.

Comment: Since no information is given as to the locations of the ten historical sites, it is not possible to know whether they are in areas that were affected by the oil spill. There is no explanation as to how the ten historical sites or the ten new sites were selected. (ESC)

Response: Most of the ten historical sites were outside the spill area with two or three exceptions. These are located in areas considered most likely to be oiled in the event of a spill. The ten additional sites were established during the early days of the spill in areas that were most likely to be oiled.

Comment: Increased efforts at modeling historical data and that obtained as part of the NRDA to predict both effects and recovery has only been given limited attention. (UM)

Response: Currently the historical hydrocarbon data are being examined in detail. Individual concentrations of aromatic hydrocarbons in sediments and mussels in spring, summer, and fall periods for the years 1977-1980 are being summarized. Results from NRDA samples will be compared to the historical data.

Comment: Part A of Phase II of the study contains only a general list of methods: there is no description of the number of transects per site or the number of tide levels sampled at each site; methods for sampling and analysis of biota and sediments are not given; there is no description of the tests of biological conditions and community function; and none of the methods for injury determination are included. Consequently, it is not possible to assess the technical soundness of this program. (ESC)

Response: One transect is sampled per site. Sampling on the transect is described in the study plan. Hydrocarbon analysis of sediments and mussels by GS/MS will occur under Technical Services 1. Direct injury will be documented if community changes are found on the photo transects.

Comment: There has been no consideration of shoreline treatment procedures in site selection for phase II - Part A and only one level (moderate to heavy) of oiling is being compared to control conditions, so the "response to varying degrees of oiling and subsequent clean-up procedures" cannot be measured. And it may not be possible to demonstrate any biological response unless all control sites in the stratified random sampling were randomly selected. (ESC)

Response: Sites were selected before any oil reached them and prior to shoreline treatment. Two sites subsequently received extensive cleanup and two received moderate to light cleanup. The sites included heavily, moderately, and lightly oiled sites. All sites were selected according to a stratified random procedure. They are all in protected bays, but the sediments vary somewhat.

Comment: There is insufficient information in Objective B of Phase II to determine whether field sampling or laboratory analysis methods used to collect the historical data are the same as those employed in selecting the 1989-90 data. No methods are outlined for determining whether differences measured over time can be attributed to the spill or to natural or anthropogenic changes. (ESC)

Response: The historical and new sites are treated the same for field sampling and laboratory analysis. Hydrocarbons in sediments over the baseline levels can be linked to Prudhoe Bay crude oil through the compounds analyzed under Technical Services 1.

AIR/WATER

Comments on Air/Water Studies - General

Comment: Other than a comment stating that extremely low concentrations of hydrocarbons in water and air observed during the 1989 sampling indicated that further sampling of these compartments was no longer needed, no rationale was given for why specific studies were excluded from the 1990 plan. Many of the species and life stages covered by the canceled studies are important resource species and/or sensitive components of the life cycle which could sustain damage in years subsequent to the spill. It would seem premature to abandon these studies so early in the damage assessment process. (UM)

Response: The portions of the Air/Water studies that were discontinued and not combined with another project were activities that the Trustees determined had little potential to contribute to the documentation of resource injury.

Comment: These studies are not cost-effective. (API)

Response: The Air/Water studies focus on a broad and complex ecosystem that provides the habitat for a large variety of organisms. Most of these organisms serve as prey items for higher trophic levels in the food chain. The overall documentation of the extent and persistence of EVOS hydrocarbons in the environment, and the pathways by which habitats became contaminated and the contamination entered the food chain, will continue to be understood poorly unless these studies are conducted. The Trustees have determined that the expense of conducting these studies is justified by their overall contribution to the documentation of injury.

Comment: These studies consist of basic research. They are not targeted to show specific injury and are not consistent with DOI damage assessment regulations. (API, ESC)

Response: An evaluation of injuries to the benthic resources and habitats addressed under the Air/Water studies is a critical component in assessing the overall injuries to natural resources caused by the EVOS, and is consistent with the DOI damage assessment regulations.

Comment: The only available techniques to measure the effects the studies are attempting to measure are unreliable. Sediment toxicity assays and their application are still being developed by the scientific community. Toxicity source identification methods for sediments are unavailable. (API)

Response: The GC-MS analysis performed on sediments under Technical Services 1 is a well developed scientific technique. It can identify enough hydrocarbon compounds in sediments so that major sources can usually be identified.

Comment: Major components of all three studies, particularly Study AW6, are research. Methods proposed for trace hydrocarbon analysis (AW2) and toxicity testing (AW6) are neither standard nor accepted for this purpose and any new methods will have to be developed as part of the studies. (ESC)

Response: The techniques used to document injury to the benthic resources and habitats addressed under the Air/Water studies are well established and documented in scientific literature.

Comment: The study plans are flawed in that they assume that the chemical analyses from sediment samples are related to the oil spill. The methods fail to take into account effects that have occurred over time from other sources. (API)

Response: The GC-MS analysis performed under Technical Services identified enough compounds so that EVOS oil should be distinguishable from hydrocarbons from other sources. Air/Water Study 6 examines long-term toxicity of weathered Prudhoe Bay crude oil and oxidation products of oil.

Comment: Site selection was not random. (API)

Response: As a random site selection approach to subtidal sediment sampling over the entire spill area is cost prohibitive, a paired sampling approach using oiled and unoiled areas is used. The Air/Water 6 study relies on the measurement of hydrocarbons in sediments under Air/Water 2 (samples were shared) to establish which sampling sites were oiled.

Comment: There are potential methodological problems with chemical analyses of sediment. (API)

Response: The GC-MS technique used to analyze sediments will provide a detailed breakdown of the hydrocarbon compounds present in sediment samples. This is a well established analytical technique accepted in the scientific community.

Comment: The method used most frequently to measure petroleum hydrocarbons in water and sediments (UV fluorescence) is specific for aromatic hydrocarbons and is not always conclusive in

distinguishing between aromatics from the oil spill and aromatic hydrocarbons from other sources. (ESC)

Response: The UV fluorescence method is used primarily as a screening device to determine whether hydrocarbons are present in a sample and to guide selection of samples for more detailed analysis.

Comment: Air/Water 2, 3 and 6 are not well integrated internally or with each other or with Coastal Habitat 1. (ESC)

Response: The sampling locations of these studies and the logistical support (vessel charters) are well-coordinated. Air/Water studies 2 and 6 share sediments from the same samples. Due to the stratified random site selection approach used for the Coastal Habitat 1 study for 1990, it was not practical to coordinate Air/Water study sites with that project. The subtidal eel grass bed sampling portion of the Coastal Habitat study was coordinated with Air/Water 2 sampling sites however.

Comments on Air/Water Studies - Specific

Air/Water Study No. 2 - Subtidal Sediments

Comment: It is questionable whether the budget cuts made as a result of combining Air/Water Studies 2 and 4 are appropriate since there is no line item in the budget for sample analysis. (NWF)

Response: Air/Water 4 was combined with Air/Water 2 in 1990. Collections for these studies were combined on tightly coordinated cruises that maximized the efficiency of the field sample collections. Air/Water 6 samples were taken on the same cruises. Sample analysis was performed by the individual components of Air/Water 2 with a major portion of the costs for hydrocarbon analysis of sediment samples included in Technical Services 1.

Comment: Proposed analytical methods are inappropriate to distinguish various hydrocarbon sources from EVOS. (ESC)

Response: Technical Services Study 1 established the quality control procedures for hydrocarbon analysis for EVOS oil. The methods employed will allow the identification of North Slope crude oil in cases where the analysis is being performed directly on oil found in the sediments sampled by Air/Water 2.