Certified:__________________
Date:__________________

MINUTES
Scientific Statistical Committee
September 5-6, 2001

The Scientific Statistical Committee met September 5-6 in Sitka, Alaska. All members were present except Jeff Hartmann, Seth Macinko, Ken Pitcher and Al Tyler.

Rich Marasco, Chair	Jack Tagart, Vice Chair	Steve Berkeley
Keith Criddle	Doug Eggers	Steve Hare
Mark Herrmann	Sue Hills	George Hunt, Jr.
Dan Kimura	Terry Quinn	

STELLER SEA LION

The SSC received presentations on four Steller sea lion (SSL) agenda items: the Draft Supplemental Environmental Impact Statement (DSEIS); the Draft Biological Opinion (BiOp4); a report from the RPA Committee (RPAC); and, a report from the Alaska Steller Sea Lion Recovery Team (ASSLRT). Tamra Faris, Dave Witherell, Galen Tromble, Sue Salveson, Lew Queirolo, Ben Muse, Mike Taylor, and Mike Downs provided an overview of the DSEIS. Shane Capron and Doug DeMaster summarized BiOp4 and provided information on changes relative to BiOp3. Larry Cotter and Dave Witherell outlined the RPAC's recommendations regarding sub-options under DSEIS Alternative 4. Earl Krygier provided an overview of the ASSLRT report on the RPAs developed as a response to BiOp3. Public testimony was provided by Thorn Smith (North Pacific Longline Association), Dave Fraser, Ed Richardson (At-Sea Processor Association), John Gauvin (Groundfish Forum), Donna Parker (Arctic Storm), and Alan Parks (Alaska Marine Conservation Council).

The DSEIS and BiOp4 are prepared to evaluate revised proposed federal actions to regulate the Alaska groundfish fisheries. The revisions are designed to mitigate presumptive impacts of the groundfish fishery on the survival and recovery of SSLs. Nevertheless, the true cause(s) of the continued decline and lack of recovery of Steller sea lions remain unknown; as does the impact of the groundfish fishery on Steller sea lion survival. Consequently, the proposed federal action is motivated by precautionary consideration of the potential for adverse fishery effects.

During 2001, the SSC was tasked by the NPFMC to critically review the November 30, 2000 NMFS Biological Opinion on Alaskan groundfish fisheries. We did so preparing extensive comments. By contrast the SSC has not had time for a thorough review of all aspects of BiOp4. Lack of comment from the SSC in these minutes does not necessarily imply agreement or acceptance of statements in the Draft BiOp4 or DSEIS. Nevertheless, the document reflects substantial improvements in analysis and balanced treatment of available information, treatment of alternative hypotheses, and explicit statements of assumptions. Thus, many concerns expressed by the SSC in comments on previous BiOps are addressed. However, a number of issues remain that the SSC would like to flag for the attention DSEIS readers and as subjects to be addressed in future studies.

DSEIS

Significance Ratings
The DSEIS uses a system of classification for the significance of assumed impacts of the alternative proposed management actions. One of the criteria (e.g. Table 4.1-5, page 4-21) for this system is the proportional change in annual TACs. The SSC recommends that change in the TAC not be used as the metric for significance classification. The global availability of pollock, Pacific cod and Atka mackerel is determined to be adequate to meet the foraging needs of SSLs in BiOp4 (p 147, lines 18-21): "*The effects described above indicate that the fisheries as proposed, are not likely to reduce the abundance of prey within local foraging areas and alter the distribution of groundfish prey in ways that could reasonably be expected to reduce the foraging effectiveness of sea lions, therefore, it would not reduce the likelihood of their survival and successful reproduction nor their likelihood of recovery in the wild.*" The DSEIS rates the impacts of harvest of prey as "conditionally significant negative (adverse)" for Alternative 1, 4 and 5. This inconsistency needs to be resolved.

Cumulative effects
The goal of this section is to identify and analyze potential cumulative effects of influences other than the directed federal action that may act additively or synergistically with the directed federal action to cause "major resource trends in the BSAI and GOA ecosystems." This effort represents the first time that a Cumulative Impacts section has been prepared for an EIS involving a North Pacific groundfish management actions. It also appears that under NEPA guidelines Cumulative Impact sections will be a regular feature of future EIS.

The SSC welcomes this effort and finds it to be of considerable potential value. However, the SSC also found this section to be somewhat confusing, needing some restructuring, lacking in explanations and literature referencing. Many of the scores in the tables appeared questionable and possibly unjustified.
A few specific examples of some of the shortcomings include:

- For the impacts on most fish species, three levels, or temporal categories, of climate effects are considered: short term, long term and regime shifts. None of these is well explained. For prohibited species, external natural events are contained in a single category, Climate and Regime Shifts. Marine Benthic habitat has four categories. Predator Prey and Biological Diversity are back to one category.
- The difference between long term climate variability (LTCV) and regime shifts (RS) is not explained. For every single category, the same score is given for LTCV and RS.
- With the exception of the category for Steller sea lions and predator-prey relationships explanation of the climate impact ratings is too brief.
- p. 4-427: "Lingering past influences on prey availability from foreign and domestic fisheries, and climate variability and related ecological regime shifts are identified for both the BSAI and GOA Pacific cod stocks." This statement is not supported by analysis or literature reference.
- There is no need in this impact analysis for 3 or 4 climate categories - a single category would suffice. In the discussion, an assessment of the importance and (if known) nature of a cumulative climate impact can be made.
- It would be of interest to know how the scores were determined. A climate influence is asserted for all fish species except GOA thornyheads and Pacific halibut. It is asserted that climate effects on halibut are "negligible because halibut are apex feeders." Halibut don't begin life as apex feeders and several papers have been published on the effect of climate on halibut recruitment. It is not possible to determine how the authors arrived these conclusions.

Monitoring/Observers

The August 2001 Draft Biological Opinion (section 8) and the DSEIS executive summary (ES-20 and ES-21) both contain a list of terms and conditions that are associated with the RPA monitoring measures. These lists contain terms that have statistical connotations such as: "statistically robust," "information should be sufficient to determine appropriate closures", and "statistically valid". Although such terms imply a certain statistical precision in estimates, they are undefined in this report. Estimates of statistical precision cannot be obtained unless they are made under a proper statistical framework. Many aspects of current analytic practice preclude such estimates; for example the lack of random assignment of observers to the 30% coverage fleet, the use of blend estimates, and the lack of replication information in observer catch estimates. Alternatives 2-5 would all create increased demands on the Observer Program. This would be mainly through the creation of larger number of quota categories (i.e., time, area, and species categories for which catch estimates have to be made) from the current 27 to 46 or more under the alternatives. This causes several problems:

1. A sufficient number of observers must be placed in these additional categories just to allow estimates to be made.
2. Observers will have an increased difficulty in correctly categorizing data.
3. Since precision is a function more of absolute sample size rather than the percent of catch that is sampled, inevitably, the precision of catch estimates in any particular quota category will be worse than over the current broader categories. This is inevitable without significantly increasing the number of samples taken.

Competition between Adult Pollock and other top predators

There is an underlying assumption in the DEIS that, for Steller sea lions and other top predators including seabirds, more pollock is better for the predator than less pollock. For those predators that eat adult pollock, there is little question that if the local (vs. global) biomass of pollock falls below some unknown threshold, there will be a negative impact on the ability of sea lions to use this prey. However, adult pollock may displace or consume more energy-rich or easily caught prey such as capelin, and particularly age-1 pollock. If it turns out that juvenile sea lions depend on small forage fishes, elevated biomasses of adult pollock near rookeries and haulouts may create formidable competition for critical prey resources. There is a need to examine the implications of competition and how it would influence the outcomes of the various alternate management actions. There is evidence for a negative relationship between the biomass of pollock on the eastern Bering Sea shelf and the productivity of at least one species of piscivorous seabird (Black-legged Kittiwake) at the Pribilof Islands.

Consideration of Alternatives and Options

The DSEIS essentially rejects Alternatives 3 and 5 in favor of Alternative 4 as the preferred alternative. In viewing the lists of evaluative criteria, the SSC notes that alternatives 3 and 5 appear to have some biological benefits over Alternative 4 but negative economic consequences. It would be useful to have further clarification about how the tradeoffs among these Alternatives were made.

Forage Fish

The term "Forage Fish" is used in different ways in the document. The DSEIS needs to descriminate between the FMP management unit unknown as "Forage fish" and those fish providing forage to piscivorous predators. The former is an explicitly defined group of fishes, while the latter includes unclassified fish as well as species classified under multiple FMPs. In the section on the prey requirements of Steller sea lions, Pacific herring and sand lance are included specifically, whereas in section 3.4, on forage fish, herring are not included. Additionally, age–1 pollock, at present the primary forage fish consumed by seabirds and possibly also important to sea lions, are not discussed explicitly. There needs to be some background information on the importance of both herring and juvenile pollock as forage fishes, and analysis of the potential effects of the various action alternatives on the likely availability of these two species of forage fish to sea lions. It is also possible that these fish should be analyzed in the cumulative effects section.

Draft Biological Opinion (BiOp4):

BiOp4 is much more deliberate than BiOp3 in its attempt to describe the standards used to evaluate jeopardy and adverse modification of critical habitat (Sec. 1.7 beginning on p 12). This is a welcome improvement.

The SSC had been critical of the failure of the prior BiOp to discriminate between an objective characterization of the state of knowledge about SSLs and fishery impacts on SSLs and the assumptions or inferences drawn from the objective data. BiOp4 takes care to be more specific regarding the critical assumptions required to reach conclusions that lead to rationalization of proposed management actions. A good illustration of this point is the description of the analysis of the "Forage Ratio Method" used to evaluate modification of critical habitat (p 141, Sec. 5.4.2.1).

Effective experiments to test the efficacy of SSL protective measures

In previous comments the SSC has supported adaptive management and effective experimental design to assess the efficacy of SSL protective measures. The SSC has stated that learning can only occur with a valid experimental design that includes contrast in the protection treatments. The failure to have done this a decade ago means that we still have learned nothing about the efficacy of the measures. The design must follow solid scientific principles, including testable hypotheses, evaluation of assumptions, and power to detect differences in trend. Although much new research is now being carried out, including the small-scale experiments to test the localized depletion hypothesis for pollock and Atka mackerel, no directed work is planned to link changes in fishery management with SSL status. The SSC regards monitoring the efficacy of the RPA measures essential and recommends development of a requisite experimental design to accomplish this task.

Global control rule

The modifications to the global control rule under Alternatives 3 and 4 are shown in Figure 4.2.1-2. Under Alternative 3, fishing mortality is reduced for all biomass levels below $B_{40\%}$ and set to 0 below $B_{20\%}$. Staff indicated that under Alternative 4 fishing mortality corresponds with that determined under the existing control rule. Therefore, ABCs for both rules are identical for all biomass levels greater than $B_{20\%}$. Under the revised rule, staff indicated that all directed fishing would be stopped, with only bycatch allowed, once the biomass drops below $B_{20\%}$. ABC at biomass levels below $B_{20\%}$ would continue to be based on the existing rule. The SSC recommends that the BiOp text be modified to reflect the intent as stated by staff.

Another consideration is whether a multispecies rule is desirable (e.g., stop all directed fishing on pollock when the combined biomass of pollock, cod and Atka mackerel drops below 20% of pristine.) A rationalization for this type of alternative is that if one species is at a high abundance level, then it might not be necessary to lower fishing mortality on the reduced species beyond that provided by the already-conservative status quo policy.

Telemetry Data:

Telemetry data form a cornerstone of the rationale for protection of specific habitats or habitat zones (3, 10, 20 nm protection zones). The interpretation of the data is controversial. The generic model for SSL distribution implies that they are predominately near-shore inhabitants and presumptively, near-shore foragers. Nevertheless, actual foraging distribution remains uncertain. The display of the distribution of telemetry "hits" has been criticized as potentially biased, and BiOp4 presents a hypothetical treatment of the assumed bias. It is important that BiOp4 acknowledge that a differential probability of siting based on distance from shore is a hypothesis. The assumed bias may or may not be present. Continued analysis of existing telemetry data and acquisition of new data will shed light on this question in the future.

BiOp climate section (4.4.1.1)
The climate section in BiOp4 (Section 4.4.1.1) is not well integrated with the climate section of the DSEIS. If the regime shift is going to be advanced as a primary hypothesis then this section needs to be better focused. There needs to be a more complete summary of the state of knowledge of long-term climate variability. There are many missing and incorrect literature citations.

Some examples of the lack of coordination

1 The Pacific Decadal Oscillation (PDO) is referred to in the DSEIS as a mode of long term decadal climate variability but is not mentioned in the BiOp. Further, the 20-30 year regime behavior of the PDO is implicated in the regime shift hypothesis affecting sea lion population trends.
2. The references Niebauer and Hollowed (1993) and Benson and Trites (2001) are not in the reference list.
3. There is not, in fact, "considerable disagreement" about the effect of oscillations on the carrying capacity of the North Pacific (pg. 74). The studies that are cited are not contradictory, but instead and present a coherent picture of how the relative fortunes of different species groups have differed.
4. There is a duplicate Merrick et al. 1987 reference in the reference list.
5. More information on the nature of decadal climate variability is contained in a recent special issue of the journal Progress in Oceanography (published in 2000). Several papers in that volume are relevant to this subject.

Research coordination and oversight.
In our June minutes, the SSC "urged [that] a more formal mechanism be formulated to ensure sharing and synthesis of data and ideas." We commend NMFS for appointing Lowell Fritz as coordinator of the new SSL research funded by NMFS. However, this research will be of greatest value if there is an ongoing effort to synthesize findings and share data and ideas. There is a critical need for NMFS to develop a mechanism for assuring that all sea lion research, regardless of funding source, is synthesized and that new research efforts are able to take advantage of and build upon ongoing work. One mechanism for this could be the development of a core group of investigators, not necessarily from NMFS, responsible for integrating research results and facilitating the synthesis and exchange of results.

Future Research Priorities.
In section 3.7.3 of BiOp4 (Expectations for information from new research programs), four areas are highlighted. The SSC would like suggest the following high priority needs.

Telemetry analysis.
In the section on telemetry data, NMFS recognizes that there is a need to account for the central-place foraging behavior of the sea lions, to account for the length of time sea lions are likely to spend beneath the water, and other factors. They do not account for variation in the size of the area available to animals within the distance zones away from colonies. To adjust for some of these issues, NMFS subtracted 90% of the observations between 0 and 2 miles from shore. It would be useful to develop a null model of the expected uniform distribution of observations and then examine the extent to which the observed at-sea distribution differs from the null model. This could help to identify preferred foraging zones or particular regions of importance to foraging sea lions.

Diets/Competition
In assessing diets of Steller sea lions, the method of reporting used to date has emphasized "percent occurrence". There is an need to develop the ability to determine the size of prey ingested based on the recovery of hard parts in scats and other material and to use these to determine the biomass and energy contributions of various dietary components to sea lion nutrition. The dependence on percent occurrence as the sole metric of prey use is potentially misleading.

Central to the issue of competition between fisheries and Steller sea lions for prey is the need to demonstrate that the most vulnerable life-stages of sea lions are taking the same species and size classes of prey and in the same regions and depths as the fishery. Since data are still inadequate to determine the size classes of fish used by adult male, adult female, juvenile and newly weaned pups, additional work is essential to resolve these issues. In the presentation of data on diets and foraging locations, it is particularly critical that NMFS specify the source of data on diets by date, location and age of animals, rather than just referring to "sea lions" as a general category. Likewise, when referring to pollock, it is important to note whether the fish taken by the sea lions are adult, juvenile or of unknown age.

In Section 3.1.1.7.3, on page 3-12, there is a statement that "Steller sea lions specialize in feeding throughout the water column in the epipelagic (herring), demersal (arrowtooth flounder), and semi-demersal (pollock, Atka mackerel) zones." This statement seems contradictory, as the types of fish taken and variety of depths at which foraging takes place suggests a very generalized forager. There also needs to be caution in statements about the use of prey "when they are densely schooled in spawning aggregations nearshore" as there are no data to support the conclusions about the behavior of prey at the time of capture. This statement apparently derives from the Discussion section of Sinclair and Zeppelin, and is not based on direct observation. It is, however, a reasonable conjecture, based on the known biology of the prey species.

<u>SSL Modeling</u>
As noted in our report on BiOp3, a comprehensive population model should be developed to understand the decline in the SSL population and to determine the impacts of additional actions to protect sea lions. Theoretically and statistically valid structural and time series models could be constructed with the available data. Such models could help address the magnitude and significance of population trends, the probability of continued decline or recovery, and the likely significance of factors contributing to the decline or recovery.

<u>RIR/Economic and Social Effects of the Alternatives (Appendices C, D and F)</u>:

1. As noted in the June 2001 SSC review of the DPDSEIS, *"value" is not a synonym for "gross revenue"*. Although it appears that the document authors intend "value" to be understood as "gross revenue," in most instances the use of the imprecise term "value" creates the potential for confusing gross revenues and net revenues, leading the incautious reader to misunderstand the magnitude of net economic benefits.

Our concern in this matter is not a simple complaint over semantics, rather it is a concern over a peculiar use of terminology in Appendices C and D and throughout the DSEIS that is at best confusing and in many instances misleading. While in most cases "value" seems to be intended as a substitute for "gross revenues", in other instances "value" is used as a synonym for the price paid to harvesters by processors, as a synonym for the net economic benefit accruing to harvesters or processors, and as a synonym for regional economic impacts. The DSEIS should be revised to replace "value" with the appropriate precise terms. In addition, readers of the DSEIS should be cautioned that gross revenue is not a meaningful characterization of the net economic benefits of fishing. Changes in gross revenues may result in similar or opposite changes in net economic benefits. Moreover, readers of the DSEIS should be cautioned that there is no statistical basis for the claimed (see e.g., section 4.12.1.4) significance of differences in the estimated economic impacts of the alternatives.

2. In section 4.10.2.6 (Effect of State of Alaska managed fisheries, state pollock fishers in Prince William Sound), it should be noted that there is no economic basis for the assertion that *catch changes up to ±20% are "not significant", those between ±20% and ±50% are "conditionally significant"* and that changes greater than ±50% are "significant". The impact of changes in the allowed catch will differ among individuals and between fisheries. In fisheries where economic rents

have been dissipated in the “race for fish”, even small reductions in the allowable harvest may result in substantial economic losses.

3. The DSEIS is over-quick to dismiss the impact of the alternatives on subsistence activities. When considering subsistence, it is essential to account for the role that commercial fishing gear and especially vessels contribute to the pursuit of subsistence fish and game resources. Even if subsistence harvests are unconstrained under the proposed alternatives, restrictions on the commercial fisheries may adversely affect the opportunity to pursue subsistence activities by reducing the cash available to pay for gasoline, etc, by reducing the number of vessels prepared for operation, etc. In effect, commercial and subsistence catches are jointly produced based on shared use of fixed and variable inputs. Although this overlap is particularly pronounced in the case of the small-boat State waters fisheries, it is also present in the CDQ fisheries.

4. The econometric model of groundfish markets provides an interesting initial characterization of supply and demand relationships. However, limitations in the structure of the model and problems with the estimated coefficients suggest that the results of the model should not serve as a basis for judging among policy alternatives. In particular, the statistical procedures employed in the analysis are not robust and several of the equations are poorly structured. The failure to find a statistically significant inverse relationship between quantity and price in the demand equations provides strong evidence of these statistical and structural problems. The model and results, as they stand in current form, are not useful. This is most likely the result of the lack of time available for development of the model. The modeling work is sufficiently important that it should be continued. Specific concerns and recommendations regarding the model are detailed in an appendix, below.

Appendix to SSC minutes–General and Specific Comments Regarding the Econometric Model of Groundfish Markets

General Comments

1. The model fails to successfully estimate the most important relationship- that between quantity and price in the demand equations. Until these relationships are determined, the model should not serve as a basis for estimating the potential impacts of the alternatives.

2. The equations are estimated using seemingly-unrelated-least-squares (SUR), an inappropriate procedure for this type of model. Instrumental variables must be used to replace right-hand-side (RHS) endogenous variables and estimation should be undertaken using three-stage-least squares (3SLS). Additionally, the reported goodness-of-fit statistics are not useful because they are done on an individual equation basis and say nothing about the performance of a system. Appropriate measures of goodness-of-fit (e.g., RMSE, and Theil U_2 stats) should be calculated for the whole system of equations through dynamic simulation in a program like SAS ETS. Dynamic simulated changes of prices in response to changes in landings should replace the reliance on elasticities. Elasticities, while interesting for examining small effects, are inappropriate when talking about large changes in quantities.

3. More care must be given to properly identifying each behavioral equation. This is especially important when using a systems-based approached. When using a systems-based estimation the effect of including a misidentified equation, with resulting bias coefficients, will filter though the entire system. Great care must be taken to assure that every equation is properly identified, i.e., all relevant variables included. Prices should be in real terms and quantities as per-capita quantities. It is unclear if this was done. If it were done it should be reflected in the presented equations. Failure to do this

violates the assumptions of demand equations being homogeneous to the degree zero and the adding up property.

4. When some variables are statistically insignificant, like monthly indicator variables, they should be dropped unless there are strong theoretical reasons to keep them. More work needs to be done on the choice of the functional forms of the equations. While linear forms offer advantages for estimation, they are often poor characterizations to the true relationships; other functional forms should be explored.

Specific Comments

1. Equation 3: U.S. Price of Pollock Fillets—The DSEIS remarks that "*The coefficient on Q_f has the expected sign but is very small, suggesting that the price of pollock fillets is not affected by changes in U.S. consumption. Put another way, U.S. buyers of pollock fillets are relatively insensitive to changes in price (D-21).*" This statement is incorrect. If the coefficient on quantity is small the price flexibility is likely to be high meaning that, if the reverse holds, that consumption will be very sensitive to prices. If this is true this means that if TAC is cut back that consumers will not bid up prices and is why the coefficient on the quantity variable, if estimated correctly, is small. A correct statement is "Put another way, U.S. prices of pollock fillets are relatively insensitive to changes in quantity."

2. Equation 5. U.S. Imports of Pollock Fillets—This equation is an excellent example of an equation that is not sufficiently identified as a demand equation. Where are variables to represent income, substitute prices etc? The lack of proper identification may be why the relationship between Q and P is not determined. The statement *"Surprisingly, import demand is found to increase with increases in the price of imports. The price elasticity of import demand is 0.79. This is consistent with the results of E3, where the world price was shown to move with the U.S. price. In other words, low U.S production will be associated with high world prices as well as an increase in demand for imports (D-23)"* is faulty reasoning. First, E3 says that the U.S. price is a function of the world price, not vice versa. But even ignoring this, the reasoning is incorrect. The reasoning for this statement seems to be that if U.S. production decreases this causes U.S. prices to rise, import prices then rise, and therefore as imports replace loss U.S. production you find this upward sloping demand curve for U.S. imports. Again, this is a function of the import demand curve not being properly identified. A properly identified demand curve will sort out the confounding effects.

3. Appendix D. p. D-4. "*Numerous past studies have indicated that the demand for groundfish is elastic, so that poor economic conditions in demand centers will cause (proportionally) large decreases in the amount of product demanded.*" If "elastic" refers to the own-price elasticity of demand (as opposed to income elasticity), the sentence makes no sense. The own-price elasticity of demand refers to the isolated relationship between quantity and prices not poor economic conditions occasioned by a recession, etc., the effects of which are removed in the formation of own-price elasticity.

<u>Other specific comments:</u>
Page 4-218 top: States that effects of fishing will have only an insignificant effect on prey abundance and availability to seabirds. The removal of adult pollock may enhance prey availability to seabirds?
Page 4-219: Disturbance to seabirds should be a non-issue.
Page 4-222, middle; 4-223 bottom: The statement that less fishing could result in more fish being available to seabirds is almost certainly wrong.

Modeling

1. p. 4-95 Fig. 4.2.1-2 The line describing the control rule under amendment 56 has a discontinuity at relative spawning biomass=0.05. The rule should intersect the axis at 0.05 and be continuous.
2. p. 4-96 Step 3. The solution to this equation is later described as F_{00}, but is never described as such in the text.
3. p. 4-97 top line. Should be for *“aa_{nage}”*
4. p. 4-97 Step 7. The equation is missing after “For $1<a<nage$”
5. p. 4-98 Table alternative 4, middle row. Should be $0.5B_{ref}B_{t,u}<B_{ref}$
6. p. 4-99 4th row from top. “proofed” should be “proved”
7. p. 4-100 Market Constraints (MC) section. 5 lines from bottom of page. What is meant by “Note that market constraints were not used in this analysis” is not clear. Why is this material presented?
8. P. 4-101 7 lines from top change “heaven” to “100%”
9. P. 4-101 4 lines from bottom “once” should be “ones”
10. General comment on the LP. Many values are constrained both below and above. Question: how does this affect the results? How often do simulations provide solutions on the boundaries?