CENTER FOR INDEPENDENT EXPERTS (CIE) INDEPENDENT PEER REVIEW

Biological Opinion on the Effects of the Federal Groundfish Fisheries and State Parallel Fisheries on listed species in Alaska, including Steller sea lions

Prepared for The Center for Independent Experts (CIE)

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EXECUTIVE SUMMARY

The Biological Opinion (BiOp) to be reviewed here relates to Federal and State Fishery Management Plans (FMP) in the Alaska Region and their potential to cause *jeopardy* under Section 7 of the Endangered Species Act (ESA). Jeopardy is poorly defined in the Act and in implementing regulations and does not provide clear standards. A Recovery Plan for Steller sea lions (SSL) was completed in 2008 in which criteria for reclassification and delisting under the ESA were laid out.

The BiOp was developed by NMFS Alaska Region Protected Resources Division (PRD) starting in 2006. A Draft was circulated in August 2010 and a Public Comment version in September 2010. Following limited consultation a Final BiOp was issued in November 2010. Because the implications of the BiOp are real and economically harsh, and because the SSL is a listed species, it is not surprising that the BiOp has added to and continued the long-running and heated debate about SSL and fisheries management. NMFS did not release comments on public submissions and there has been no formal opportunity for review of the Final BiOp though this was called for by the Scientific and Statistical Committee (SSC) of the North Pacific Fishery Management Council (NPFMC) in 2010. This report is one of three independent reviews undertaken through the Center for Independent Experts (CIE) at the request of NMFS. It has primarily been a desk-based review although a two day meeting was held in Seattle in august 2012 to allow presentations primarily on new information since September 2008. That meeting accommodated all request to make presentations. The Terms of reference (ToR) for this review require separate chapters on the BiOp as of November 2010 (and information as of September 2010) and on implications for the BiOp, if any, given information available since September 2010. The review thus contains two main chapters as well as a common background and comment on review process.

Risk analysis is a component of risk management. The context for risk assessment is the setting of standards against which risk can be measured. Once risk is assessed, the risk management process can consider appropriate risk treatments. Within a clear risk management framework, risk assessment can be an objective, scientific endeavor. Standard setting and risk treatment rely on non-scientific input to the process and the weighing of multiple factors. The BiOp would ideally be considered as a risk analysis and as a strictly scientific exercise. Certainly, the ToRs for this review focus on consideration of the science in the BiOp. However, the BiOp, inclusive of the Recovery Plan and in proposing RPA (Reasonable Prudent Alternatives to proposed management), effectively includes standard setting and development of risk treatments. The BiOp is therefore a difficult document to review from a strictly scientific perspective. The logic applied in the BiOp is driven by NMFS legal interpretation of Section 7 of the ESA. That logic creates a tension in the way evidence is weighed and conclusions reached. The BiOp also clearly represents an attempt by NMFS to demonstrate it has taken a hard look at all information in developing an opinion; this has resulted in a very long and citation-rich document. The combination of its mixed nature and at times reversed logic, together with sheer length, makes the BiOp a difficult document to digest.

New data since September 2010 are scarce. New information is therefore mostly limited to analyses of previous data or updates of analyses. Despite serious efforts to delve further in to the multiple areas of importance, there is no reason in Chapter 2 to alter Chapter 1 comments on the BiOp as of November 2010, based on information as of September 2010. Chapter 1 comments relate to the nature of the BiOp and, as noted above, its use of evidence and logic to arrive at conclusions as well as to specific scientific issues. The BiOp is focused on an action (fishing under FMP) and whether it is *likely* to create *jeopardy* to the SSL population(s) or to critical habitat, defined effectively as prey availability. NMFS finds that the action is likely to cause jeopardy and therefore develops RPA. The fundamental reasoning for the finding of jeopardy is that in the Western and Central Aleutian Islands sub-regions of the Western Distinct Population Segment (WDPS) of SSL, populations have continued to decline even as populations in other WDPS sub-regions have stabilized or are increasing. NMFS view is that the proximate cause of the lack of population growth in these areas is reduced and low reproductive output due to i) pups undergoing nutritional stress, ii) therefore weaning late, and iii) lactating pregnant females terminating pregnancies to enable them to continue feeding their pups. The ultimate cause for low reproductive rate, that is the cause of nutritional stress, is opined to be a reduction in food availability due to fishing. Alternative explanations for population decline or lack or recovery include nutritional stress caused not by fisheries but by natural prey field changes in the North Pacific ecosystem since 1977, predation by killer whales and perhaps other predators, contaminants, disease, direct mortality, etc. NMFS considers all of these to a greater or lesser degree and concludes that a number are possible or likely. It is the finding that fishery-induced nutritional stress is *likely* that leads to the finding of jeopardy.

In Chapter 1 of this review, the evidence used and conclusions reached by NMFS are considered. It is found that the evidence for fishery-induced nutritional stress is weak. The measure of reproductive output used, pup to non-pup counts, is potentially problematic; evidence for nutritional stress (whether fishery-induced or natural) is very limited and the hypothesis effectively remains conjecture; and the analysis of risks posed by fishing to prey fields is flawed. Consideration of alternative explanations is relatively brief and betrays some bias in evaluating the naturally caused nutritional stress hypothesis. While the BiOp concludes that fishery-induced nutritional stress is *likely* and thus the FMP cause jeopardy, this review finds the evidence to suggest no more than a possibility. The BiOp, however, applies a logic that effectively says if the effect is not disproven it must be likely whereas the standard for being likely applied in the review is more in line with that applied elsewhere for resource standards. Overall, the review finds all of fishery-induced and natural nutritional stress, and killer whale predation, to be possible. The reality is that the reasons for SSL lack of recovery in some sub-regions are complicated and may never be unraveled.

Despite these findings, the review concludes that generally the BiOp is thorough, accepting that for such a large undertaking some references will have been missed and some analyses might arguably have been extended or modified. It does not seem that important information has been excluded although some interpretation of available data is debatable, as noted above. Questions concerning the BiOp relate primarily not to the science *per se* but to the conclusions reached that link the science to legislation.

1 BACKGROUND

The decline in Steller sea lion (SSL) numbers since the 1980s has led to protracted and ongoing biological, economic and legal debate. The history of legal actions in the USA is well recorded in multiple documents, including the Final Biological Opinion (BiOp) issued by the National Marine Fisheries Service (NMFS) in November 2010 (NMFSa). The historical details of that debate are interesting and provide context for current deliberations but are not strictly relevant to the review that follows.

Work leading to the BiOp started when the North Pacific Fishery Management Council (The Council) recommended in 2005 that NMFS initiate consultation under Section 7 of the Federal Endangered Species Act (ESA) on the effects on ESA-listed species of Federal groundfish fisheries. Subsequently, in 2006, the NMFS Alaska Region Sustainable Fisheries Division (SFD) requested the NMFS Alaska Region Protected Resources Division (PRD) to initiate Section 7 ESA consultation on the Alaska Groundfish Fishery Management Plan (FMP) to evaluate the effects of Federal fisheries management on ESA-listed species, given information available since previous consultations. By agreement, the consultation was to consider a number of specified marine mammals, including SSL, split since 1997 in to Western and Eastern Distinct Population Segments (WDPS and EDPS respectively).

A Revised Recovery Plan for Steller sea lion was issued by NMFS in 2008 (NMFSb). That Plan includes criteria for threat classification and listing. The criteria are important as they provide a benchmark for population levels and trends and expectations when considering the action in question (FMP) under the BiOp. The BiOp must also, however, consider wider matters under the ESA, notably the effects of the action on critical habitat.

On 2 August 2010, NMFS released a Draft BiOp for public consultation, with an initial consultation of three weeks which was subsequently extended to 3 September. Approximately 10,600 submissions were received, the majority being form letters. NMFS issued the Final BiOp on 24 November 2010. NMFS did not provide commentary on submissions made or an explanation for any changes made to the BiOp in response to submissions. The Final BiOp is an extensive document with main text of close to 400 pages and a total length, including figures, tables and appendices, approaching 1,000 pages. It attempts to cover all relevant science concerning listed species, especially SSL, and fisheries of importance covered by both Federal and State fisheries FMP. The BiOp attempts to collate, synthesize and analyze available scientific and commercial information to provide explanations for SSL declines and recovery, or lack thereof, and to consider the potential impacts of Federal and State fisheries on listed species, especially SSL and most notably the WDPS, as well as on critical habitat (effectively prey availability by area). The BiOp ultimately attempts to conclude whether or not Federal and State fisheries as authorized by FMP are likely to cause jeopardy as defined under Section 7 of the ESA.

Many concerns have been expressed about the Draft and Final BiOp, especially about the science and the interpretation of ESA requirements. In raising concerns about the science, in August 2010 the Council's Science and Statistical Committee recommended an independent review of the BiOp. The Center for Independent Experts (CIE) has been engaged to provide

that independent review in August 2012, as outlined in FR77(112). This report is one of three produced by independent experts contracted by the CIE. The report considers extensive Terms of reference (ToR; see APPENDIX 2, ANNEX 2 and section 2, below). As required, the report includes specific sections and two "chapters" dealing separately with the scientific analysis and interpretation as of 3 September 2010 and, following a review meeting in August 2012, the implications of any new information since 3 September 2010.

The ToR for the review as outlined in the SOW are extensive. It is notable that the ToR are crafted to exclude i) explicit reference to the ESA Section 7(a)(2) 1973, as amended (16 U.S.C. §1536; ESA) requirement that Federal Agencies

> … *ensure that their actions are not likely to jeopardize the continued existence of any listed species or result in the destruction or adverse modification of critical habitat that has been designated for those species*

and, ii) because *jeopardy* is not defined in Section 7 of the ESA, the ESA implementing regulations (50 C.F.R. § 402.02) expanding that

> …*jeopardize the continued existence*" is defined as "… *engaging in an action that reasonably would be expected, directly or indirectly, to reduce appreciably the likelihood of both the survival and recovery of a listed species in the wild by reducing the reproduction, number, or distribution of that species*.

Presumably this crafting of ToR is to prevent reviewers getting distracted or mired in quasi legal interpretations or to question the NMFS interpretation of the law as reflected in the BiOp. Put otherwise, the ToR are aimed at ensuring the reviews concentrate on science *per se* rather than on the context provided for the science to proceed usefully as part of a *de facto* risk management process. This is understandable as the separation of science as risk analysis within a wider risk management framework (including risk treatment) is desirable. The problem is that for risk analysis to proceed, clear standards against which risk can be measured are necessary (see, e.g., IEC/ISO 2009). The ESA and implementing regulations do not clearly provide those standards. Separation of science from interpretation and the wider risk management process is therefore not straightforward. Indeed, the BiOp includes not just an opinion on whether the action might cause jeopardy, but also Reasonable and Prudent Alternatives (RPA); that is, it goes beyond risk analysis directly to advice on risk treatment.

In development of the BiOp, NMFS has arguably side-stepped the dilemma of definition by concentrating on the word *ensure* in Section 7 of the ESA, effectively making definition of *jeopardy* irrelevant. But substituting the implementing regulation text in to Section 7 (and cleaning up the grammar), the ESA requires that Federal Agencies *ensure that their actions would reasonably be expected, directly or indirectly, to reduce appreciably the likelihood of both the survival and recovery of a listed species.* If the focus is placed on *ensure* (i.e. providing certainty), then any possibility of creating jeopardy, regardless of definition, needs to be avoided. Logically, not just the action in question (FMP), but any Reasonable and Prudent Alternatives (RPA) must provide certainty as to avoidance of jeopardy. If, however, the focus is on determining a *reasonable expectation* that there is no *appreciable* (i.e.,

measurable) effect on the *likelihood* of reducing survival and recovery prospects, then evidence needs to be weighed somewhat differently. Judgment as to the science and interpretation in the BiOp must take account of the interpretation used. Under the first interpretation, evidence need merely suggest the possibility of the action or RPA causing any chance of a detrimental effect. Under the second interpretation, evidence needs to be weighed carefully to provide insight in to the degree of possible impacts individually and relative to other factors. Science is well suited to helping in the latter endeavor but arguably has a much lesser role in the former. In detail the ToR seem to suggest the latter course but the context for the detailed ToR and clear interpretations of the ESA laid out in the BiOp suggest the former.

There are a number of ToR for Chapter 1 that make it necessary to consider the issue of definitions for *jeopardy* and *likelihood*, as well as other terms used in the implementing regulations, notably *appreciably*. At ToR 2e, there is a requirement to consider whether the weight of evidence is *strong, moderate or weak* to support conclusions. This cannot be considered without translation of *strong, moderate or weak* into a likelihood scale. At ToR 6a, there is a requirement to consider whether the findings of the BiOp are *contradicted* by any scientific information. From a scientific perspective, contradiction is not absolute but also requires a balancing of likelihoods. At ToR 6b, there is a direct requirement to consider the *likelihood* of factors other than fishing negatively affecting population status, critical habitat or recovery. Generally in the BiOp there is an interpretation of ESA Section 7(a)(2) such that the verb *ensure* dominates the adverb (*not) likely* associated with the key verb *jeopardize*. No explicit mention is made in the BiOp of the implementing regulation in which *jeopardy* is further defined and where the terms *reasonably* and *appreciably* are used. *Appreciably* implies the ability to perceive or measure. As noted above, combining the legal definition from the ESA and the implementing regulation, there appears therefore to be a requirement that '*…actions would reasonably be expected, directly or indirectly, to reduce appreciably the likelihood of both the survival and recovery of a listed species …*' The point here is that attention to the ToR cannot strictly ignore definitions; some leeway must necessarily be taken in considering the ToR if there is to be any scientific value to the exercise.

Risk is most simply defined as the probability (or likelihood) of something bad or unwanted happening. Usually, the key issue in risk analysis is estimating quantitatively or qualitatively, the likelihood of the unwanted event. Depending on the event in question, and on societal values, the acceptable likelihood will vary. For example, in the well-known Marine Stewardship Council Certification Requirements (MSC CR, 2012), the definitions of "likely", "highly likely" and "with a high degree of certainty" vary depending on the issue/event in question. Definitions of the meaning of likely (or similar) are common in international standards and are well-known in national fisheries harvest strategy applications. Usually, for fish, marine mammals and other issues, likelihood requirements with respect to status define *likely* at the 70% level and with regard to consequences of actions the same or lower (e.g. 60%). For clarity, in considering the ToR, a similar standard for evidence is kept in mind.

2 REVIEWER'S ROLE IN REVIEW ACTIVITIES

2.1 REVIEW PROCESS

The review process is outlined in FR77(112). The process, including CIE involvement and the Review Panel to be held on 1-2 August 2012 at the Alaska Fisheries Science Center (AFSC), was notified in that listing, with an invitation that parties interested in presenting information should submit a statement of interest including an abstract. Background to the process, review Terms of Reference (ToR), and an outline of the Panel Review are provided in the FR. In the description of the Panel Review, due to limited time available, there is a statement that the schedule of presentations at the Review Panel will be considered by NMFS "…*in consultation with the CIE reviewers*…". NMFS distributed the draft agenda to the CIE reviewers and review meeting Chair by e-mail on 7 July 2012. As far as I am aware, the only comment was to note the tightness of the schedule and to query if all potentials presenters had been accommodated. Discussion with the meeting organizers from NMFS Alaska Region PRD and with meeting attendees suggested that all persons interested in presenting were satisfied with the agenda.

The Panel Review, independently chaired by David Fluharty (University of Washington), closely followed the agenda laid out in APPENDIX 3. Materials were provided in advance *via* web links given in the CIE Statement of Work (SOW; APPENDIX 2, ANNEX 3) and were additionally and conveniently made available by the CIE to CIE reviewers using a shared Google Drive folder set up for the purpose. File versions of presentations were not made available in real time but most were collected and made available for reviewers at the end of the meeting. Administration of the review seems to have been sound.

Review attendees are listed in Appendix 3. The list may not be complete as no attendance log was kept and the list was collated by NMFS Alaska Region PRD staff towards the end of the meeting. Attendees included a range of relevant (scientific, legal and policy) governmental staff, fishing industry and environmental NGO representatives, as well as the Chair and three CIE reviewers. I am not aware of any problems with notification of the meetings and interpret from the presence of stakeholder representatives, and lack of complaint, that notification was appropriate. Given the limited time available, preferential participation in the meeting was provided to the CIE reviewers. Participation by other attendees was generally in response to specific questions from the reviewers. There was no explicit opportunity given for public comment except through the presentations.

It is notable that the ESA does not require that scientific information developed to support decision/rule-making be subject to consultation, only the rule-making itself. States, however, are allowed to comment at all stages. It is arguable that as a primary input to rule-making, any scientific information and analyses constituting a BiOp might reasonably also be subject to consultation during and after development, especially when that analysis contains both risk analysis and advice on risk treatment. In this case, a Draft BiOp was released for public comment on 2 August 2010 and substantial public submissions were received by the extended but still short deadline of 3 September 2010 (of the order of

10,600 comments; see BiOp, Section 7.4.4, p343; of which *circa* 10,000 were form letters). The 2 August Draft and submissions were not provided to CIE reviewers; neither was any NMFS response to submissions or articulation of changes from the draft to the final 2010 BiOp, although such comments were apparently planned (see quoted letter from NMFS Regional Director in Bernard *et al*, 2011, p81). The CIE review meeting offered an opportunity for the many and various interested parties to follow up on submissions previously made on the Draft BiOp as well as on the Final BiOp. While the review meeting was ostensibly to consider new information available since publication of the Draft BiOp in September 2010, many presentations concentrated on materials relevant to the unseen (by reviewers) Draft and on the November 2010 Final BiOp, including how it was different to the draft, as well as on the Final BiOp.

Somewhat unusually, especially given the interest in the review, neither a report nor minutes of the review meeting are being prepared. As noted above, no formal log was made of attendees. The list of attendees (APPENDIX 3) and collation of presentations for electronic circulation were made *post hoc*. There is thus no formal record of the review meeting and the meeting itself has no products. The independent reviews by CIE reviewers are the only outputs from the process.

2.2 REVIEWER'S ROLE

The role of the CIE reviewers is set out in the CIE Statement of Work (SOW; Attachment A, attached here in Appendix 2, Attachment A). All three CIE reviewers are tasked with producing an independent report containing separate chapters relating to i) the BiOp as of 2010, and ii) the effect of new information since that time. The CIE reviewers are additionally tasked with contributing a brief synopsis of their individual desk reviews (of the Final BiOp) to be included by the Chair of the Review Meeting as part of the Executive summary; discussion with the Chair during the review meeting made clear that this was not required as no review meeting report would be produced.

In line with the SOW and ToR, I read the Final BiOp (November 2010) and related background materials as specified in the SOW (attached here as APPENDIX 2, ANNEX 3, and as part of APPENDIX 1), and undertook analyses consistent with the SOW ToR for chapter 1 (as outlined in the SOW and attached here as APPENDIX 2, ANNEX 2) in advance of, around, and after the Panel Review meeting. I participated in the Panel Review from August 1-2 in Seattle at the AFSC and subsequently reconsidered matters as outlined in the ToR related to Chapter 2 (as outlined in the SOW).

In accordance with the SOW Project Description and ToR, I have produced a final report (this report) with separate chapters (as sections 3 and 4 of this report) related to ToR relevant to: i) the Final BiOp as of 24 November 2010 and scientific information as available at 3 September 2010 (section 3), and ii) information available subsequent to issuance of the BiOp (section 4). Note that the SOW (see APPENDIX 2, ANNEX 1) specifies that each chapter should include a background section and comments on the reviewer's role in activities; for simplicity, a consolidated background and comment on process is provided (as sections 1 and 2 respectively), with sections 3 and 4 containing comments on the ToR and conclusions.

3 CHAPTER 1

3.1 SUMMARY OF FINDINGS BY ToR

1. *Read the Final BiOp (November 24, 2010) on the BSAI and GOA groundfish fisheries; and state waters parallel fisheries for groundfish fisheries and related background documents (list of documents provided is attached) and the recovery plan. Refer to Annex 3 for listing of Final BiOp report and background documents.*

Listed documents were read in advance, together with some additional source references. Following the Review Meeting on 1-2 August, other relevant but unlisted documents were identified and read.

2. *Provide a scientific peer review and comment on the final BiOp, including scientific information available to NMFS through the end of the public comment period (Sept. 3, 2010) for the Draft BiOp, evaluate the scientific information and its interpretation that developed the rationale and the subsequent findings regarding factors potentially affecting **Steller sea lion** population status, vital rates, critical habitat, risk of extinction, and recovery including in particular the findings regarding the effects of fisheries on Steller sea lion population status, vital rates, and critical habitat. Address the following:*

The BiOp deals with a number of species. It is noted here that the ToR deals specifically with Steller sea lion (SSL); therefore, no comment is made below on other species. Although the BiOp considers both the Eastern and Western Distinct Population segments (EDPS and WDPS), the focus is on the WDPS unless EDPS is considered to provide evidence with respect to WDPS. Similarly, within the WDPS, the focus tends to the Western Aleutian Islands (WAI), the sub-region in which decline is still evident and for which a finding of jeopardy and adverse modification (JAM) was made and RPA developed.

Note that ToR 1 requires reading of the Final BiOp as of 24 November 2010 but this ToR (2) requires scientific review and comment on the Final BiOp and on information as of 3 September 2010, the effective last date for information available to the BiOp authors.

   a. *Does the BiOp thoroughly and accurately (i.e. using the best available scientific information) describe what is known about the status of the listed species?*

Status may be considered generally in relation to population size and trends or more narrowly as related to definitions of listing classification. Information is presented in the BiOp to describe what is known generally about overall and regional population size and structure of Steller sea lion populations, as well as on vital rate trends. Information is presented on both the EDPS and WDPS as a whole, split by multiple sub-regions (sometimes also referred to as "sub-areas") defined by the SSL Recovery Team and important in the classification and listing criteria laid out in the 2008 Recovery Plan (e.g., "Western Aleutian

Islands"), as well as by Rookery Cluster Areas (RCA) defined in 2010 by the Alaska Fishery Science Center (AFSC; see BiOp section 3.1.3.2), and by NMFS fishery management areas (541, 620, etc.). It is worth noting that the sub-regions do not map perfectly to the separately defined RCA or to the management areas; this is unhelpful and at first confusing although the relationship between the categories is laid out in a text table in the BiOp (section 3.1.3.2) and in various figures (e.g. Figure 3.8). The key population categories for use in determining listing are the sub-regions; it would have been helpful to concentrate specifically on that classification and then to provide separate, non-interleaved, text for other categorizations. Regardless, it is unclear how the sub-region categories were determined; the only reference in the BiOp is a footnote (number 6 in section 3.1.3.2) that states the "subareas" (sic) are "*…geographically convenient but do not necessarily reflect biologically important units."*

The lack of consistent use of terminology and use of multiple categorizations with no clear rationale for definition or presentation of each is unhelpful. It is of particular concern that the second listing criterion (as laid out for the WDPS in the BiOp at Section 7.2) relies on measurement and comparison of trends within sub-regions. If those sub-regions do not reflect biologically important units but merely geographical convenience it is unclear why such a criterion exists. However, it is beyond the scope of this review to consider listing criteria.

The recently defined RCA seem to have been developed to provide a more detailed picture of trends across rookery clusters, with the driver apparently a desire to show patterns of trends (e.g., as seen in Figure 5.1). However, as for the sub-regions, it is unclear if and how the RCA relate to meaningful conservation-related population or demographic differences. The BiOp suggests the RCA are defined taking account of "*demographic similarities of animals in groups of sites*", as well as of survey areas, NMFS fishery management units and similarities in abundance trends. Of these factors, only demographic similarities, and perhaps abundance trends, are likely meaningful biologically. The details of the 'similarities' used to define RCA are not provided in the BiOp and the reference is to an unpublished manuscript; a request for information on RCA definition during the CIE Review Meeting was not filled.

The spatial definitions may or may not matter with regard to reclassification. However, that the criteria relate to the categorizations suggests there should be greater transparency and justification for the categorization(s) used. Also, when status descriptions are given, and classification considered, there needs to be consideration of sensitivity to the details of spatial categorization and monitoring, especially when there is no fundamental biological basis for the categories. Specific issues that should be considered are the use of "trend sites" rather than random/stratified sampling across the 161 identified rookeries, and the sensitivity of resulting trend estimates to differing categorization graininess. The BiOp justifies the use of trend sites as it reduces the variance on estimates (and therefore increases the statistical power in population trend detection); this is a good statistical justification but it ignores that i) use of consistent sites may introduce bias, which might also be different between sites and hence defined spatial categories (making, e.g., hypothesized west-east patterns in trends potentially misleading); and ii) a statistical justification does not constitute a biological one.

For non-pup population estimates, results are described in the BiOp with reference to sub-regions, RCA, and Western and Central Aleutian Island (WAI and CAI) fishery management areas (541-3) as well as for both whole of area DPS. Three methods for estimating trend are referred to (all in an unpublished manuscript from the AFSC. Results primarily reference one preferred method (cited as due to Johnson 2010). Despite the confusion of multiple (3) spatial categorizations and multiple (3) statistical approaches, the picture painted in the BiOp is of a west-east gradient in population trends between 2000 and 2008. Confidence intervals are quoted and are also shown in the key figure (Figure 5.1) and some tables (e.g., Table 3.9) showing estimates by each spatial categorization. Care is needed not to over interpret these figures because i) the spatial categorizations are not justified explicitly and no sensitivity testing to their definition is considered; ii) the use of trend sites may introduce bias in trend estimation, rendering west to east comparisons invalid; and possibly iii) lack of transparency due to unpublished materials. However, taken at face value, the analyses suggest that only for the WAI (equivalent to fishery management area 543 and to RCA 1) is there a statistically significant trend (negative) between 2000 and 2008. For all other areas, howsoever defined, there is no statistically significant trend. This information appears to have been correctly portrayed in the synthesis at Section 7.2. In that section, however, further comments relating to listing criterion 2 are perhaps overstated. Specifically, and perhaps just reflecting poor drafting of the criteria, there is reference to the WAI coming close to reaching a 50% decline . The criterion says "*The population trend in any sub-region cannot have declined by more than 50%."* The comment at Section 7.2 refers not to a decline in trend but to a decline in absolute population.

As noted above, it is beyond the scope of this review to comment on the reasonableness of the listing criteria themselves.

The BiOp provides substantial information on trends in pup counts by spatial category. These are not used in the status classification but are important principally because of the recourse in the BiOp to pup to non-pup ratios as a potential proxy for natality (undefined in the BiOp but birth rate less first month pup mortality (Holmes *et al*, 2007); confusingly also referred to as "fecundity" in the caption to Table 3.8) and the linkage made between possible decline in natality and fishery-induced nutritional stress. Those issues are considered under ToR 4, below. The section in the BiOp on pups is hard to follow with confusing tables hiding numerous multipliers and comparing pup/female rates with 1976 even though no data are provided on 1976 (Table 3.2 shows data only back to 1978-1979). Of note in the section on pup production is the comment that the *core of the WDPS range is the eastern AI and western GOA*. It is interesting that no reference is made to differential importance of sub-regions in the status determination; all are treated as equal.

In the BiOp, at page 84, it is stated that the ratio of pups to non-pups "*provides a proxy of sorts for natality*." Limited discussion is provided that explains why the ratio may over- or under-estimate natality but no clear justification is given for use of the ratio, nor is there any consideration given as to how biases in inferences about natality might occur through time or differentially within areas (due, e.g., to differences in feeding trip duration by area, weather conditions affecting foraging opportunities and/or survey timing, etc…) It is therefore difficult to understand how to interpret pup to non-pup ratios through time or

across areas, as they are presented. It is also difficult in the BiOp to unravel estimates from data and how conclusions have been reached. At section 3.1.3.2, no detailed information is given on the cited modeling (Holmes *et al*, 2007) though direct consideration of the cited paper does explain fully the data and models used and inferences drawn. From Holmes *et al.*(2007) there does seem to be support for the conclusion that in the Central GOA at least (based on models), and possibly the Western GOA and Eastern AI (directly from data), there has been a gradual decline in natality from the mid 1970s to the present time. From this, it might (but only might) be possible to infer an overall decline in WDPS natality over the same period. Certainly, the evidence with respect to the Western AI is not compelling. The evidence cited in the BiOp also suggests a lower current pup to non-pup ratio in the WDPS than the EDPS. Associated with estimated and inferred declines in natality in the WDPS are estimates of increases in juvenile and adult survivorship over the same period in the CGOA. In the section on status, these changes are reasonably reported. What matters, of course, is not how they are reported in the section on status, but how they are used to construct hypotheses and arguments in subsequent sections, most notably about fishery-induced nutritional stress (see 4, below).

Section 7.4.3 summarizes the issue of interpretation of pup to non-pup ratios, making reference to Table 5.8 which summarizes for all sub-regions information on trends and status. The section states that pup to non-pup ratios *are* an indicator of reproductive rates (or natality) and that the Central GOA *has experienced* a 36% reduction in natality over the past three decades. In the summary section, these statements are provided as fact even though there are clearly caveats and alternative interpretations. More concerning with respect to natality is that while there is apparently a clear difference (in 2009) between the WDPS and EDPS, and between the WAI and other WDPS sub-regions, there is little difference across the remaining WDPS sub-regions in which population trends diverge. Using the WDPS to EDPS comparison of inferred natality to support an argument of nutritional stress in the WAI does not seem reasonable when the natality and stock trend indicators in the remainder of the WDPS suggest there is no linkage. Ultimately, the BiOp argument in support of nutritional stress, itself only an hypothesized mechanism to explain a decline in natality (the only indicator of nutritional stress listed in Fig. 4.25 that has been observed or inferred), comes down to the statement on page 342 that "*A reasonable explanation consistent with the pattern of natality in the western DPS relative to the eastern DPS is that the western DPS is nutritionally stressed because other hypotheses related to mechanisms associated with decreased natality (e.g., disease and contaminants) have not been supported with the available data*." This ignores the within-WDPS population trends *versus* natality patterns as well as the multiple caveats that could affect natality inference through time and across areas. In any case, failure to find an explanation elsewhere does not constitute good evidence, and certainly not proof, that nutritional stress constitutes a "*reasonable explanation*."

The ToR asks "*Does the BiOp thoroughly and accurately (i.e. using the best available scientific information) describe what is known about the status of the listed species?"* The answer is a qualified "perhaps". As pointed out by others (e.g. Boyd, 2010), while the BiOp dismisses work that counters its main sources, the main sources used for inference are often barely described and used uncritically (e.g., in the case of natality estimation, Maniscalo *et al.*(2009) is explicitly criticized while Holmes *et al.*(2007) is barely described

and caveats are little mentioned despite being of major importance to the hypothesis developed). The summary of section 3.1.4.2 is a good example of how the BiOp tends to overstate conclusions based on weak evidence. On page 92 it is stated that:

> *… the lack of a robust rate of increase in abundance in the 2000s in this population [is] associated with decreased reproductive success at least in some areas:*
>
> • *Birth rates for Steller sea lions in the central Gulf of Alaska in the period 1998-2004 declined 36% from those estimated in the mid-1970s (Holmes et al. 2007, York 1994, Holmes and York 2003).*
> • *Young females collected in the 1970s were larger than females of the same age collected in the 1980s (Calkins et al. 1998). Given that decreased size translates into delayed maturity and decreased body condition for reproduction, the lifetime reproductive success of females collected in the 1980s was inferred to be lower than those collected in the 1970s.*
> • *Female pregnancy rates appeared to decline between the 1970s (67%) and the 1980s (55%), consistent with the hypothesis that reproductive effort in the 1980s was compromised Pitcher et al. (1998). This decline suggests a high rate of fetal mortality and/or an indication of stress (possibly nutritional) experienced by individual females.*
> • *Late season pregnancy rates in lactating females declined between the 1970s (63%) and the 1980s (30%), indicating a decreased ability in females to support a fetus and successfully complete consecutive pregnancies (Pitcher et al. 1998).*

Consider the four bullet points. The first point refers to Holmes *et al.* (2007) and two other papers. However, on page 89 only one paper quoted, by Holmes *et al.* (2007), is strictly relevant to the quoted 36% decline in birth rates over time. The other papers are quoted in the same, brief paragraph but are not obviously relevant to the summary bullet point. No caveats are mentioned. The other three points all effectively relate to a single paragraph on page 90. The second bullet refers to a conclusion from a paper but there is no text that analyzes that and other papers to determine if conclusions are in fact robust and directly supported by evidence. There is a danger of quoting inferences that may themselves have been conjecture. The third and fourth bullets refer to the same paper. The third point, however, is a good example of how the BiOp transforms suggestion and possibility in to fact. On page 89 the quoted statistics on near-term pregnancy rates are provided and it is stated the difference is not statistically significant. On page 90, the figures are again quoted but this time in describing an actual fall in birth rates. On page 92, the figures are again quoted as a major, summary bullet point. Admittedly the word *appeared* is used, fudging the lack of significance, but the bullet continues to introduce the notion that the decline is caused by nutritional stress. As presented this is entirely speculative. There is no supporting analysis. The fourth bullet, again citing the same paper, refers to a single sentence in a paragraph on page 90. The summary refers to decreased reproductive output at least in some areas but only the first summary bullet specifies the area of relevance although all refer to papers in which the areas of relevance are specified

The section on reproduction and growth (3.1.4.2) is five and one half pages long and includes approximately 100 references (some repeated) but very little critical analysis. The

conclusion and summary on page 92 cites just 5 of those 100 references, two of which seem inappropriate. The bullets themselves restate only occasional sentences buried in the section, principally in just two paragraphs amongst the five and one half pages. Is the BiOp thorough and accurate? It is thorough as a reference list but arguably not thorough in describing and analyzing references of importance. It may or may not be accurate in its conclusions but as presented they are not well supported.

b. *Does the BiOp thoroughly and accurately describe what is known about groundfish fishery practices and catch statistics under the current ongoing "status quo" action, as defined in the BiOp?*

Section 2 of the BiOp provides a thorough and accurate description of the groundfish fishery practices and catch statistics. Arguably, it is the clearest and most complete part of the BiOp. The description of key stock status as of September 2010 takes account of stock assessments and projections as of 2009. This fits with the timing of the draft BiOp released for public comment in September 2010. Although new stock assessments for key species were completed at that time, they had not fully been processed through the Council system and were thus not included.

c. *While the agency is directed to evaluate the effects of the action on listed species and critical habitat, does the BiOp also adequately address alternative scientific explanations to the apparent population dynamics of the WDPS of Steller sea lion, such as (but not limited to) predation, disease, ecosystem/carrying capacity, or emigration?*

It is not clear what is intended by the first (dependent) clause, nor by the word "also" in the second clause. Presumably, the dependent clause indicates NMFS thinking that the action (FMP) affects fisheries directly and through them the listed species and critical habitat for SSL. Given NMFS' apparent focus on *ensuring* that the action does not cause jeopardy (see section 1), this suggests a lesser weight being placed on non-fishery explanations for SSL decline and lack of recovery. From the narrow focus of ensuring no anthropogenic jeopardy this makes sense - the BiOp needs to focus on the action. From the wider perspective of balancing evidence to determine the relative importance of anthropogenic *versus* other effects, it is not so straightforward. The word *also* in the ToR appears to indicate NMFS presumption that the fishery-induced nutritional stress hypothesis is adequately addressed in the BiOp; this, however, is the subject of a separate ToR (no 4) and is commented on also at ToR 2(a).

The ToR lists possible alternative reasons for *apparent population dynamics*. The list of alternative factors is prefaced by *such as* and is not intended to be complete but it does include the main alternative options – predation (notably by killer whales) and nutritional stress through poor diet due to ecosystem/carrying capacity changes (rather than fishery-induced nutritional stress).

The BiOp, in section 4 and in summary in Section 7, considers a range of explanations for the SSL decline and recovery, or lack thereof. The BiOp is clear that for some issues information is lacking. The BiOp is also clear in concluding that *no one factor can explain the overall and local patterns and trends in abundance* (page 264). Sections 3 and 4 generally address reasonable alternative explanations although it is arguable that the issue of environmental change and poor diet (e.g. at 4.2.1, 4.7.1.1) has been lightly treated. Of concern, however, is not so much how well the BiOp addresses each alternative explanation as the way in which the BiOp summarizes the alternatives and their importance relative to the primary explanation of fishery-induced nutritional stress. In section 4, on pages 264-265, bullets 1, 3 and 4 are reasonable. Bullet 5 refers to killer whale predation. The first half of the bullet is a fair summary of information presented in section 4 but the latter part of the bullet refers to "*compelling evidence to seriously question the hypothesis that killer whale predation was the primary factor driving the overall decline of [the WDPS]."* This is an overstatement of the arguments presented (for example, on pages 260-261). In any case, regardless of causes of decline, more important in the BiOp would be a summary of understanding of factors affecting potential recovery; this is missing. It would also be useful to see comment on whether killer whale predation is included in multispecies modeling (Aydin, 2007, 2010) described in the BiOp though ultimately not used. It is noted that multispecies modeling by Guénette et al.(2006) is referenced and difficulties with drawing conclusions explained. Bullet 6 effectively restates bullet 1 but provides a link to more detailed exploration of the effects of the action in section 5. While restating that it is hard to disentangle multiple factors, the bullet essentially re-emphasizes the fishery-induced nutritional stress hypothesis even though bullet 2 does not point very conclusively in this direction. The most notable bullet point in the summary is in fact number 2. The bullet starts by noting there is evidence inconsistent with the nutritional stress (however caused) hypothesis but that it is possible to interpret some evidence as consistent with it. The argument is that comparison of the WDPS and EDPS lends support but that evidence within the WDPS (essentially WAI and CAI *versus* EAI and GOA) shows inconsistencies. This is true and can be seen from Table 5.8 (see ToR 4, below). This use of evidence seems counter-intuitive; why is greater emphasis placed on comparisons across wide spatial scales and between-DPS instead of finer scales within- DPS? The conclusion that "*Therefore, nutritional stress cannot be dismissed…"* is wrong. Dropping *therefore* might lead to an acceptable statement but the logic implied by *therefore* is flawed and is reliant on treating evidence unequally; evidence against is essentially dismissed while evidence not inconsistent with the hypothesis is given weight. In a sense this is at least arguably consistent with the scientific approach of not rejecting a null hypothesis until there is statistically significant proof to do so. However, this is not in reality the way most ecology progresses (see e.g. Gurney *et al.*1999). It is also not the way common sense or legal tests would usually proceed.

The Roman indents at bullet 2 cover that acute nutritional stress does not appear to be an important mechanism but that chronic nutritional stress "*if it is occurring"* is a reasonable mechanism related to the lack of robust population growth in the WDPS. This is again a conjecture but with an indication that the hypothesis is in fact not definitive. The third and fourth Roman bullets again put the emphasis on the fishery-induced rather than environmental drivers for chronic nutritional stress. The indent on environmental forcing, as at 4.7.1.1, says very little while the indent on fishery induced nutritional stress strangely (in the context of the BiOp) only says it is "*possible"*, "*could contribute"* and that the

relationships are "*inconclusive"*, likely varying temporally and geographically. The indents do not place particular emphasis on fishery-induced nutritional stress, though the final bullet, 6, does. It seems that the BiOp generally relies on the logic betrayed in the final sentence of bullet 2 and is driven towards a consideration of fishery-induced nutritional stress less by compelling evidence than by the fact that it is the only possible risk factor which might in fact be treatable (if it is occurring).

Interestingly, the synthesis and conclusions laid out in Section 7 are more balanced. Competitive interactions and killer whale predation are admitted as possible factors in *depressing the rate of recovery* and changes in carrying capacity due to environmental change are acknowledged. Other factors such as disease, physical conditions (affecting survival) and direct mortality are noted. The issue of nutritional stress is (implicitly as due only to naturally caused) is effectively dismissed in two bullet points. Interestingly, the life history arguments used to dismiss naturally occurring nutritional stress are pertinent also to a consideration of fishery-induced nutritional stress, though this is not mentioned.

Much has been made of the BiOps failure with respect to consideration of killer whale predation and of environmentally driven nutritional stress (aka 'poor diet' or the "junk food hypothesis"). Generally, the BiOp does contain relevant information and analysis on killer whale predation. It does not dismiss killer whale predation as a causal factor in decline or lack of recovery. The issue seems to be adequately addressed although, as alluded to above, its potential importance may be understated in section 4 (though not in Section 7).

With regard to poor diet and effective changes in carrying capacity due to environmental change (as opposed to fishery-induced), the BiOp considers the issue in multiple places, describing work on environmental change and its effect on in fish stocks, fish distributional changes through time, and competition and ecosystem linkages, as well as SSL foraging, nutritional requirements, and food availability and quality. The BiOp reasonably points out (e.g. at section 4.1.5) the great difficulty in disentangling natural and fishery-induced drivers of SSL population change. The BiOp is clear that the perceived most likely obstacle to growth (or *recovery* in terms of listing criteria) in the AI is chronic nutritional stress acting to reduce reproductive output, especially in the WAI, by causing delayed weaning leading to a decline in near-term pregnancy rates as females suckle their young for longer and abort pregnancies. This mechanism, if correct, could be caused by either or both natural (poor diet) or anthropogenic (fishery) factors. The nutritional stress issue as such is considered below in section 4. Here, the only real issue relevant to the ToR is whether or not the BiOp adequately addressed the issue of whether natural diet quality and availability could cause decline or prevent population growth regardless of fishing.

Throughout the BiOp the term *hypothesis* is regularly applied even though there is considerable conjecture. The section on "Junk Food" in the BiOp (3.1.14.3) refers to the "*junk food hypothesis*" as a "*notion"*. This is pejorative in the context of the BiOp and it is hard not interpret the language as betraying bias. Essentially, the poor diet hypothesis is a variant on the nutritional stress hypothesis. It relies on the same core reasoning (as fishery-induced stress) with nutritional stress leading to reduced reproductive output through prolonged weaning and consequent reduction in near-term pregnancy rates. Unlike fishery-induced nutritional stress, the poor food hypothesis says that due to regime change (in this

case *circa* 1977) the natural prey field has shifted and is now more dominated by low energy species (notably Pollock). In terms of the core nutritional stress hypothesis the issues are as considered at ToR 4; it is possible but the evidence is weak. In terms of the driver for nutritional stress it is necessary to consider information on prey fields, ideally through time for areas of interest, but perhaps across areas. One way of looking for evidence across areas would be to deduce corollaries of the hypothesis. The BiOp considers food suitability, ration and nutrition from an energetics perspective. There is a wealth of information on the subject but it is not conclusive. Bernard *et al.*(2011) in commenting on the BiOp (using data as for the BiOp) approached the issue by examining evidence across WDPS sub-regions in support of corollaries. They found some support for the hypothesis on two counts (one if pup to non-pup counts are discounted as evidence of natality) and no information on three counts. They could not therefore reject the hypothesis. The BiOp also does not dismiss the poor diet hypothesis; it simply downplays it. In terms of this ToR, the BiOp adequately considers the energetic issues related to the hypothesis but could have made more of the available information as demonstrated by Bernard *et al*. Of course, this may not have altered any final conclusions as the BiOp ultimately addresses just the effects of the action and deals (without being explicit) with risk and potential risk treatment.

d. Does the BiOp thoroughly and accurately assess the effects (direct and indirect) of the action on the listed species and its critical habitat?

Conclusions and synthesis on the WDPS at 7.4.3 are generally fair, providing balance in interpretation of data although it would be possible to quibble with a few instances of "*may*" that enter in. It is in the conclusions at 7.4.5 where the overall finding that the proposed action "*…is likely to jeopardize the continued existence of the [WDPS]…"* is cause for concern. The penultimate paragraph fairly points out the equivocal nature of the evidence and appears to attempt to pull back from a finding of jeopardy. The sentence noting lack of specific data [in the WAI] notes the difficulty of evaluation. The final sentences then revert to the necessity of the finding given ESA requirements. The problem lies with the movement from equivocal evidence and a balanced synthesis to the use of the term *likely* in the final determination. The information available - the scientific evidence - reveals only the possibility, weakly supported (see ToR 4), that the action might cause jeopardy. This does not pass the scientific test of constituting *likely*.

With respect to SSL critical habitat, the summary at BiOp Section 7.5 contains rather more *mays* than Section 7.4.3 and makes clear the difficulty of interpreting the limited information on critical habitat (i.e. Atka mackerel, Pollock and Pacific Cod availability as SSL prey).The final jeopardy conclusion is effectively reliant on the very limited information contained in BiOp Fig. 5.8 from which (at 7.5.3) a negative relationship between the intensity of fisheries and SSL population response since 2000 is inferred. The analysis in section 5 is not extensive. Essentially, the information used to develop conclusions is all contained in Fig. 3.10 and Table 5.8. The Table mixes trends from 2000-2008, pup to non-pup ratios in 2009/2008 and 2008 fishery information and Fig. 3.10 shows pup count changes from 2005 to 2009. Some care must be used in interpreting such diverse statistics, especially in the absence of confidence intervals or other information on errors, though it is recognized that the 2009/2008 ratio and 2008 fishery statistics are probably correctly

matched. It is especially difficult to know how much confidence to place in the inferred negative relationship when comparing trends from 2000—2008 with single year fishery figures for 2008, especially the fishery fraction by nm zone; how typical is that fraction? How has the fraction varied from 2000 to 2008? The BiOp concentrates on the zonal fractions and draws the conclusion of a negative relationship and hence that the action "*…is likely to adversely modify the designated critical habitat for the western DPS of Steller sea lion*."

It is a matter of interpretation, but the conclusion of a negative relationship is not straightforward from the limited data to hand. The first point of note is that despite a trend in trends (as seen in Table 5.8), some rookeries show exceptions (Fig. 3.10) and the trends in pup counts as seen for 2005-2009 in Fig. 3.10 and for 2000-2008 (as in Table 5.8) do not appear to be consistent. This adds some doubt to comparing the 2000-2008 trends with any given fishery year (2008 in this case). The second point is that although there is an apparent west to east general increase in pup and non-pup trends, some eastern areas (e.g. RCA 8 and 9) show little increase. Third, despite the trends in pup counts, apart from RCA 1, pup to non-pup ratios are effectively constant, making interpretation *via* the nutritional stress hypothesis fragile. Fourth, while fishery zonal fractions are given, it is unclear whether any analysis should instead consider biomass availability by zone after fishery removals (i.e., species specific forage ratios by zone). The BiOp inference of a negative relationship is superficially appealing but the caveats make interpretation difficult and the conclusion of *likely* adverse modification of critical habitat again tenuous.

The most direct way of determining if there is a negative relationship between fishing and SSL is through direct statistical testing. Numerous studies are reported in Bernard *et al.*(2011) that attempt to find relationships between fish catch, effort or CPUE and SSL counts or vital rates. All but one of the reported studies predates the BiOp. Those studies that consider SSL variables since 2000 primarily result in non-significant results although some suggest a positive relationship between the count of non-pups and fisheries in FMA 541-543. The explanatory variables in these studies, however, are catch and effort. None of the studies have been able to consider prey availability as opposed to fishing. This is an unfortunate but understandable weakness. Given the great difficulty in developing understanding of the complex processes working at the individual SSL level (behavioral and physiological responses to environmental variables) but manifesting at the population level (e.g. in vital rate estimates), the most profitable route to determining whether the action could indeed cause jeopardy is directly to consider the relationships between fisheries and SSL. The studies referenced by Bernard *et al.*and the BiOp attempt to do that and need to be encouraged. Such work may not reveal deep biological understanding of processes but it gets to the heart of risk analysis and potential risk treatment. In order to proceed, fine-scale and representative data are needed both on fisheries, fish stocks, and SSL. Fisheries are well monitored and temporal and spatial data on catch and effort are available. Fish stocks are monitored through surveys but only at fixed times; though they can also be monitored through fisheries. Deriving indices of local prey availability and composition throughout the year, and across years, is critical if statistical analyses are to proceed usefully; that might require enhanced localized sampling across years. Data on SSL at sufficiently fine-scale are also critical in order to allow comparison. Data on counts and diet are the most important to allow direct statistical testing of relationships.

The Bop at 4.5.3 pays relatively little attention to the direct statistical testing of the relationship between fisheries and SSL. The summary at 5.1.7.6 covers briefly the problems with data availability and scale, and the need to measure prey availability rather than fishing that confront robust statistical analysis. Ultimately, however, the summary and progress towards conclusions hinges on the statement "*…the Agency is not required to establish a statistically significant cause-and-effect relationship under the ESA. Rather as noted above, the ESA requires the Agency to conclude that a given action is not likely to jeopardize the continued existence of a DPS or adversely modify its critical habitat*." And the bullet "*At this time with available data, it is not possible to demonstrate a statistically significant relationship between commercial fisheries on pollock, cod, Atka mackerel and arrowtooth flounder and the productivity of Steller sea lions in the western DPS. However, it is also not possible with the available data to conclude that commercial fisheries are not having a significant impact on the recovery of the western DPS of the Steller sea lion*." The general finding of no negative relationship is thus used to conclude that "*a significant impact*" is "*possible*". Regardless of NMFS reliance on ESA interpretation, this is perplexing. Why does the lack of evidence allow the possibility of a ***significant*** impact? Why is ***possibly*** later transformed in to being ***likely***.

Doubt is also cast on the conclusion of jeopardy to critical habitat by the limited information on the Commander Islands in which the population of SSL is reported to have declined substantially through the 1980s and 1990s, as in Alaska, and has shown no signs of recovery. The population has not recovered even though a fishery closure to 30nm has been in place since 1958 (prior to the decline) although not effective possibly until the mid1980s (Burkanov, pers. comm. during Review Meeting).

Returning to the TOR and the question "*Does the BiOp thoroughly and accurately assess the effects (direct and indirect) of the action on the listed species and its critical habitat?"* the answer is that the BiOp is reasonably thorough given information available and that it is accurate insofar as it describes data available at the time. It is debatable, however, whether the final conclusions relating to jeopardy are scientifically "accurate". As at ToR 4, from a scientific weight of evidence perspective the action possibly has an effect on WDPS recovery and on critical habitat functionally. The conclusion that these effects are *likely* or *significant* is however an overstatement of the evidence.

e. Evaluate the scientific weight of the evidence presented in the BiOp. Does the evidence provide strong, moderate or weak support for the discussion, findings and conclusions made in the document?

See all other ToR for details. The evidence for a change in natality through time, as the basis for population decline and or lack of growth, in the WDPS is weak. The nutritional stress hypothesis depends on needing to explain that weakly evidenced trend and is itself only weakly supported. Most evidence that would support it is missing or neutral, notably direct estimates of changes in weaning age and near-term pregnancy rates by area. The primary evidence used to support the hypothesis is in fact the putative trend in natality. Even accepting nutritional stress, the evidence for fishery-induced nutritional stress is weak. Considering nutritional stress as a threat the measures of exposure, necessary to translate

the threat in to a risk, are weak. The exposure is considered in terms of i) overlap between SSL and fisheries, through poorly justified and incompletely measured indicators, and ii) the extent of fishing, through removal and rate measures on a scale wider than the likely scale of importance. Evidence for alternative explanations is variable. The two main competing explanations are predation by killer whales, for which there is moderate evidence, and environmentally-induced nutritional stress for which evidence is also weak. The weight of evidence for fishery- *versus* environmentally-induces nutritional stress is comparable.

3. *Reviewers shall evaluate the quality and completeness of the scientific and commercial information used in the BiOp analysis, and identify if the BiOp analysis is comprehensive or if there are relevant scientific or commercial data or information that were not used in the BiOp analysis.*

In developing arguments, the BiOp cites extensive literature, some published, some not, but does not thoroughly analyze all available information. Analysis of so much information is all but impossible and the BiOp is in any case arguably already barely digestible. Focused omission is therefore not unreasonable (to use a BiOp-like double negative) especially on issues that are not influenced by the action (i.e., cannot be treated), but leaves open the door to criticism. Undoubtedly, some of the criticism leveled at the BiOp falls in to this category. In this regard, the BiOp does not help in that it frequently falls back on what looks like a presumption of fishery-induced nutritional stress, and regularly uses language that on many issues changes from suggestive (*may, could, might*) to definitive (*is, has*). Others have already commented on this (*inter alia* Boyd, 201; Bernard et al.2010) and further examples are unnecessary. Although the BiOp is generally sound, covering most issues with reasonable thoroughness, it is therefore somewhat frustrating to scientific readers (see also comments at ToR 5).

4. *Reviewers are specifically asked to evaluate the scientific basis for the nutritional stress findings of the final 2010 BiOp. Reviewers shall evaluate and comment on the strength of the linkages among fish biomass estimates, fishery removals, Steller sea lion reproductive rates, and recovery of the WDPS. Does the BiOp accurately evaluate the inter-relationships between Steller sea lion population status and trends, foraging ecology, and groundfish fisheries effects across broad geographic areas (ecosystems to highly localized regions) and temporal scales (years to seasons)?*

Nutritional stress and the BiOp proposed mechanism for how it can affect reproductive output and hence population level changes might be caused by either or both of natural or anthropogenic drivers. This ToR therefore requires consideration of the nutritional stress hypothesis *per se* as well, separately, of possible drivers and their relative likelihoods. As the BiOp states at page 263 "*Both climate change and fisheries induced changes in prey communities likely have affected the condition of Steller sea lions over the last 40 years, but the relative importance of each is a matter of considerable debate*."

As noted at ToR 2(c), at section 4.1.5 the BiOp points out the great difficulty in disentangling natural and fishery-induced drivers of SSL population change. The BiOp is clear that the

perceived most likely obstacle to growth in the AI is chronic nutritional stress acting to reduce reproductive output in the AI, especially WAI, by causing delayed weaning leading to a decline in near-term pregnancy rates as females suckle their young for longer and abort pregnancies.

Mechanisms at the individual animal level are considered in the BiOp at section 3. Section 4 of the BiOp makes useful reference to Fig. 4.25 in which a set of 17 indicators of chronic nutritional stress is listed; Table 3.17 provides a summary of data gaps for assessing physical manifestations of nutritional stress in the WDPS and includes comments on relationships found in the 1980, 1990s and early 2000s. Bernard *et al.*(2011) provide a helpful expansion of Table 3.17. In the context of review of the BiOp and action, the most relevant information relates to the early 2000s. A review of the BiOp suggests its Table 3.17 and Table 4.6 in Bernard *et al.*are accurate. Consideration of the Tables and listed indicators, and the extensive BiOp references and text, reveals that: i) the majority of indicators are untested for all periods; ii) indicators are generally consistent with the inference of acute nutritional stress in the 1980s leading to rapid decline; iii) indicators for the 1990s are equivocal in suggesting chronic nutritional stress; indicators for the early 2000s generally suggest no chronic nutritional stress; iv) for the late period, the only indicator of nutritional stress is the estimated decline in natality in the Central GOA due to Holmes *et al.*(2007).

The issue of natality trend estimation from pup and non-pup counts is covered under ToR 2(a) and some comments are also made there on the nutritional stress hypothesis. The BiOp at Section 7.4.3 summarizes the issue of interpretation of pup to non-pup ratios, making reference to BiOp Table 5.8 which summarizes for all sub-regions information on trends and status. The section states that pup to non-pup ratios *are* an indicator of reproductive rates (or natality) and that the Central GOA *has experienced* a 36% reduction in natality over the past three decades. In the summary section, these statements are provided as fact even though there are clearly caveats (see ToR 2a, above) and alternative interpretations. More concerning with respect to natality is that while there is apparently a clear difference between the WDPS and EDPS, and in 2009 between the WAI and other WDPS sub-regions, there is little difference across the remaining WDPS sub-regions in which population trends diverge (with decline in WAI and western CAI and increase elsewhere; see e.g. Table 5.8). Using the WDPS to EDPS comparison of inferred natality to support an argument of nutritional stress in the WAI does not seem reasonable when the natality and stock trend indicators in the remainder of the WDPS suggest there is no such relationship.

Given the primary mechanism for operation of chronic nutritional stress is hypothesized as a natality decline due to prolonged weaning leading to termination of pregnancies, the most direct information of relevance would be on weaning times and both early and near-term pregnancy rates. It is unfortunate that relevant data are lacking. It is also notable that the hypothesis works at the individual physiological/behavioral level but the effect is at the population level. No consideration has been made in the hypothesis of how individuals differentially react to available and suitable prey; are the mechanisms a response to contest competition for access to specific prey items, is it younger or older females that abort pregnancies, is there a genetic predisposition, etc.?

Ultimately, the BiOp argument in support of nutritional stress, itself only an hypothesized mechanism to explain a decline in natality, comes down to the statement on page 342 that "*A reasonable explanation consistent with the pattern of natality in the western DPS relative to the eastern DPS is that the western DPS is nutritionally stressed because other hypotheses related to mechanisms associated with decreased natality (e.g., disease and contaminants) have not been supported with the available data*." This ignores problems with interpretation of data as indicators of natality, the within-WDPS population trend *versus* natality patterns and the lack of evidence from the remaining 16 indicators given in BiOp Fig. 4.25 that jointly suggest chronic nutritional stress may not be the underlying cause of continuing population decline. Failure to find an explanation elsewhere does not constitute good evidence, and certainly not proof, that nutritional stress constitutes a "*reasonable explanation*."

Notwithstanding the difficulties with acceptance of the common part of the nutritional stress hypothesis, it is necessary also to consider evidence that might suggest fishery-induced or natural drivers of that stress. Although poorly evidenced, if nutritional stress is a threat to population growth, then that threat only becomes a risk that might be treated if the exposure is such as to create a risk. In the case of natural nutritional stress it is unclear how to interpret exposure, implying that risk *per se* does not exist – it is what it is and is not treatable but could have implications for listing criteria. In the case of fisheries, exposure might be measured by consideration of i) the nature of fisheries (and hence overlap with SSL) in space and time, and ii) and the extent of fishing (and what remains for SSL).

The BiOp uses the decision tree outlined in BiOp Fig. 4.24 for this purpose, providing a framework to consider size, spatial, temporal and depth overlap between SSL and fisheries as well as "*compressed fisheries*" (i.e. degree of localization, and related to spatial and temporal overlap). The decision tree is only applied to fisheries for stocks that comprise 10% or more of the diet of SSL in an area (BiOp 3.2.2.1) as observed by frequency of occurrence. As pointed out by Bernard *et al.*(2011) and considered by the Review Panel, frequency of occurrence is a poor measure of actual diet content. Bernard *et al.*(2011) point out that the frequency is affected not just by what is eaten, but also by how many other species are eaten. Of equal concern is that digestion, and hence presence in scat samples, of identifiable parts varies by species (see e.g. Bowen, 2012, and references therein). Using frequency of occurrence can thus lead to biased estimates of diet composition. Perhaps more importantly, the use of the 10% threshold is arbitrary. In fact, all of the overlap criteria applied in the BiOp are unclear; no specific standard is developed for any of the criteria to trigger the "yes" response and no rationale is provided for the use of three such responses to trigger the next level of analysis. It appears exceptionally difficult under the scheme used, but not justified, to escape a conclusion that selected fish stocks (by area) might constitute critical habitat. This part of the exposure analysis does not form a robust and credible basis to measure exposure.

Overlap is only part of the exposure equation. In addition, the degree or extent of fishing needs to be considered. Removals and fishing rates by area are provided in the BiOp as Tables 5.3-5.7, with a useful summary at Table 5.8 showing by sub-region, FMA and RCA SSL trends, primary prey (using the 10% criterion), zonal (i.e. nm bands) fractional removals and harvest rates and biomasses by fish stock. It is evident that harvest rates for Atka mackerel and Pollock, at least at the sub-region level, are low and, given the high biomasses of these

species, the fishery removals should leave high biomasses of the species available to SSL. What is left is in fact the important issue, not what is removed. Given that SSL generally take smaller fish than fisheries this suggests a low exposure. However, care is needed in such a conclusion because the sub-region scale is large relative to SSL habitat and the wide scale information may be misleading. It is for this reason, effectively, that the BiOp sensibly ignores forage ratio (page 335) information. Overall, information on the extent of fishing is hard to interpret. In the case of Pacific Cod there is the further difficulty of interpretation caused by the use of harvest rates derived from trawl surveys rather than from stock assessment. The harvest rates for Pacific Cod therefore appear very high. Why rates derived from stock assessments were not used is unclear but is inappropriate. Superficially, harvest rates are low and sufficient fish of all species remain in the system more than adequate to meet at least SSL needs. It is unclear from information provided if forage ratios would imply sufficient food for all predators, not just SSL.

Overall, therefore, for fisheries, the exposure equation is unclear. As used in the BiOp, the nature (overlap) part of the equation is poorly constructed and leads almost inevitably to a conclusion in all cases of sufficient overlap potentially to create risk. The fishery extent part of the equation at least at the sub-region level appears to suggest little or no effective risk but interpretation is problematic because the scale of analysis is arguably too high to integrate with overlap information. Does the fishing exposure result suggest a possible risk when combined with the nutritional stress hypothesis? It is a matter of interpretation and depends on the form of logic applied. The nutritional stress hypothesis relies on conjecture and weak evidential support. The threat is therefore hard to measure. From the evidence presented, the exposure created by the fishery is low at a wide scale but unknown at a more localized scale. Bringing the threat and exposure information together suggests fishery-induced nutritional stress is not impossible (again using the BiOp standard double negative) but is not actually likely (subjectively applying the informal 60-70% criterion reflected in section 1).

5. *Reviewers will determine if there is any additional literature, assessments, or analyses that should have been considered in the BiOp (as of the end of the public comment period for the Draft BiOp, September 3, 2010).*

See also ToR 3. There are no obvious, major omissions in the BiOp. It is always possible to consider additional analyses, where data permit, but it is not clear that any would have helped in development of the BiOp. Literature cited is extensive, arguably to the point of excess, with the majority referenced but not actually described or critically analyzed. The BiOp, no doubt driven by administrative and legal considerations and requirements, appears to result from an attempt to ensure that NMFS was seen to take a 'hard look' in reaching relevant conclusions. The result of the hard look and the requirement to interpret the ESA jeopardy provision in a specific way has arguably led just to a "hard read". The size of the BiOp, and logic resulting from legal interpretations, makes for a document that is scientifically unsatisfying, to the point of causing frustration.

6. *In making these evaluations, reviewers shall consider and address the following questions:*

    a. *Are the findings of the BiOp contradicted by any scientific information available as of Sept 3, 2010 presented in, or omitted from, the BiOp?*

There is no place in the descriptive and analytical text where the BiOp is absolutely contradicted by scientific evidence. As noted in section 1, however, contradiction in the scientific context is not absolute but must rely on weight of evidence. There are places, as pointed out under specific ToR, where the weight of evidence is not as strong as suggested by the BiOp. In particular, from a scientific perspective, the conclusion that the action is *likely* to cause JAM is contradicted by the weight of evidence. From a scientific perspective, JAM is possible, but does not seem to be likely using a commonplace standard for the term as outlined in section 1. The conclusion of jeopardy being likely appears to be driven by legal interpretation rather than by science *per se*. This is not a criticism of the science; it is recognition of the context in which the science has been undertaken.

    b. *As part of this consideration, reviewers shall also assess the scientific record to determine whether adequate consideration has been given to the likelihood that factors other than fishing are negatively affecting the population status, critical habitat or recovery of the WDPS including predation, changes in the ecosystem or carrying capacity, emigration, exposure to contaminants, or other factors.*

This ToR seems to repeat ToR 2(c).

3.2 CONCLUSIONS

Much of the argument surrounding the BiOp focuses on specific issues. Notable amongst these are trends in natality; the correlation between fish stock size and Steller sea lion populations; diet quality, overlap and Steller sea lion nutritional needs; and alternative factors that might influence sea lion decline and recovery. The arguments are scientifically interesting. The bottom line, however, is that data are scarce and information poor, and unequivocal understanding of the undoubtedly complex picture is and will remain elusive. It would be possible to pick through each and every issue from a scientific perspective but ultimately the value of the science is determined not by what it can reveal so much as how it can be used in decision-making. The science itself does not determine this. Rather, the interpretation of the ESA and implementing regulations is paramount.

NMFS is clear throughout the BiOp that its opinion is driven by interpretation of the legal requirements. It is clear from its emphasis on avoiding Type II errors (false negatives) and on its reversal of commonplace logic (e.g., "*… it is also <u>not</u> possible…..to conclude that fisheries are <u>not</u> having a significant impact on the recovery of the [WDPS];* section 5.1.76) that any evidence of an effect, however weak, will possibly result in a finding that fishing could create risk to the WDPS or adversely modify critical habitat as defined.

This is perhaps best exemplified in section 5.1.7.6 with two statements: i) "*…the Agency is not required to establish a statistically significant cause-and-effect relationship under the ESA. Rather as noted above, the ESA requires the Agency to conclude that a given action is not likely to jeopardize the continued existence of a DPS or adversely modify its critical habitat.";* and ii) "*At this time with available data, it is not possible to demonstrate a statistically significant relationship between commercial fisheries on pollock, cod, Atka mackerel and arrowtooth flounder and the productivity of Steller sea lions in the western DPS. However, it is also not possible with the available data to conclude that commercial fisheries are not having a significant impact on the recovery of the western DPS of the Steller sea lion."* Constant recourse to the double negative sets an impossibly high burden of proof against NMFS opinion while allowing NMFS immense leeway to construct hypothetical arguments.

The problem with the BiOp is that while many factors are considered, it pays attention largely to detailing the limited evidence in support of a possible relationship between fisheries (as they modify the Steller sea lion prey field at different spatial scales) and a putative decline in Steller sea lion reproductive output. The BiOp pays somewhat less attention the direct statistical relationship between fisheries, prey and SSL or to other factors that could cause Steller sea lion decline or prevent recovery. Arguably, this is not critical because ultimately the BiOp does not have to balance every competing factor in the complicated sea lion story. Rather, it needs to concentrate on whether or not the action in question (the Fishery Management Plan) is likely to cause jeopardy. The other factors are of concern only if they might be of such high importance as to overwhelm fishery-induced jeopardy. Nevertheless, there is an apparent bias perhaps best exemplified by the differential descriptions of the hypotheses relating to nutritional stress – nutritional stress itself is referred to throughout as an hypothesis, the matter of fishery-induced nutritional stress is addressed at length and in a matter of fact way although conditional words frequently appear; these contrast starkly to the description of the "junk food hypothesis" as a "*notion*" in section 3.1.14.3. It is easy to misconstrue words but the single description of an hypothesis of major concern is hard to ignore.

In developing arguments, the BiOp cites extensive literature, some published, some not, but does not thoroughly analyze all available information. Analysis of so much information is all but impossible and the BiOp is in any case arguably already barely digestible. Focused omission is not unreasonable (to use a BiOp-like double negative) especially on issues that are not influenced by the action (i.e., cannot be treated), but leaves open the door to criticism. Undoubtedly, some of the criticism leveled at the BiOp falls in to this category. In this regard, the BiOp does not help in that it frequently falls back on what looks like a presumption of fishery-induced nutritional stress, and regularly uses language that often changes from suggestive to definitive within a brief segment of text. Others have already commented on this (*inter alia* Boyd, 201; Bernard et al.2010) and further examples are unnecessary. Although the BiOp is generally sound, covering most issues with reasonable thoroughness, it is therefore somewhat frustrating. Reactions to the BiOp have undoubtedly been driven by its implications, but as a reviewer I have a strong suspicion that reactions have also been in response to the nature of the BiOp.

Putting such matters aside, my general conclusion is that the BiOp is thorough, accepting that for such a large undertaking some references will have been missed and some analyses might arguably have been extended or modified. It does not seem that important information has been excluded although some interpretation of available data is debatable. Questions concerning the BiOp relate primarily not to the science *per se* but to the conclusions reached that link the science to legislation. The key issues in the BiOp are that while the WDPS is increasing overall, it is at a rate below that required for reclassification (or delisting) and within the WDPS there are sub-regions that are not increasing at all. Because SSL is listed under the ESA and especially because the WDPS population performance is less than required for reclassification (or delisting), it is necessary to provide a BiOp on Federally mandated actions (FMP). The causal factor currently (and for the past two decades) limiting population growth (*recovery* given reclassification/listing criteria) is deduced to be reduced reproductive output rather than survival. The evidence for this is primarily model based and is a matter of debate with caveats in the interpretation generally downplayed. Linking the action to natality is the hypothesis of nutritional stress. The hypothesis is quite complicated, depending on increased pup weaning times in response to nutritional stress and consequent pregnancy termination in lactating females. There is no compelling evidence for either of these; without direct measurements they remain supposition. The BiOp generally uses reduced natality as evidence of the nutritional stress hypothesis – this is circular. Even if the nutritional stress hypothesis is correct, whether it works through natural or anthropogenic channels, or both, is moot. The evidence for a natural channel cannot be dismissed but is weak. Evidence of an anthropogenic channel is equally weak. The analyses in support of the latter rely on both a threat analysis and an exposure analysis. The two effectively multiply to determine risk. The threat analysis is arbitrary and poorly determined. The exposure analysis is scale dependent. On a wide spatial scale exposure seems to be low. Evidence of local scale exposure is limited but generally inferred from low reproductive output – again, this is circular. Evidence for alternative explanations for poor population performance is limited but amongst the explanations, the possibility of killer whale predation is downplayed but not dismissed.

Overall, the BiOp is unwieldy and not compelling. It is a valiant attempt to synthesize and shoehorn an immense amount of information into a single document in order to demonstrate a hard look and support an opinion required for legal reasons. If it were not for those legal requirements, a simpler, more explicit risk analysis could have been constructed, emphasizing the most relevant information (instead of all) to comment systematically on threats, exposure and ultimately risk. Most importantly, more commonplace logic could have been applied. In a presentation to the SSC in 2011, NMFS Alaska Region Science and Research Director (DeMaster (2011); file copied to Reviewers at review Meeting) provided responses to Bernard *et al.*(2011). The response included a simple summary table of what at the time of the BiOp was known about the various possible SSL population stressors. That table encapsulates much of what is in the BiOp but lays it bare. It summarizes that at the current time direct human effects and disease are unlikely stressors, killer whale predation, inter-specific competition and contaminants are possible stressors, and that both environmental change and indirect fisheries effects are *likely*. In other words it places equal emphasis on both forms of nutritional stress. As noted above, however, the conclusion of *likely* rather than possible for the nutritional stress channels seems to be driven by legal interpretation rather than the science *per se* which merely suggests possibility. The main

point here is that the BiOp does present most issues reasonably fairly; it is just too big and too clumsy in places, apparently written in multiple phases by different authors, and ultimately driven by the requirement to consider an action and whether or not it is a risk factor that can be treated.

4 CHAPTER 2

4.1 SUMMARY OF FINDINGS BY ToR

1. *Reviewers will convene as a Panel and will conduct a scientific peer review during the panel review meeting in TBD. In addition to scientific presentations regarding the BiOp analysis and related scientific information, the meeting will include presentations by experts from environmental organizations, the fishing industry, affected communities, and other agencies and institutions. The Panel will conduct the peer review in accordance with the ToRs for Chapter 2 and consider all relevant scientific information available up to the date of the Panel meeting. Refer to Annex 3 for listing of report and background documents.*

As for Chapter 1, listed documents were read in advance, together with some additional source references. Following the Review Meeting on 1-2 August, other relevant but unlisted documents were identified and read. A number of presentations were made during the Review Meeting. A complete list of presentations for which files were provided is given in APPENDIX 1 (3). Other presentations are as indicated in the agenda (APPENDIX 3). The presentations essentially cover all information provided that might be considered new and potentially relevant.

2. *Following the same ToR identified for Chapter 1 (above), the reviewers will reexamine the Final BiOp, its scientific record and **any new information** available subsequent to the issuance of the Final BiOp and **may provide** additional commentary on the findings they made in Chapter 1 based on scientific information that arises through the **panel presentations**. This re-visitation of Chapter 1 shall be part of Chapter 2 of the report. As part of this commentary the reviewers are tasked to **reevaluate the scientific basis for the conclusions of the final 2010 BiOp, that fisheries are causing nutritional stress in Steller sea lions, which in turn is adversely impacting the survival and recovery of the WDPS of the Steller sea lion**. The reviewers shall **evaluate and comment on the strength of the relationship between fishery removals and recovery of the WDPS**.*

The presentations listed at APPENDIX 1(3) are tabulated below in the Table 1 that synthesizes information provided, whether it is new, and a note on relevance to specific ToR. That is, is there information such that the Chapter 1 comments at the ToR need to be reconsidered? The table provides a summary and the entries are not comprehensive; it is used to determine further avenues for evaluation. Areas where revaluation might be considered are highlighted. Table 2 lists the explicit comments and questions posed by the NPFMC in its presentation (*NPFMC Comments to CIE Review.pdf*) and provides comments. If comments are made that relate to issues in Table 1, these are highlighted there in bold. Table 3 lists remaining issues (not covered in Table 2) outstanding from the analysis shown in Table 1. If comments are made that relate to issues in Table 1, these are highlighted there in italics.

Table 1. Summary of presentations and analysis of what new information needs to be considered relative to ToR

PRESENTATION	INFO	NEW	Reconsider ToR?
1) ADFG WDFW BSAI FMP BiOp SSL Sea Mtg Aug 2012.pptx	Extensive	Primarily comment on BiOp as of 2010 but some new analysis of that information and some new data (alluded to in other talks).	Not explicitly. Used already in Chapter 1.
2) Bill Tweit CIE comments Aug 2.doc	Comment on Independent Review	No	NA
3) Burkanov MM protection zones in RU.pptx	Russian/Asian SSL stock structure, status, movement, protection	Unscheduled (requested) summary; no new data/information	No. Already used in Chapter 1.
4) Consolidated Industry Presentation 8.2.2012.ppt	4 separate talks, each with multiple components. Merrigan> SSL USA and Russia updates relative to listing criteria, covered also by NMFS and note ToR not commenting on criteria just status. Forage ratios – but note Ch 1 comments about interpretation/utility. P Cod, AM and Pollock biomass trends and update – note for AI P cod the difference in harvest arte ex survey (as used in BiOp) compared to ex assessment. True but at ToR 4 considering fishery exposure does the difference really matter in the BiOp judgment? Gauvin> 3 issues re AM> i) the need for a hard look at prey and feeding (diet, overlap, competition, zonal fraction usage). Note these issues effectively picked up already at ToR 4. ii) 2010 AM SA availability and use. This covered in preamble to ToR 2 but in any case can now look to 2010 and later SA (covered by NPFMC questions). iii) Use of AM tagging info for localized measure of depletion; telemetry and foraging range not appropriately used in BiOp. iv) Comment on Adaptive Management experiment poor wrt east and west of 178degrees, and on Commander Islands as potential site. Frasier> ADAK> P cod> Excellent summary of major issues related to exposure at ToR 4. Particularly emphasizes the 3-D components of overlap and MS effects as well as context of abundance (not just rates/fractions), also details on killer whale issue. Largely highlights process issues and use of reverse logic in BiOp. Covered in Chapter 1. Down> No slides> on P Cod and LL> Primarily process issues and notes points covered by other speakers.	Yes	Yes. i) ex Merrigan> **possible issue of P Cod harvest rate usage at ToR 4 and in NPFMC comment 5**. Ii) ex Gauvin> **Much covered already or in NPFMC questions.** *Need to check issue of telemetry and evidence of foraging outside 20nm esp in RCA 1,2,3.* Adaptive Management considered at Ch 2, ToR 3 and in **comment to NPFMC in text table below**. Ex Frasier> excellent, careful synthesis but primarily of relevance at Chapter 1.
5) Fadely_CIE__01Aug2012.pptx	Recent telemetry tracks and modeling of SSL	Some, but generally confirm previous results.	Interesting and good to see progress but no obvious implications for Chapter 1 considerations.
6) Gelatt_SSL_diet_presentation_for_Mitigation_meetings….pptx	Update of diet analyses and comparison of decadal patterns as measured using frequency of occurrence. Cluster Aalysis used and shows constant decadal patterns with breaks related to sub-regions. WAI and C AI show a possible increase in the proportion of Pollock in winter (depending on caveat about use of frequency of occurence).	Yes	No. No obvious change in potential fisheries exposure as tested using Fig. 4.24 and FO in any case confounds interpretation. (Potentially helpful is support of sub-region definition.)
7) HORNING-CIE-review.ppt	New paper and update since publication reporting on use of life history transmitters and inferences about survival, Holmes et al.2007 model, and	Yes. Relevant to natality evaluation and	*i) Natality at ToR 2(a).* *ii) Predation at ToR 2 (c). Note also NMFS and Oceana*

	predation – all in E GOA. Note care necessary in interpreting model suggesting natality can vary with age-structured predation because the d-d age-related predation needs some justification (it isn't obvious).	to killer whale predation.	*comments on this paper.*
8) Logerwell CIE 2012.pptx	Initial results on local area cooperative acoustic study in CAI (vessels a month apart), also aerial counts on SSL rookery and haulouts and scat sampling (using FO and a month later than latest survey)	Possibly	Not specifically. Study components separated temporally and FO in scat analysis – methodological concerns and paper not yet published. No obvious bearing on ToR evaluation **but comment at NPFMC comment 7.**
9) Lowe 2011_AI_Pcod_Pollock_Atka.pptx	Assessment updates for AI P Cod, A Mackerel and Pollock. Note assessment changes for AI Pollock in 2011 (use of GAMs to fit weight-at-age and most importantly estimation of M). Also, note that the recruitment estimate time-series shown with respect to the 1977-2011 means are potentially misleading – arguably, given the 1977regime shift, first few recruitment estimates should be ignored as they arise from the stock with age and size structure from prior conditions.	Yes, updates on species considered as main AI prey field (critical habitat).	Biomass as Prey field updates potentially relevant at ToR 4 but the changes in stock size seen are likely not significant in the general story.
10) NMML AEP CIE 1Aug 2012 ssl update.ppt	Extensive update on population trends and vital rate modeling. i) Updated counts showing continued decline in W and C AI and some rookeries in other areas (effective update of BiOp Fig. 3.10). Update on Russian trends. General picture of hole in central WDPS distribution (consistent with SSC comments). ii) Update to Holmes et al.with new data, comparison on juvenile survival estimates with Horning and Mellish, comparison of pup to non pup counts across DPS and sub-regions.	Yes	*i) Possibly at 2(a)* – **ref also SSC comment (3) on central portion of WDPS as indicative of other factors: (ToR 2(c)?** *ii) Vital rate information and modeling - Comparison to Horning and Mellish.*
11) NPFMC Comments to CIE Review.pdf	No new information provided but pointers given to SSC concerns and specific questions raised.	No	**Explicitly consider questions posed and if related to ToR reevaluate. SeeTable 2.**
12) Oceana_Steller sea lions CIE Aug1 2012.pptx	Extensive commentary on issues, including on new studies. Nearly all covered by other presentations (NMFS, Horning, Trites…). Issue of the Donut Hole raised – note Bop incudes a figure of Pollock catch but no text. Oceana ask Panel to consider split of Pribiloff Islands split from RCA.	No – useful comments (e.g. on Horning and Mellish 2012) but information, including new, is generally a summary of info presented elsewhere.	Questions re **Donut Hole (see Table 2 (3))** and *Pribiloff Islands for which relationship to ToR is unclear but possibly ToR 2(a) (noting Chapter 1 comment on spatial classification).* ii) Other info covered through primary authors, NMFS, etc.
13) Trites Atka CIE Presentation FINAL.pptx	Four pieces of info. I) New stat analysis (GEEs) on NMFS supplied data on SSL numbers and A mackerel catch rates for AI, uses lagged models – suggests +ve relationship between fishing and SSL (whereas BiOp concludes there is a –ve one). ii) (Gryba et al.2012 ms) Considers prey distribution as critical habitat using NMFS supplied trawl data to predict fish distbns. Concludes can better define dynamic CH by season etc. iii) Overview (Rosen et al) of feeding study conclusions to date. iv) A mackerel captive feeding studies (Rosen and Trites poster) and conclusion that seasonality matters – fits with conclusions at (iii) Also Traditional Knowledge (TK) quote citing Weissinger (1961). TK relevance is unclear but provides context and reminder that the past holds lessons despite a ack of systematic data collection.	Yes, variously relevant.	*Yes. i)* **relevant to** *ToR 4* **and NPFMC comment 7**. *ii) Primarily of relevance to Critical Habitat definition which is not covered in the ToR explicitly but relevant to 4 (as exposure to fishing)? Iii and iv) Relevant at ToR 4 and other places where nutrional stress is evaluated (ToR 2(a,c)).*

Table 2. Questions and comments to CIE reviewers from NPFMC, and responses

NPFMC Explicit Questions or Comments	Response
1) 2010 Atka mackerel biomass estimate was not considered in the BiOp although those data were available before the BiOp was signed. It is noteworthy that the biomass (2010) estimate was already within the range ultimately projected by the BiOp given fisheries closures in the central and western Aleutians.	See preamble to ToR 2 re BiOp timing and use of information. All updated SA and survey information is relevant. NMFS provided an update of stock assessment results to 2011(Lowe 2011_AI_Pcod_Pollock_Atka.pptx).
2) The SSLMC notes that the most current population estimate for the WDPS has climbed to approximately 77,000 animals; 52,000 in the US and 25,000 in Russia. The SSLMC notes that the WDPS continues to increase at an overall rate of approximately 1.5% per year. Accordingly, the SSLMC questions whether this population can legitimately be considered to be in jeopardy of extinction and request that the CIE review that determination.	The jeopardy determination does not relate to *extinction* but rather to *continued existence*. Although these appear equivalent experience suggests that interpretation of such terms needs to be treated with caution by reviewers. The criteria for classification and delisting are given in the 2008 Recovery Plan and have been explicitly excluded from the CIE review. This was made clear by NMFS Alaska Region staff during the 1-2 August review Meeting. Certainly, condition 1 for reclassification to "threatened" seemed in 2010 almost to be met (animals numbered close to the 2015 target and the part-decadal increase was very close to 1.5% pa, though not yet over 15 years). Classification in any case also depends on criterion 2, which is more problematic. See comments in Chapter 1, ToR 2(a). Regardless, the CIE Reviewers are not permitted to review the status determination.
3) The SSLMC notes that the populations of SSLs at the easternmost and westernmost portions of its range (Eastern Aleutians & GOA, Sea of Okhotsk, Kuril Islands) are robust and increasing. The central portion of the wDPS range (central and western Aleutians, Commander Islands) is the area of notable decreases in population. Given the localized area of population decline, it is likely that localized factors (other than chronic nutritional stress) are affecting the population recovery and growth.	The area in question is large and it is not clear what is meant by localized factors. The area is large and also surrounds the so-called Donut Hole which was subject to high fishing rates during the 1980s until closure in 1992, following onset of the well-documented regime shift in the last 1970s. It is difficult to disentangle wide-scale fishery effects from natural ones. In any case, monitoring of the Donut Hole is limited and indirect (for Pollock at least through NMFS surveys of the Bososlof area, presumed connected to the Donut hole from early Japanese tracking as well as to the Aleutian Basin). The role of the Donut Hole is also raised in the presentation by Oceana with reference to Bailey (2011). That paper notes the history of the fishery and the lack of information on the relationship fish stocks associated with the Donut Hole to other areas. It does not, however, provide insight in to SSL issues. As noted in Chapter 1, ToR 2(d), and by various Review Meeting presenters, the Commander Islands SSL decline and lack of recovery despite fishery closures does suggest that factors other than nutritional (initially acute and then chronic) stress may be important. Although existing data appear inadequate to help identify or prove alternative explanations for decline and lack of recovery of the WDPS across the North Pacific, it may be possible to include the Commander Islands within potential adaptive management approaches (see Chapter 2 ToR 3).
4) The SSLMC also notes that the Commander Islands and the western and central Aleutians have very similar population declines, despite the creation of "no fishing zones" in the Commander Islands beginning in the late 1950s, although effective management of those zones was not put into place until the 1980s (V. Burkanov, Pers comm. to SSLMC 7.16.2012). Additionally, the SSLMC notes that fishing for walleye pollock in the Aleutian Islands has been closed since 1999. This suggests that other factors, such as exposure to contaminants or disease, may be adversely affecting population recovery and growth. For example, the SSLMC recently received a paper by Castellini et al. (in press) that indicates mercury contamination in pups increases along a gradient from east to west in the Gulf of Alaska and Aleutian Islands.	See comment above re Commander Islands. The BiOp at 3.1.11.2 addresses the issue of contaminants and considers various papers by Castellini. Even if there is an east-west gradient in mercury contamination in pups, the consideration in the BiOp suggests that it likely does not affect pup survival and, as the mercury is lost by the juvenile stage and certainly by maturity, if the main life history difference along the east-west gradient is indeed weaning time and abortion rate (as in the nutritional stress hypothesis) then it is not obvious how mercury contamination would be involved. Of course, those traits are not yet supported by evidence, they are hypothesized. Note that in his summary of the BiOp in response to Bernard *et al.*(2011) and presentation to the SSC, DeMaster (2011) includes contaminants as a possible stressor and causal factor in both decline and lack of recovery. The fundamental problem with disentangling the effects of any potential stressors is that apart from predation (direct), they act sub-lethally or indirectly. Given the multitude of possibilities, spatial and temporal variations in SSL and fisheries, problems and impossibilities of experimentation, definitive determination is likely impossible.
5) The SSLMC notes that the cod harvest rate in the central and western Aleutians was exaggerated because of the use of the survey biomass estimates, rather than the total biomass estimates. As a result, the impacts from the harvest of Pacific cod in the western and central Aleutians were substantially lower than those assumed in the 2010 BiOp.	Also raised in the presentation by Merrigan in the "Consolidated Industry Presentation 8.2.2012.ppt" (see text table above). Chapter 1 ToR 4 considers BiOp Table 4.24 and the issue of the nature and extent of exposure. It is commented on there that the use of trawl surveys for Pacific Cod seems inappropriate when stock assessments are available. Use of the stock assessment-derived harvest rates instead of survey-derived may have given a slightly different "flavor" to the BiOp considerations but given the logic applied would likely not have changed conclusions reached.
6) The SSLMC received a presentation from Dr. Kerim	The BiOp includes limited information from Aydin *et al.*(2007) at Fig. 4.7. Bernard

Aydin (AFSC) that summarized results from multi-species models that indicate that large Pacific cod consume Atka mackerel almost exclusively, in some years accounting for up to 34% of total Atka mackerel mortality. The Committee questions whether restricting Pacific cod fishing in areas 543 and 542 achieves the conservation goals for Steller sea lions.	*et al.*(2011) show similar figures from Aydin (2010) indicating the possible consequences of a 10% reduction in mortality on Pacific Cod or Atka Mackerel. The results are for relatively small changes in mortality and given the number of species and linkages in the model it would be surprising to see effects on other species except where they were directly linked to the varied species. Of interest beyond these figures would be an analysis of the sensitivity of effects to changes in species linkages and relationships. Note also that the models do not take account of nutritional quality and sub-lethal effects, just direct and indirect size-structured predation consequences. They also show equilibrium changes, not short- to medium-term changes as projected from single species assessments and as relevant to the "threatened" reclassification criteria (and perhaps delisting criteria). In the case of Pacific Cod the model suggests reducing mortality would have no measurable (i.e. *appreciable*) equilibrium effect on SSL. In the case of Atka mackerel the effect on SSL might be measurable but it is small. The increases in other SSL prey species are marginal and unlikely measurable. The BiOp interprets this differently (as pointed out by Bernard *et al*, 2011) and somewhat peremptorily dismisses the use of multispecies modeling in favor of single species information (at section 8.3.2.1, with respect to FMA 543). This is perhaps not surprising given experience with multispecies modeling elsewhere (e.g. Stokes, 1992; Punt and Butterworth, 1995) which suggests that different results can ensue for even subtle reasons, making interpretation of multispecies models for management advisory purposes difficult. Based on the results of Aydin (2007, 2010) doubt is certainly cast on whether restricting fishing for Pacific Cod in FMA 543 and 542 would benefit SSL. The analysis, however, works at a fairly coarse scale and does not (cannot) take account for fine spatial details. The BiOp could make more of the multispecies results but if it were to use multispecies modeling results in place of single species ones, the debate would shift to the details of the multispecies models, leading to more questions and uncertainty.
7) The SSLMC is charged with developing alternatives for consideration in the 2012 SSL Mitigation Measures EIS. The Committee notes that alternative metrics to measure the effectiveness of those measures are needed, such as methods to estimate the biomass of SSL prey species that remain after fishing rather than relying solely on fishery removals. The SSLMC requests that the CIE review and recommend other metrics by which mitigation measures may be evaluated.	The likely importance of what is left rather than what is taken is commented on in Chapter 1, ToR 2(d) and 4, although no consideration of metrics is made. Bernard *et al.*(2011) summarize studies looking for statistical relationships between fisheries and SSL and split their consideration to pre and post 2000. It is notable that i) the majority of studies show no support for the hypothesis that fisheries negatively impact SSL, ii) the majority of studies work at large spatial scales and use fisheries catch, effort, or CPUE as explanatory variables, not measures of prey availability *per se.* As implied in the NPMFC comment, these may not be appropriate and a more useful measure would be of prey availability, for example the prey actually available to SSL given fishing within relevant spatial zones. Of the studies listed by Bernard *et al*, only that by Hui (2011), a Masters thesis, uses prey biomass as an explanatory variable in an analysis of pup and non-pup growth rates ion FMA 541-543. The study is briefly described in Trites (2012), itself a draft ms but available in the presentation "Trites Atka CIE Presentation FINAL.pptx". The methods by which available prey are derived are not described. Clearly, measuring prey availability within fine-scaled locations (10 nm bands in the case of Hui) is not straightforward and it is important not just to consider how to develop suitable measures but also how to determine and account for errors in those measures when undertaking statistical analyses. Regardless, as presented by Bernard et al, Hui's study does not suggest any significant relationship between fisheries and SSL growth rates in FMA 541-543. The study by Trites (2012), also in FMA 541-543, used GEEs and seeks to explain SSL counts or changes in counts using Atka mackerel abundance, catch and effort as explanatory variables but, importantly, also accounting for lags. The models suggested no negative relationship between fishing and SSL. [Repeating and slightly modifying some of Chapter 1, ToR 2(d) text] Given the great difficulty in developing understanding of the complex processes working at the individual SSL level (behavioral and physiological responses to environmental variables) but manifesting at the population level (e.g. in vital rate estimates), the most profitable route to determining whether the action could indeed cause JAM is to consider the relationships between fisheries and SSL. The work alluded to above and by Bernard *et al.*(2011) attempts to do that and needs to be encouraged. Such work may not reveal deep biological understanding of processes but it gets to the heart of risk analysis and potential risk treatment. In order to proceed, fine-scale and representative data are needed both on fisheries, fish stocks, and SSL. Fisheries are well monitored and spatial information on catch and effort is available. Fish stocks are monitored through surveys but only at fixed times; though they can also be monitored through fisheries. Deriving indices of local prey availability and composition throughout the year, and across years, is critical if statistical analyses are to proceed usefully; that might require enhanced localized sampling across years. Perhaps the cooperative research study reported by Logerwell "Logerwell CIE 2012.pptx" provides a potential way forward in this

	respect. Data on SSL at sufficiently fine-scale are also critical in order to allow comparison. However, while direct data on weaning times and pregnancy might be critical in evaluating the nutritional stress hypothesis (on which so much weight has been placed), data on counts and diet are the most important to allow statistical testing of relationships.

Table 3. Remaining comments on new information not covered in Table 2.

INFORMATION/ISSUE	ToR	COMMENT
1) Natality/vital rates	2(a)	Information on vital rates is included in presentations *HORNING-CIE-review.ppt* (refer to as Horning) and *NMML AEP CIE 1Aug 2012 ssl update.ppt* (refer to as NMFS). NMFS presents a summary of the Holmes (2007) model and usefully includes some summary model fits (unlike the BiOp). These are not strictly new. NMFS continues to summarize and review vital rate estimates derived from branding and LHX (including data since BiOp) and model fits. There are issues with modelling (accounting for errors, treating pup to non-pup counts as unbiased or consistently biased, etc.). Ultimately, the only new information is the estimates of survival by age showing a kick up in age 1 survival in WDPS but not in EDPS. Note that this only applies to GOA and EAI, not to WAI and CAI. The age 1 difference is noted as consistent with the maternal care/nutritional stress hypothesis but does not add significant evidence. IN BiOp terms it might be more reasonable to note that the results are not inconsistent with the hypothesis. However, the results would also not be inconsistent with alternative explanations, such as predation. Overall, the new information does not provide any cause to reevaluate comments made in Chapter 1.Horning has some new LHX data and new model that links vital rate estimation and possible predation. However, the model results need to be treated with caution because they are driven by the assumed age-structured, density-dependent consumption relationship (which is debatable). At face value, the conclusion is that predation "could" reduce effective reproductive output and that form of predation could also lead to variations in natality observed. As for NMFS, analyses are based on data from areas other than WAI. Note that differences in inferences about vital rates (between Horning and NMFS) are explicable in terms of sample biases. **Overall, there does not appear to be any compelling reason to reevaluate comments at ToR 2(a) or elsewhere. BiOp conclusions about natality are still circumspect due to potential biases in interpretation of pup to non-pup ratios and inferences about WAI are being drawn from other WDPS sub-regions with different population trajectories.** Note that the LHX work does hold out hope of better direct data through time on weaning and pregnancy; it might be possible in time with sufficient deployment to better test hypotheses.
2) Predation	2(d)	See comments at (2). The Horning modelling demonstrates under certain assumptions that predation could account for estimated variation in vital rates, but does not confirm it. Direct evidence from telemetered animals is that of 36 young (i.e. weaned) SSL in E GOA the only mortalities were due to predation away from rookeries. This confirms that predation occurs (already known) but does not of itself suggest it is the likely or overwhelming mechanism of population control. I **Overall, while interesting and suggesting the value of telemetry to understand control mechanisms, it does not substantively affect evaluation at ToR 2(d).**
3) Prey field; foraging range, critical habitat	4	Presentation "*Trites Atka CIE Presentation FINAL.pptx*" includes a summary of work by Gryba *et al.*(2012; manuscript provided to CIE reviewers) that investigates alternative definitions of critical habitat, essentially using GLMs to fit complex surfaces of prey densities (using survey data) modified using environmental variables. The authors conclude that it is possible to develop more refined CH definitions accounting for the wider information and also allowing for SSL foraging behavior. In principle this might allow more targeted management measures. The approach is tentative and unpublished and it is possible to see a number of practical difficulties relating to understanding of foraging behavior by area and the need to update management measures, possibly in real time, depending on variable prey fields. **This work is interesting and could foreshadow future approaches to targeted management but it does not currently have bearing on reevaluation of the ToR**.
4) Pribilof Islands	2(a)?	The sub-regions defined and used for classification listing do not separate the Pribilof Islands. As noted in Chapter 1 ToR 2(a) it is unclear how the sub-region categories were determined as the only reference in the BiOp is footnote 6 (section 3.1.3.2) that states the "subareas" (sic) are "*…geographically convenient but do not necessarily reflect biologically important units."* As implied at ToR 2(a), this is cause for concern and it is not clear how the classification/listing criteria are biologically meaningful. Nevertheless, consideration of classification/listing criteria and sub-region definition are not included in the CIE Review ToR. **The very specific requests made by Oceana to CIE reviewers (slides 39,41 of *Oceana_Steller sea lions CIE Aug1 2012.pptx*) are outside the scope of the review.** Whether the BiOp could or should have considered the Pribiloff Islands in greater detail is moot given spatial classification adopted elsewhere.

5) feeding studies/nutritional stress	2(a,c), 4	Presentation "*Trites Atka CIE Presentation FINAL.pptx*" also provides a summary of what is known about feeding studies and SSL. The presentation contained little new information as such, so much as synthesis and clarification. Particular emphasis is placed on new experimental feeding trials (SSL on Atka mackerel and herring) showing response of SSL to food type and seasonal availability (poster by Rosen and Trites (2012) supplied to CIE reviewers). The conclusion is that for young, particular food types for which energy density varies seasonally (e.g. Atka mackerel) in particular seasons (spring) may be inadequate (hence leading to longer weaning). Generally, the conclusions are consistent with the poor diet/nutritional stress hypothesis. However, as noted elsewhere, while consistency may support conclusions of possibility, it has little bearing on whether something is likely. For confirmation or improved support of the poor diet hypothesis, direct measurements on weaning and pregnancy are ideally required (e.g. using telemetry) on large enough samples of animals through time and/or across areas. **Overall, more interesting studies and finalized and reviewed publications may be important long-term. Currently, however, conclusions regarding the possibility or likelihood of the poor diet hypothesis remain unaffected – it is definitely possible and its likelihood is by weight of evidence at least as high as fishery-induced nutritional stress.**
6) Status	2(a)	Information on population trends for the USA and Rusia is included in presentation *NMML AEP CIE 1Aug 2012 ssl update.ppt* (refer to as NMFS). The presentation shows continued decline in WAI and CAI since 2009 (or for 2005-2009 for CAI/RCA2) as well as some individual rookery declines across the wider WDPS. Overall patterns are similar as seen e.g. through BiOp Fig. 3.10 though scales have changed. **Overall, the status update is important context and further decline in WAI and CAI is a cause for concern. There are no obvious implications, however, for BiOp findings relating to JAM.**

Considering new information (data and analyses) as presented and made available, there are no compelling reasons to adjust thinking as to the findings and conclusions of the BiOp.

3. *The Reasonable Prudent Alternative (RPA) presented in the BiOp (Section 8.3.4) and as implemented through an Interim Final Rule (75FR77535; December 13, 2010) may present an opportunity for an **adaptive management experiment** to test the response of fisheries and Steller sea lions to the fisheries closures implemented by the RPA/IFR. Reviewers will be asked to (1) **comment on the utility of this opportunity**, (2) **evaluate the metrics identified in the BiOp** (e.g., trends in Steller sea lion abundance, trends in biomass of Atka mackerel and other groundfish, etc.), and (3) **suggest other metrics** not described in the BiOp that could be used to evaluate the efficacy of the action in ensuring the groundfish fisheries are not likely to adversely affect the survival and recovery of western distinct population segment (WDPS) of the Steller sea lion.*

The ToRs for Chapter 1 do not include explicit provision to comment on the RPA. However, it is not possible to comment on the possibility of an adaptive management experiment or formal experimental design as anticipated at BiOp Section 8.3.4 without some comment on the RPA. It is stated in Section 8.3.4 that a formal experimental design will be finalized and reviewed by June 2011. No design was provided as part of the CIE review and it is unclear if one exists. In any case, given that the RPA at Section 8.3.2 are described without any apparent consideration of adaptive management it is unclear how any *post hoc* "*design*" will work. Prior to Section 8.3.4 the only mention of adaptive management is in the bullets at page 373 (Section 8.3.3) which says implementation of the RPA is "*expected to achieve…an adaptive management strategy for exploited groundfish forage species*…." The BiOp is therefore confusing with respect to intentions for adaptive management – it appears to be an afterthought.

The RPA Objective at Section 8.2.1 is clear but the Performance Standards at 8.2.2 consist of a list of considerations and specific objectives rather than any measurable standards. At the Recap of Indicators (Section 8.3.1) the two indicators at the Recovery Plan sub-region scale are trends in SSL populations and pup to adult female ratios as "*one*" indicator of birth rate. It is not specified what SSL population measure is to be used but presumably, following classification criteria, this would be non-pups. The second indicator is contentious as there are clear difficulties with interpretation of pup to non-pup ratios with considerable room for bias and misinterpretation. It is unclear what is meant by *one*. Will other indicators of birth rate also be used? It is unclear what is intended by the sub-bullets at 8.3.1; they, and the listed Evidence, effectively summarize the fundamental but contentious hypothesis of the BiOp.

Specific RPA text (e.g. 8.3.2.1 for FMA 543) lists a rationale that result in a "*require[ment]"* for specified measures. The link between the rationale and specific requirements is not clear as there is no analysis of how alternative measures could meet the specified need. In the description of the RPA there a consideration of multispecies *versus* single species issues with the conclusion drawn that measures need to be predicated on single species considerations because multispecies models amplify uncertainties. One area in which the RPA might be useful would be to test the single- and multi-species forecasts, recognizing that the time-scales for the forecasts are different. Of course, both will be wrong due to the inherent assumptions and uncertainties in each approach and the lack of consideration of issues such as the way different species distribute in space within and through years. Testing would rely on measurement at the sub-region (or ideally finer) scale the possibility of which for different species will depend on survey timing, method, frequency in relation to species-specific use of space, as well as on assessment methods able to deal with sub-regions. This appears to be recognized at 8.3.4 (relating to adaptive management experiments for WAI and CAI only) where it is commented an experimental design will be formalized but is likely to be less effective for fish than for SSL. Note that experimental design for learning about fish is not RPA-dependent. Rather, it is dependent on fish stock and fishery monitoring and assessment and could take place regardless of RPA. Certainly, areas of no fishing could help in testing single- *versus* multi-species fish complex projection if the closed areas are large enough to match feasible monitoring and modeling. The issue of testing fish projections is in any case secondary. Ultimately, whatever mechanisms are the proximate cause of differences in SSL population performance, the only real issue given classification criteria, is how SSL sub-regional and overall WDPS populations respond to differential conditions. Comparison of trends in FMA 543 as opposed to 542 and 541 will not necessarily lead to strong conclusions regarding the effects of fishing on SSL and will certainly not lead to easy conclusions on the proximate mechanisms about which there is so much debate.

In terms of the ToR, the RPA for FMA 543 and 542-541 do not appear to provide an opportunity with much utility. This is not so much because of the metrics used for SSL (population trends) so much as because of the limited ability to vary experimental treatments or to ensure comparable baselines. With respect to fish, the metrics are not made explicit but would presumably be observed or estimated biomass by sub-region. Again, utility would be compromised not so much by the potential metrics as by the unclear baseline, incompatible time-scales for valid contrast, difficulty of effective monitoring, and

limited treatment options. Given variability in fish stock biomass by sub-region and uncertainty in measurements of fish stocks, two treatments over a limited time would provide little statistical power. Overall, as stated above, the possibility of adaptive management experiments based on RPA as treatments in WAI and CAI seems to have little utility.

If a truly adaptive experimental approach were to be undertaken, it would require a consideration of what might be learned about the direct relationship between fisheries (and hence prey availability) and SSL populations. That might best be achieved by looking across all WDPS sub-regions or perhaps RCA and manipulating fishing intensity and proximity to rookeries. The power of any experiment would be increased by contemplating not just closures and constraints in areas where SSL are still in decline, but also adding pressure through increased harvest rates or proximity to rookeries in areas currently increasing. Note that on BiOp page 360 (final bullet of 8.3.1) and in the DeMaster (2011) summary, a primary reason for NMFS jeopardy determination is the SSL population response east of 178 degrees west following fishery restrictions. Selectively increasing fishing in one or more eastern areas where populations are increasing could be a useful component of an adaptive management experiment.

Comment was made in presentation *Consolidated Industry Presentation 8.2.2012.*ppt about the potential to include increased fishing at the Commander Islands as part of an adaptive management experiment. This is at first glance consistent with the comments above. Comments were also made in *Oceana_Steller sea lions CIE Aug1 2012.pptx* as to how conditions at the Commander Islands are different to the Aleutian Islands and why therefore the area might not be a good choice for inclusion within an experimental design. Any design would need to take account not just of physical and other differences between regions but also distance. It has been noted (e.g. at Table 2(3)) that patterns in SSL decline at the Commander Islands, WAI and CAI are similar. This appears to be true but even if the ultimate cause of the patterns in decline were the same (e.g. environmental forcing) it does not mean the proximate mechanisms are. It would be easier to have confidence in experimental results from areas in which the same ultimate and proximate factors are at play. This is not to say that the Commander Islands could or should not be included in any design, just that careful consideration is necessary.

4.2 CONCLUSIONS

Much of the information presented at the Review Meeting or made available was restatement of previously available materials. Of the genuinely new information, most was further analysis or updated analysis. Not surprisingly, given the short time between September 2010 and the review Meeting, logistic difficulties and high costs, there are few genuinely new data available. Overall, therefore, while “new information” provides further food for thought, there is unsurprisingly nothing that compels a reevaluation of the BiOp as of 2010. Specifically, comments in Chapter 1 ToR 4 concerning the nutritional stress hypothesis need little additional comment; the core hypothesis remains conjecture and until direct evidence (data) on weaning times and pregnancy rates (and terminations) are available, will remain so. Mounting evidence will otherwise always be through inconsistency

or consistency and somewhat subjective interpretation. The causes of nutritional stress are poor diet (not enough and/or poor quality food) or fishery-induced alterations in the prey field (not leaving enough and/or poor quality food). Disentangling these two possible drivers is fraught with difficulties. Chapter 1 concluded both drivers were *possible* though evidence suggesting either was *likely* is weak. That conclusion is subjective, using a definition of *likely* at the 60-70% level. Chapter 1 also concluded that the two drivers were about equally possible and that predation in some areas could not be ruled out as a (top down) control mechanism. There is no reason given new information to change those conclusions.

With regard to adaptive management experiments, these seem to be an afterthought in the BiOp. The RPA were not apparently developed with experimentation in mind. Given the baselines and limited treatments, there appears to be little utility as an adaptive management experiment in the RPA as proposed and implemented. A formal experimental design was due in 2011 but it is unclear if it has been finalized and reviewed. If an adaptive experimental approach is to be adopted, then consideration needs to be given not just to fishery constraint but also to adding fishery pressure in areas of current high growth.

APPENDIX 1

BIBLIOGRAPHY

1 BIBLIOGRAPHY SUPPLIED

See below at APPENDIX 2 (SOW), ANNEX 3

2 MATERIALS SUPPLIED ELECTRONICALLY IN ADVANCE

- Appendices for Final Biological Opinion- Authorization of Groundfish Fisheries under the Fishery Management Plans for Groundfish the Bering Sea and Aleutian Islands Management Area and the Gulf of Alaska.pdf
- Final Biological Opinion- Authorization of Groundfish Fisheries under the Fishery Management Plans for Groundfish the Bering Sea and Aleutian Islands Management Area and the Gulf of Alaska.pdf
- Was the decline of Steller sea lions in the Aleutian Islands from 2000 to 2009 related to the Atka mackerel fishery.pdf
- An internal review of Trites et al. 2010, NOAA_NMFS_NMML, Polar Program. February 11, 2011.pdf
- Presentation to the North Pacific Fishery Management Council of NMFS Comments on the Bernard et al. 2011 review of the 2010 biological opinion.pdf
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- Aleutian Islands pollock chapter 258 pages.pdf
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- Endangered Species Act.pdf
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- High Natality Rates of Endangered Steller Sea Lions in Kenai Fjords, Alaska and Perceptions of Population Status in the Gulf of Alaska.pdf
- Fixed Gear Marine Mammal Study, North Pacific Wildlife Consulting, LLC.pdf
- An Independent Scientific Review of the Biological Opinion (2010) of the Fisheries Management Plan for the Bering Sea_Aleutian Islands Management Areas, October 8, 2011.pdf
- Predation on an Upper Trophic Marine Predator, the Steller Sea Lion- Evaluating High Juvenile Mortality in a Density Dependent Conceptual Framework.pdf
- Prey competition between sympatric Steller sea lions (Eumetopias jubatus) and northern fur seals (Callorhinus ursinus) on Lovushki Island, Russia.pdf
- Results of Steller Sea Lion Surveys in Alaska, June-July 2011.pdf
- Section 7 Synopsis for CIE.docx
- SSL BiOp NUMBERED LISTING OF DOCUMENTS.pdf

3 LIST OF PRESENTATIONS MADE AT REVIEW MEETING, 1-2 AUGUST 2012

- ADFG WDFW BSAI FMP BiOp SSL Sea Mtg Aug 2012.pptx
- Bill Tweit CIE comments Aug 2.doc
- Burkanov MM protection zones in RU.pptx
- Consolidated Industry Presentation 8.2.2012.ppt
- Fadely_CIE__01Aug2012.pptx
- Gelatt_SSL_diet_presentation_for_Mitigation_meetings_7_9__2012tz3[2].pptx
- HORNING-CIE-review.ppt
- Logerwell CIE 2012.pptx
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- NMML AEP CIE 1Aug 2012 ssl update.ppt
- NPFMC Comments to CIE Review.pdf
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APPENDIX 2

STATEMENT OF WORK

Attachment A: Statement of Work for Dr. Kevin Stokes

External Independent Peer Review by the Center for Independent Experts

Biological Opinion on the Effects of the Federal Groundfish Fisheries and State Parallel Fisheries on listed species in Alaska, including Steller sea lions

Scope of Work and CIE Process: The National Marine Fisheries Service's (NMFS) Office of Science and Technology coordinates and manages a contract providing external expertise through the Center for Independent Experts (CIE) to conduct independent peer reviews of NMFS scientific projects. The Statement of Work (SoW) described herein was established by the NMFS Project Contact and Contracting Officer's Technical Representative (COTR), and reviewed by CIE for compliance with their policy for providing independent expertise that can provide impartial and independent peer review without conflicts of interest. CIE reviewers are selected by the CIE Steering Committee and CIE Coordination Team to conduct the independent peer review of NMFS science in compliance with the predetermined Terms of Reference (ToRs) for the peer review. Each CIE reviewer is contracted to deliver an independent peer review report to be approved by the CIE Steering Committee and the report is to be formatted with content requirements as specified in **Annex 1**. This SoW describes the work tasks and deliverables of the CIE reviewer for conducting an independent peer review of the following NMFS project. Further information on the CIE process can be obtained from www.ciereviews.org.

Project Description: NMFS Alaska Region has issued a Final Biological Opinion (November 24, 2010) under the ESA on the effects of the current fishery management regime for federal groundfish fisheries on listed species. The main listed species of concern is the endangered western distinct population segment (WDPS) of the Steller sea lion; the threatened eastern distinct population segment (EDPS) of Steller sea lions was also considered. In addition, the effects on listed humpback whales (Central Pacific and Western Pacific populations), fin whales and sperm whales were considered. The basis for the consultation is the new information available to the agency as a result of almost 10 years of intensive research on Steller sea lions in Alaska. The new information pertains to the status of the species, population and sub-regional trends in abundance, and the impacts of the existing conservation measures as well as the prosecution of the federal fisheries and the State of Alaska parallel groundfish fisheries. The focus species for this CIE review is the WDPS of the Steller sea lion.

The review will consist of two parts: (1) conducting a desk review of the Final BiOp including information available to NMFS through up until September 3, 2010 and (2) convening as a panel to peer review new scientific information (e.g. available subsequent to issuance of the Final BiOp). During the public session of the panel review meeting, presentations addressing the scope and context of the BiOp analysis and related scientific information may also be provided from experts in environmental organizations, scientific

groups, the fishing industry, and affected communities. In accordance with the predetermined terms of reference (ToRs) as specified in Annex 2, each reviewer will produce an independent peer review report consisting of two chapters: Chapter 1 will describe findings based on the desk audit of the Final Biological Opinion and will be produced prior to the public panel session; Chapter 2 will be based on the evaluation of new scientific information presented during the subsequent panel review meeting. Each reviewer report will be delivered with the two described Chapters as a single document at the end of the review process according to the scheduling of the deliverables.

Based on the ToRs for Chapter 1, each reviewer will conduct a desk review to specifically review and comment on the scientific information and interpretation that led to the rationale and subsequent findings contained in the Biological Opinion regarding factors affecting Steller sea lion population status, their critical habitat, and recovery. In particular, the desk review will include findings regarding the effects of fisheries on Steller sea lion population status, vital rates, and critical habitat. The reviewers are asked to comment on the adequacy of the best available science and of the appropriate interpretation of that science to reach the conclusions presented in the BiOp.

Based on the ToRs for Chapter 2, each reviewer shall review, evaluate, and consider the Final Biological Opinion, its findings, and scientific and commercial information made available since issuance of the Final BiOp up to the date of the panel review meeting. In addition to the peer review tasks in accordance with the ToRs for Chapter 2, reviewers may also provide additional commentary on the science included in presentations made in the public session during the panel review meeting. The Terms of Reference (ToRs) for the scientific peer review are attached in **Annex 2**.

Requirements for CIE Reviewers: Three CIE reviewers shall be provided with adequate time to conduct a thorough, impartial and independent peer review in accordance with the SoW and ToRs herein. Each CIE reviewer's duties shall not exceed a maximum of 40 days to complete all tasks of the desk peer review, participate during the panel review meeting and complete their independent peer report, as described herein. CIE reviewers shall have the expertise, background, and experience to complete an independent peer review in accordance with the SoW and ToRs. The expertise of the combined CIE reviewers should include marine fisheries management, marine fish biology, ecology and stock assessments, marine mammal population biology and foraging ecology. It is desirable that one or more of the reviewers have familiarity with the standards of the Endangered Species Act Section 7 in relation to conservation biology.

Location of Peer Review: Each reviewer shall conduct the peer review as desk review during which travel is not required and then each reviewer will participate in a panel review meeting in Seattle, Washington.

Statement of Tasks: Each CIE reviewer shall complete the following tasks in accordance with the SoW and Schedule of Milestones and Deliverables herein.

Prior to the Peer Review: Upon completion of the selection of the CIE reviewers by the CIE Steering Committee, the CIE shall provide the CIE reviewer information (full name, title, affiliation, country, address, email) to the COTR, who forwards this information to the NMFS Project Contact no later the date specified in the Schedule of Milestones and Deliverables. From the date when the selected CIE reviewer information is sent to the NMFS,

the NMFS will be provided five working days to solicit comments from the North Pacific Fisheries Management Council (Council) in regard to whether there are any conflicts of interest issues that may have been overlooked by the CIE selection process, as related to conflicts defined under the CIE conflict of interest conditions (see http://www.ciereviews.org/interest.php). After this five-day period, if there is agreement that there are no conflicts of interest issues, the NMFS Project Contact may communicate directly with the CIE reviewers in regard to all necessary peer review arrangements. The CIE Steering Committee will make the ultimate decision, based on supporting information, on the eligibility of the CIE reviewers The CIE Coordinator and COTR must be copied on all email correspondence with the CIE reviewers during the duration of the contract to ensure all contract obligations are satisfied. The CIE is responsible for providing the SoW and ToRs to the CIE reviewers. The NMFS Project Contact is responsible for providing the CIE reviewers with the background documents, reports, and other pertinent information. Any changes to the SoW or ToRs must be made through the COTR prior to the commencement of the peer review.

Pre-review Background Documents: The NMFS Project Contact will send (by electronic mail or make available at an FTP site) to the CIE reviewers the necessary background information and reports with sufficient lead time before the peer review. In other words, a desk review can begin when the necessary information is received while the necessary reports and background documents for a panel review meeting should be sent to the reviewers about two weeks before the meeting. In the case where the documents need to be mailed, the NMFS Project Contact will consult with the CIE Lead Coordinator on where to send documents. CIE reviewers are responsible only for the pre-review documents that are delivered to the reviewer in accordance with the SoW scheduled deadlines specified herein. The CIE reviewers shall read all documents in preparation for the peer review. A list of specific background documents is provided in Annex 3.

Peer Review: Each CIE reviewer shall conduct the independent peer review in accordance with the SoW and ToRs, and shall not serve in any other role unless specified herein. **Modifications to the SoW and ToRs cannot be made during the peer review and any SoW or ToRs modifications prior to the peer review shall be approved by the COTR and CIE Lead Coordinator.** The CIE Lead Coordinator can contact the Project Contact to confirm any peer review arrangements.

Panel Review Meeting: Each CIE reviewer shall conduct the independent peer review of the scientific information presented at the panel review meeting in accordance with the SoW and ToRs and shall not serve in any other role unless specified herein. **Modifications to the SoW and ToRs cannot be made during the panel review, and any SoW or ToRs modifications prior to the panel review shall be approved by the COTR and CIE Lead Coordinator.** Each CIE reviewer shall actively participate in a professional and respectful manner as a member of the meeting review panel, and their peer review tasks shall be focused on the ToRs as specified herein. The NMFS Project Contact is responsible for any facility arrangements (e.g., conference room for panel review meetings or teleconference arrangements). The NMFS Project Contact is responsible for ensuring that the Chair understands the contractual role of the CIE reviewers as specified herein. The role of the Chair during a panel review is to facilitate the scientific presentations and discussions with a focus on the ToRs. The CIE Lead Coordinator can contact the Project Contact to confirm any peer review arrangements, including the meeting facility arrangements.

Contract Deliverables - Independent CIE Peer Review Reports:

Desk review: Each CIE reviewer shall complete an independent peer review of the Final BiOp Report addressing each ToR as described in Annex 2 pertinent to Chapter 1. The desk review will be produced prior to the onset of the public panel review and each reviewer will deliver their report on Chapter 1 as a single deliverable after the panel review meeting as a single report that includes both Chapters 1 and 2.

Scientific panel review: Each CIE reviewer shall participate during the panel review meeting to conduct a scientific peer review subsequent to the desk review in accordance with the SoW. Each CIE reviewer shall complete and deliver the independent peer review report that includes Chapters 1 and 2 as separate sections of the report described herein, according to required format and content as described in Annex 1. Each CIE reviewer shall complete the independent peer review addressing each ToR as described in Annex 2 as specified for Chapter 2.

Other Tasks – Contribution to Executive Summary: In addition to each reviewer's individual peer review report, CIE reviewers will provide a brief synopsis of their desk review for compilation by the Chair into an Executive Summary (see Annex I). CIE reviewers are not required to reach a consensus. In addition the Executive Summary will list briefly the findings and conclusions reached by each panelist in accordance with the ToRs.

Specific Tasks for CIE Reviewers: The following chronological list of tasks shall be completed by each CIE reviewer in a timely manner as specified in the **Schedule of Milestones and Deliverables**.

1) Conduct necessary pre-review preparations, including the review of background material and reports provided by the NMFS Project Contact in advance of the peer review;
2) Conduct an independent peer review as a desk review described herein in accordance with the ToRs (Annex 2, Chapter 1);
3) Participate during the panel review meeting in Seattle, WA during **August 1-3, 2012** to conduct an independent peer review based on the scientific information presented during the panel review meeting in accordance with the ToRs (Annex 2, Chapter 2).
4) No later than **August 21, 2012**, each CIE reviewer shall submit an independent peer review report, including Chapters 1 and 2 in accordance with the ToRs, addressed to the "Center for Independent Experts," and sent to Mr. Manoj Shivlani, CIE Lead Coordinator, via email to shivlanim@bellsouth.net, and Dr. David Die, CIE Regional Coordinator, via email to ddie@rsmas.miami.edu. Each CIE report shall be written using the format and content requirements specified in Annex 1, and address each ToR in Annex 2.

Schedule of Milestones and Deliverables: CIE shall complete the tasks and deliverables described in this SoW in accordance with the following schedule.

June 5, 2012	CIE sends reviewer contact information to the COTR, who then sends this to the NMFS Project Contact who has 5 days to confirm there are no conflicts of interest before the contract is finalized with the reviewers.

June 13, 2012	Upon finalizing the contract, the NMFS Project Contact sends the CIE Reviewers the BiOp and background documents and begins correspondence with the reviewers.
July 5-19, 2012	Each reviewer conducts an independent scientific peer review as a desk review (Chapter 1).
August 1-3, 2012	CIE reviewers participate at the panel review meeting in Seattle WA to conduct a scientific peer review (Chapter 2)
August 21, 2012	CIE reviewers prepare and submit their independent peer review reports, including Chapters 1 and 2, to the CIE Coordinator.
September 4, 2012	After the CIE Steering Committee review process, the CIE reports with Chapters 1 and 2 are submitted to the COTR
September 7, 2012	The COTR distributes the final CIE reports to the NMFS Project Contact, AFSC Science Director, and Administrator, Alaska Region.

Modifications to the Statement of Work: Requests to modify this SoW must be made through the Contracting Officer's Technical Representative (COTR) who submits the modification for approval to the Contracting Officer at least 15 working days prior to making any permanent changes. The Contracting Officer will notify the CIE within 10 working days after receipt of all required information of the decision on substitutions. The COTR can approve changes to the milestone dates, list of pre-review documents, and Terms of Reference (ToR) of the SoW as long as the role and ability of the CIE reviewers to complete the SoW deliverable in accordance with the ToRs and deliverable schedule are not adversely impacted. The SoW and ToRs cannot be changed once the peer review has begun.

Acceptance of Deliverables: Upon review and acceptance of the CIE independent peer review reports by the CIE Lead Coordinator, Regional Coordinator, and Steering Committee, these reports shall be sent to the COTR for final approval as contract deliverables based on compliance with the SoW. As specified in the Schedule of Milestones and Deliverables, the CIE shall send via e-mail the contract deliverables (the CIE independent peer review reports) to the COTR (William Michaels, via William.Michaels@noaa.gov).

Applicable Performance Standards: The contract is successfully completed when the COTR provides final approval of the contract deliverables. The acceptance of the contract deliverables shall be based on three performance standards: (1) each CIE report shall have the format and content in accordance with Annex 1, (2) each CIE report shall address each ToR as specified in Annex 2, (3) the CIE reports shall be delivered in a timely manner as specified in the schedule of milestones and deliverables.

Distribution of Approved Deliverables: Upon notification of acceptance by the COTR, the CIE Lead Coordinator shall send via e-mail the final CIE reports in *.PDF format to the COTR. The COTR will distribute the approved CIE reports to the NMFS Project Contact and regional Center Director and will notify the Executive Director, North Pacific Fishery Management Council of availability of the report.

Support Personnel:

William Michaels, Program Manager, COTR
NMFS Office of Science and Technology
1315 East West Hwy, SSMC3, F/ST4, Silver Spring, MD 20910
William.Michaels@noaa.gov Phone: 301-427-8155

Manoj Shivlani, CIE Lead Coordinator
Northern Taiga Ventures, Inc.
10600 SW 131st Court, Miami, FL 33186
shivlanim@bellsouth.net Phone: 305-968-7136

Key Personnel:

Robert Mecum, Deputy Regional Administrator, NMFS Project Contact
NMFS, Alaska Region, 709 W.9th Street, Juneau, AK 99802
Doug.Mecum@noaa.gov Phone: 907-321-0506

Melanie Brown
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Jon Kurland, Assistant Regional Administrator
Protected Resources Division
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17109 Pt Lena Loop Road, Juneau, AK 99801
Douglas.Demaster@noaa.gov Phone: 206-399-1431

Annex 1: Format and Contents of CIE Independent Peer Review Report

1. The CIE independent report (Report) shall be prefaced with an Executive Summary providing a concise summary of the findings and recommendations.

2. The Report will include two chapters. The first chapter will be based on each reviewer's independently conducted desk review. The second chapter will be based on each reviewer's independent peer review of scientific information presented at the panel review meeting, including the evaluation of the full scientific record including scientific information available after September 3, 2010.

3. The main body of each chapter shall consist of a Background, Description of the Individual Reviewer's Role in the Review Activities, Summary of Findings for each ToR, and Conclusions and Recommendations in accordance with the Terms of Reference (ToRs).

   a. Reviewers should describe in their own words the review activities completed during the panel review meeting, including providing a brief summary of findings, of the science, conclusions, and recommendations.

   b. Reviewers should discuss their independent views on each ToR even if these were consistent with those of other panelists, and especially where there were divergent views.

   c. The CIE independent report shall be a stand-alone document for others to understand the strengths and weaknesses of the science reviewed. The CIE independent report shall be an independent peer review addressing each ToR.

4. The reviewer report shall include as separate appendices as follows:

   Appendix 1: Bibliography of materials provided for review
   Appendix 2: A copy of the CIE Statement of Work
   Appendix 3: A list of persons and organizations participating in the panel review meeting and other pertinent information from the panel review meeting.

Annex 2: Terms of Reference

Background and Context:

The purpose of this independent CIE Peer Review is to evaluate a Final Biological Opinion issued by NOAA Fisheries on November 24, 2010. The Endangered Species Act (ESA) requires NOAA Fisheries to consult with federal agencies proposing actions that may affect ESA listed species. The consultation results in a Biological Opinion (BiOp) that describes the action, reviews species biology, and makes a conclusion as to whether or not the action is likely to jeopardize the continued existence of the listed species or to adversely modify its designated critical habitat. Adverse modification is determined to occur when the direct or indirect effects of an action "appreciably diminishes the value of critical habitat for both the survival and recovery of a listed species" (FWS/NMFS 1998). The consultation process is not required to employ a "prove-disprove" or statistical evaluation process, but instead may evaluate the best available information in a "weight of evidence approach" to make a determination. The process follows the ESA statute, related regulations, and case law; with guidance to authors provided within the Endangered Species Consultation Handbook (FWS/NMFS 1998) and the Final Recovery Plan for the Eastern and Western Distinct Population Segments of Steller Sea Lion (NMFS 2008).

Tasks specific to developing Chapter 1 (conducting the desk review):

7. Read the Final BiOp (November 24, 2010) on the BSAI and GOA groundfish fisheries; and state waters parallel fisheries for groundfish fisheries and related background documents (list of documents provided is attached) and the recovery plan. Refer to Annex 3 for listing of Final BiOp report and background documents.

8. Provide a scientific peer review and comment on the final BiOp, including scientific information available to NMFS through the end of the public comment period (Sept. 3, 2010) for the Draft BiOp, evaluate the scientific information and its interpretation that developed the rationale and the subsequent findings regarding factors potentially affecting Steller sea lion population status, vital rates, critical habitat, risk of extinction, and recovery including in particular the findings regarding the effects of fisheries on Steller sea lion population status, vital rates, and critical habitat. Address the following:

    f. Does the BiOp thoroughly and accurately (i.e. using the best available scientific information) describe what is known about the status of the listed species?
    g. Does the BiOp thoroughly and accurately describe what is known about groundfish fishery practices and catch statistics under the current ongoing "status quo" action, as defined in the BiOp?
    h. While the agency is directed to evaluate the effects of the action on listed species and critical habitat, does the BiOp also adequately address alternative scientific explanations to the apparent population dynamics of the WDPS of Steller sea lion, such as (but not limited to) predation, disease, ecosystem/carrying capacity, or emigration?
    i. Does the BiOp thoroughly and accurately assess the effects (direct and indirect) of the action on the listed species and its critical habitat?
    j. Evaluate the scientific weight of the evidence presented in the BiOp. Does the evidence provide strong, moderate or weak support for the discussion, findings and conclusions made in the document?

9. Reviewers shall evaluate the quality and completeness of the scientific and commercial information used in the BiOp analysis, and identify if the BiOp analysis is comprehensive or if there are relevant scientific or commercial data or information that were not used in the BiOp analysis.

10. Reviewers are specifically asked to evaluate the scientific basis for the nutritional stress findings of the final 2010 BiOp. Reviewers shall evaluate and comment on the strength of the linkages among fish biomass estimates, fishery removals, Steller sea lion reproductive rates, and recovery of the WDPS. Does the BiOp accurately evaluate the inter-relationships between Steller sea lion population status and trends, foraging ecology, and groundfish fisheries effects across broad geographic areas (ecosystems to highly localized regions) and temporal scales (years to seasons)?

11. Reviewers will determine if there is any additional literature, assessments, or analyses that should have been considered in this BiOp (as of the end of the public comment period for the Draft BiOp, September 3, 2010).

12. In making these evaluations, reviewers shall consider and address the following questions:

    c. Are the findings of the BiOp contradicted by any scientific information available as of Sept 3, 2010 presented in, or omitted from, the BiOp?

    d. As part of this consideration, reviewers shall also assess the scientific record to determine whether adequate consideration has been given to the likelihood that factors other than fishing are negatively affecting the population status, critical habitat or recovery of the WDPS including predation, changes in the ecosystem or carrying capacity, emigration, exposure to contaminants, or other factors.

Tasks specific to Chapter 2 (panel review meeting):

4. Reviewers will convene as a Panel and will conduct a scientific peer review during the panel review meeting in TBD. In addition to scientific presentations regarding the BiOp analysis and related scientific information, the meeting will include presentations by experts from environmental organizations, the fishing industry, affected communities, and other agencies and institutions. The Panel will conduct the peer review in accordance with the ToRs for Chapter 2 and consider all relevant scientific information available up to the date of the Panel meeting. Refer to Annex 3 for listing of report and background documents.

5. Following the same ToR identified for Chapter 1 (above), the reviewers will reexamine the Final BiOp, its scientific record and any new information available subsequent to the issuance of the Final BiOp and may provide additional commentary on the findings they made in Chapter 1 based on scientific information that arises through the panel presentations. This re-visitation of Chapter 1 shall be part of Chapter 2 of the report. As part of this commentary the reviewers are tasked to reevaluate the scientific basis for

the conclusions of the final 2010 BiOp, that fisheries are causing nutritional stress in Steller sea lions, which in turn is adversely impacting the survival and recovery of the WDPS of the Steller sea lion. The reviewers shall evaluate and comment on the strength of the relationship between fishery removals and recovery of the WDPS.

6. The Reasonable Prudent Alternative (RPA) presented in the BiOp (Section 8.3.4) and as implemented through an Interim Final Rule (75FR77535; December 13, 2010) may present an opportunity for an adaptive management experiment to test the response of fisheries and Steller sea lions to the fisheries closures implemented by the RPA/IFR. Reviewers will be asked to (1) comment on the utility of this opportunity, (2) evaluate the metrics identified in the BiOp (e.g., trends in Steller sea lion abundance, trends in biomass of Atka mackerel and other groundfish, etc.), and (3) suggest other metrics not described in the BiOp that could be used to evaluate the efficacy of the action in ensuring the groundfish fisheries are not likely to adversely affect the survival and recovery of western distinct population segment (WDPS) of the Steller sea lion.

Annex 3. Listing of documents for the CIE peer review

Mandatory documents for the 'desk' review (Chapter 1):

National Marine Fisheries Service. November 2010. Final Biological Opinion: Authorization of Groundfish Fisheries under the Fishery Management Plans for Groundfish the Bering Sea and Aleutian Islands Management Area and the Gulf of Alaska. 472p + 224p. Available at: http://www.alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm

National Marine Fisheries Service. March 2008. Recovery Plan for the Steller Sea Lion: Eastern and Western Distinct Population Segments (*Eumetopias jubatus*). Revision. 325p. Available at: http://www.alaskafisheries.noaa.gov/protectedresources/stellers/recovery/sslrpfinalrev030408.pdf

L. Boyd (2010) Views expressed by Professor I.L. Boyd on the Biological Opinion Groundfish Fisheries, Bering Sea and Aleutian Islands Management Area US National Marine Fisheries Service – 8 pp. Available at: http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm

J. M. Maniscalco, A. M. Springer, and P. Parker (2010) High Natality Rates of Endangered Steller Sea Lions in Kenai Fjords, Alaska and Perceptions of Population Status in the Gulf of Alaska – 33 pp. Available at: http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm

D. Calkins (2008) Fixed Gear Marine Mammal Study, North Pacific Wildlife Consulting, LLC. NOAA Grant Number: NA07NMF4390024, April 6, 2008– 45 pp. Available at: http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm

Mandatory documents for the panel review (Chapter 2):

Bernard, D. R, S. J. Jefferies, G. Knapp, and A. W. Trites, 2011, An Independent Scientific Review of the Biological Opinion (2010) of the Fisheries Management Plan for the Bering Sea/Aleutian Islands Management Areas, October 8, 2011. 128 pp. Available at: http://wdfw.wa.gov/conservation/steller_sealions/final_fmp_biop_ind_sci_rev_08oct2011.pdf

M. Horning1 and J. E. Mellish. (2012). Predation on an Upper Trophic Marine Predator, the Steller Sea Lion: Evaluating High Juvenile Mortality in a Density Dependent Conceptual Framework. January 2012 | Volume 7 | Issue 1 | e30173. Plosone.org. 10 pages. Available at: http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm

J.N. Waite, V.N. Burkanov, and R.D. Andrews (2012). Prey competition between sympatric Steller sea lions (Eumetopias jubatus) and northern fur seals (Callorhinus ursinus) on Lovushki Island, Russia. NRC Research Press. 18 pages. Available at: http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm

Demaster, D. (2011) Memorandum for Jim Balsiger regarding Results of Steller Sea Lion Surveys in Alaska, June-July 2011, December 5, 2011, Alaska Fisheries Science Center. 18 pages, Available at:
http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm

Trites, A.W., R. Flinn, R. Joy, and B. Battaile. 2010. Was the decline of Steller sea lions in the
Aleutian Islands from 2000 to 2009 related to the Atka mackerel fishery? University of British Columbia Fisheries Centre Working Paper 2010-10. 29 pp. Available at:
http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm

Conn, P. B. (2011). An internal review of Trites et al. 2010, NOAA/NMFS/NMML, Polar Program. February 11, 2011 3 pages. Available at:
http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm

Demaster D. (2011) Presentation to the North Pacific Fishery Management Council of NMFS Comments on the Bernard et al. 2011 review of the 2010 biological opinion. 24 pages, Available at:
http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm

T. C. Y Hui. (2011). Steller Sea Lions and Fisheries: Competition at Sea? Masters Thesis University of British Columbia, March 2011. 114 pp. Available at:
http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm
http://www.afsc.noaa.gov/REFM/stocks/assessments.htm

Additional background documents:

Fisheries of the Exclusive Economic Zone off Alaska; Steller sea lion protection measures for the Bering Sea and Aleutian Islands Groundfish fisheries off Alaska. Interim Final Rule (75FR77535; December 13, 2010). 26p. http://www.fakr.noaa.gov/frules/75fr81921.pdf and http://www.fakr.noaa.gov/frules/76fr2027.pdf

Fishery Management Plan for Groundfish of the Bering Sea and Aleutian Islands Management Areas. North Pacific Fishery Management Council. November 2011. 145p. Available at: http://209.112.168.2/npfmc/PDFdocuments/fmp/BSAI/BSAI.pdf

Fishery Management Plan for Groundfish of the Gulf of Alaska. North Pacific Fishery Management Council. December 2011. 128p. Available at:
http://209.112.168.2/npfmc/PDFdocuments/fmp/GOA/GOA.pdf
North Pacific Fishery Management Council (2011) 2012 Bering Sea and Aleutian Islands Groundfish Stock Assessment and Fishery Evaluation Report. Introduction 50 pages, BSAI Pacific cod chapter: 476 pages, BSAI Atka mackerel chapter: 1156 pages. BS pollock chapter: 168 pages, Aleutian Islands pollock chapter 258 pages. Available at:
http://www.afsc.noaa.gov/REFM/stocks/assessments.htm

N. Zerbini, J. M. Waite, J. W. Durban, R. LeDuc, M. E. Dahlheim, and P. R. Wade (2007). Estimating abundance of killer whales in the nearshore waters of the Gulf of Alaska and Aleutian Islands using line-transect sampling. Mar Biol (2007) 150:1033–1045 DOI 10.1007/s00227-006-0347-8. 13 pages. Available at:
http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm

J. Durban,• D. Ellifrit, M. Dahlheim, J. Waite, C. Matkin, L. Barrett-Lennard, G. Ellis, R. Pitman, R. LeDuc, and P. Wade (2010) Photographic mark-recapture analysis of clustered mammal-eating killer whales around the Aleutian Islands and Gulf of Alaska. Mar Biol DOI 10.1007/s00227-010-1432-6. 14 pages. Available at: http://alaskafisheries.noaa.gov/protectedresources/stellers/esa/biop/final/1210.htm.

Aleutian Islands Fishery Ecosystem Plan. North Pacific Fishery Management Council. December 2007. 190p. Available at: http://www.fakr.noaa.gov/npfmc/PDFdocuments/conservation_issues/AIFEP/AIFEP12_07.pdf

2000 Endangered Species Act Section 7 Consultation Biological and Incidental take Statement. Authorization of Bering Sea/Aleutian Islands groundfish fisheries based on the Fishery Management Plan for the Bering Sea/Aleutian Islands Groundfish; and Authorization of Gulf of Alaska groundfish fisheries based on the Fishery Management Plan for Groundfish of the Gulf of Alaska. November 2000. National Marine Fisheries Service. 2000. 588p. Available at: http://www.fakr.noaa.gov/protectedresources/stellers/plb/fmp_sec07-NOV30_2000_FINAL.pdf

2001 Biological Opinion and Incidental Take Statement. October 2001. Authorization of Bering Sea/Aleutian Islands groundfish fisheries based on the Fishery Management Plan for the Bering Sea/Aleutian Islands Groundfish as modified by amendments 61 and 70; and Authorization of Gulf of Alaska groundfish fisheries based on the Fishery Management Plan for Groundfish of the Gulf of Alaska as modified by amendments 61 and 70. Parallel fisheries for pollock, Pacific cod, and Atka mackerel, as authorized by the State of Alaska within 3 nm of shore, plus selected supporting documents. National Marine Fisheries Service. 2001. 201p. Available at: http://www.fakr.noaa.gov/protectedresources/stellers/biop2002/sec7_ssl_protection_measures_final.pdf

2003 Supplement to the Endangered Species Action Section 7 Biological Opinion and Incidental take statement of October 2001, plus appendices. National Marine Fisheries Service. 2003. 183p. Available at:http://www.fakr.noaa.gov/protectedresources/stellers/biop2002/703remand.pdf

Endangered Species Act (available at: http://www.nmfs.noaa.gov/pr/pdfs/laws/esa.pdf) and implementing regulations. Available at: http://www.alaskafisheries.noaa.gov/protectedresources/esa/

Endangered Species Consultation Handbook. US Fish and Wildlife Service and the National Marine Fisheries Service. Final 1998; 315pp. Available at: http://www.fws.gov/endangered/esa-library/pdf/esa_section7_handbook.pdf

APPENDIX 3

OTHER INFORMATION

REVIEW MEETING ATTENDEES

Name	Affiliation
Jon Kurland	NMFS Alaska Region Protected Resources Division
Dana Seagars	NMFS Alaska Region Protected Resources Division
Brandee Gerke	NMFS Alaska Region Protected Resources Division
Mary Grady	NMFS Alaska Region Sustainable Fisheries Division
Mary Furuness	NMFS Alaska Region Sustainable Fisheries Division
Glenn Merrill	NMFS Alaska Region Sustainable Fisheries Division
Melanie Brown	NMFS Alaska Region Sustainable Fisheries Division
Stefanie Moreland	Sen. Murkowski Office
Larry Cotter	Chair of Steller Sea Lion Mitigation Committee
Gerry Merrigan	Member Steller Sea Lion Mitigation Committee
Dave Fraser	Member Steller Sea Lion Mitigation Committee
John Gauvin	Member Steller Sea Lion Mitigation Committee
Todd Loomis	Member Steller Sea Lion Mitigation Committee
Kenny Downs	Member Steller Sea Lion Mitigation Committee
Nicole Kimball	Alaska Dept. of Fish and Game
Doug Vincent-Lang	Alaska Dept. of Fish and Game
Doug Demaster	Alaska Fisheries Science Center Director
Jim Balsiger	NMFS Alaska Region Administrator
Jim Iannelli	Alaska Fisheries Science Center
Lowell Fritz	National Marine Mammal Laboratory
Brian Fadely	National Marine Mammal Laboratory
Tom Gelatt	National Marine Mammal Laboratory
Tonya Zepplin	National Marine Mammal Laboratory
Libby Logerwell	Alaska Fisheries Science Center
Sandra Lowe	Alaska Fisheries Science Center
Pat Livingston	Alaska Fisheries Science Center and Chair of Scientific and Statistical Committe
Dave Fluharty	CIE Panel Review Chair and University of Washington
Kevin Stokes	Consultant, CIE expert
Don Bowen	Bedford Inst. Of Oceanography, CIE expert
Brent Stewart	Hubbs Sea World Institute, CIE Expert
Glenn Reed	Fishing Industry
Donna Parker	Fishing Industry
Tom Gemmell	Consultant
David Bernard	Consultant
Andrew Trites	University of British Columbia
Shannon Atkinson	UAF
Marcus Horning	Oregon State University
Steve MacLean	North Pacific Fishery Management Council
Dave Benton	Consultant
Paul McGregor	Fishing Industry
Vladimir Burkanov	Russian SSL Researcher
John Lepore	NOAA General Counsel
Susanne McDermott	Alaska Fisheries Science Center
Jeremy Sterling	National Marine Mammal Laboratory

Brian Bataille	University of British Columbia
Mike Levine	Oceana
John Warrenchuk	Oceana
Merrick Burden	Marine Conservation Alliance
Matt	Fishing Industry
Bill Tweit	North Pacific Fishery Management Council and Washington Dept. of Fish and Wildlife
Katie Sweeney	National Marine Mammal Laboratory
Steve Ignell	Alaska Fisheries Science Center
Stephanie Madsen	At Sea Processors Association
Steve Barbeaux	Alaska Fisheries Science Center
Frank Kelty	Dutch Harbor

DRAFT AGENDA 7/10/12

Center for Independent Experts Panel Review Meeting for the
Review of the 2010 Biological Opinion on the Effects of the
Alaska Groundfish Fisheries on Steller Sea Lions and Other Endangered Species

Seattle, Washington
August 1-2, 2012

David Fluharty, Ph.D., Meeting Chair

August 1, 2012

9:00 – 9:10 Welcome and introductions (Dave Fluharty)

9:10 – 9:30 Purpose of the meeting, overview of the CIE Review and Terms of Reference (Dave Fluharty)

9:30 – 12:00 Presentations by Alaska Fisheries Science Center

SSL counts, telemetry data, food habits (Tom Gelatt)
[Break]
Stock assessment updates for SSL prey (Sandra Lowe, Steve Barbeaux, Grant Thompson)
Spatial distribution and abundance of SSL prey (Libby Logerwell)

12:00 – 1:00 Lunch

1:00 – 3:00 Presentations by the States of Alaska and Washington

1. Introductory summary (Doug Vincent-Lang and Bill Tweit)
2. AK/WA science review panel findings (Dave Bernard and Andrew Trites)
3. Update on additional, recent data and research results (Doug Vincent-Lang)
4. Concluding summary (Doug Vincent-Lang and Bill Tweit)

3:00 – 3:15 Break

3:15 – 4:30 Presentations by the North Pacific Fishery Management Council (Steve MacLean)

1. Review of Council comments regarding development of RPA
2. Review of SSC comments on available science and analysis
3. Council views on need for additional information
4. Council concern about the level of information that must exist to support a link between natality, nutritional stress, and fisheries interactions
5. Any new information identified by the SSL Mitigation Committee to date

August 2, 2012

9:00 – 9:10 Welcome and introductions (Dave Fluharty)

9:10 – 9:20 Structure for presentations, consistent with the Terms of Reference (Dave Fluharty)

9:20 – 11:30 Fishing Industry Presentations

1. Gerry Merrigan, Fisherman and former NPFMC member
2. John Gauvin, Scientific advisor to trawl industry participants
3. Dave Fraser, Longtime Aleutian Islands fisherman

[Break]

4. Kenny Down, Representative of the freezer longliner fleet
5. Todd Loomis, Director of government affairs for Ocean Peace, Inc.

The industry panel will provide perspectives regarding the scientific analysis used in the BiOp, the operational characteristics of Aleutian Island fisheries and their interaction with SSL and critical habitat, the management measures adopted pursuant to the 2010 SSL Biological Opinion, and possible alternative management measures or adaptive management experiments.

11:30 – 12:30 Lunch

12:30 – 1:30 Jon Warrenchuk, Oceana (also on behalf of Ocean Conservancy and Greenpeace)

The presentation will address new information since 2010 and whether such information affects the analysis or conclusions of the BiOp, including a discussion of updated stock assessment and trends of SSL prey prey abundance, and recent relevant publications.

1:30 – 2:15 Markus Horning, Oregon State University

1. Update contemporary survival rate estimates for the eastern Gulf of Alaska region from Horning & Mellish, PLoS ONE 2012 (Chapter 2 mandatory document that presented results based on 12 mortalities detected via implanted telemetry transmitters in juvenile SSL from Nov. 2005 through Nov. 2011) with data based on 16 mortalities detected through June 30, 2012.
2. Update our contemporary regional (eastern GoA) predation estimate to at least 14 predation events in 16 detected mortalities (we previously reported at least 11 in 12).
3. Clarify the intent and applicability of the density dependent SSL population conceptual model we presented in the referenced PLoS ONE paper. The intent of this conceptual model is not to make inferences on causes of the past population trajectories of western SSL. The intent is to highlight linkages between the hypothesized, age-structured and density-dependent predation and vital rates including survival, female recruitment, and pup production.

4. Present an additional output from this conceptual model that pertains to the use of pup to non-pup ratios (P/nP) from surveys to make inferences on natality (birth rates), as applied by the NMFS to the western Aleutian Islands. Our conceptual model suggests that P/nP can be substantially depressed even with constant natality for declining populations under high predation pressure.

2:15 – 2:30 Break

2:15 – 3:00 Andrew Trites, University of British Columbia

1. An update of Trites et al. (2010) that includes additional data and analyses that addressed review comments received from NMFS.
2. Results of ongoing proximate analysis of Atka mackerel that addresses the nutritional quality of this prey species relative to the energetic requirements of Steller sea lions.
3. Predicted biomass of Atka mackerel, Pacific cod and walleye pollock available to Steller sea lions in the western Aleutians relative to the designated critical habitats (from Gryba et al. 2012).

3:00 – 3:45 Shannon Atkinson, University of Alaska Fairbanks

Summarize results from recent feeding trials (Calkins et al. 2012) demonstrating juvenile SSL experienced rapid growth on pollock diets in fall and spring: 1) measurement of average daily mass gain or loss, 2) measurement of average daily intake, 3) proximate analysis of the pollock diet, and 4) assessment of body composition. The results are not consistent with existing published mechanisms regarding digestive capacity of growing (juvenile) SSLs (Rosen and Trites 2000, 2004). Further, the ability to understand the feeding ecology of SSLs and the associated dietary implications is greatly aided by protocols well developed in the animal science literature and we propose directions that this line of work should pursue. In particular, information on the proximate analysis of diets of different prey species and their implications on SSL bioenergetics is likely to be of considerable importance to managing for the recovery of this ESA listed species.

3:45 – 4:30 Final questions from the CIE reviewers for any of the presenters