Center for Independent Experts Independent Peer Review of the November 2010 North Pacific Groundfish Fishery Biological Opinion

CIE Independent Peer Review Report

by

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Executive Summary

In November 2010, the National Marine Fishery Service (NMFS) released a Biological Opinion (BiOp), prepared in compliance with section 7 of the Federal Endangered Species Act (ESA). The BiOp documents the consultation on the effects of the authorization of groundfish fisheries in the Bering Sea and Aleutian Islands region (BSAI) and in the Gulf of Alaska (GOA), including parallel groundfish fisheries in Alaska state waters on both the western and the eastern distinct population segments of Steller sea lions (SSL, *Eumetopias jubatus*). The objective of this formal consultation was to determine if the groundfish fisheries in the BSAI, GOA and the State of Alaska are likely to jeopardize the continued existence of SSL and are likely to destroy or adversely modify designated critical habitat.

This report constitutes my scientific review of the findings and conclusions contained in the November 2010 BiOp for the Center of Independent Experts. Chapter 1 reports my findings from a desktop review of the BiOp and Chapter 2 incorporates new information gather during a panel review meeting that I attended in Seattle, WA in August 2012.

The five terms of reference specified in the Statement of Work (Appendix 2) are listed below. Although these are addressed separately in each chapter of the report, I have combined my findings in the Executive Summary.

Overall conclusion

It is my conclusion that the BiOp fails to provide reasonable support for the conclusion that continued fishing for Alaska pollock (*Theragra chalcogramma*), Pacific cod (*Gadus macrocephalus*) and Atka mackerel (*Pleurogrammus monopterygius*) in the BSAI and the GOA is likely to jeopardize the survival or adversely modify critical habitat (JAM) of the western population of SSL. There is no direct evidence that by removing fish, these fisheries compete with SSL in the central and western Aleutians and elsewhere. Harvest rates for Atka mackerel are too low and the fraction of the Pacific cod stock in these areas is too small for a fishery on these species to result in nutritional stress. The only indirect evidence advanced to support the effects of fishing on SSL is the suggestion of reduced natality, based on pup to non-pup ratios. It is not known if these ratios are a reliable proxy for natality. Reduced natality is speculated to result from nutritional stress cause by the removal of groundfish by fishing. In my opinion, the weigh-of-evidence argument for JAM rests on speculation of what is thought possible rather than what is supported by scientific evidence.

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?*

The BiOp reviews relevant scientific information on the biology and status of SSL. However, in my opinion, the BiOp is marred by a lack of critical evaluation of research findings that underpin many of the statements about our knowledge of SSL biology, environmental variability, and hypothesized fisheries effects, that in turn are used as evidence in the exposure analysis to evaluate the likelihood that various stressors have or continue to negatively impact SSL demography.

The description of regional trends in the western population is consistent with previous analyses. Non-pup survey data from 2000-2008 indicates that all sub-areas have increased or fluctuated without trend with the exception of the western and the western-part of the central Aleutian sites (BiOp Fig. 3.7). Additional surveys of pups in 2011 and non-pups in 2012 confirm the overall trends presented in the BiOp.

Pups counts are not used in trend analysis, but they are used to estimate pup/non-pup ratios and those ratios have been used to make inferences about natality rates. It is not known if such ratios provide a reliable proxy for natality rate. However, the BiOp gives considerable weight to pup/non-pup ratios in concluding JAM. Without an understanding of the uncertainty associated with these ratios, and how those uncertainties might vary across the populations range, it is difficult to place much confidence in inferences about natality based on pup/non-pup ratios.

There is a degree of consistency in the results from population modelling studies used to infer changes in vital rates over time. However, these studies are not independent as they all use the same SSL count data as input to the models. Therefore, consistency among models is not a measure of confidence in the conclusions. Nevertheless, model estimates of the temporal patterns of juvenile and adult survival are confirmed by independent estimates from longitudinal, mark-resighting studies.

There is little information on natality rates of SSL. Estimates from observations of individual marked females at one site, Chiswell Island, differ from model-based estimates for the adjacent central GOA. Given the importance of estimates of natality to the weight of evidence, further research is needed to resolve the apparent difference.

Much has been learned about the spatial distribution of SSL from the use of satellite tags. Nevertheless, I was disappointed that there was so little quantitative analysis and synthesis presented in the BiOp, given the importance of movement and distribution to the assessment of JAM. Attempts to use the Platforms of Opportunity data to obtain a better understanding of the foraging distribution of SSL are based on questionable assumptions that need to be tested before the results of this work can be accepted.

The metric used to describe the diets of SSL is frequency of occurrence (FO) – well known as the least informative estimate of what is consumed. Experimental studies show that estimates of diet composition are biased by the effects of digestion of prey hard parts. Without correction for the effects of digestion, FO tends to systematically over-estimate prey species with robust hard parts (e.g., gadoids like pollock) and under-estimate those prey species without or with fragile hard parts. I conclude that the identification of both important and principal prey species of SSL is flawed as no attempt has been made to correct for known biases associated with the use of FO as a measure of importance. It is my opinion that the estimates of SSL diet composition reported in the BiOp cannot be considered accurate.

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?*

Chapter 2 and Chapter 4 (Section 4.3) of the BiOp provide a detailed description of the fisheries, management strategies, and the procedures used to assess stock status and to provide harvest advice. Also discussed are the various mitigation measures for pollock, Atka mackerel, and Pacific cod fisheries which have been taken to avoid JAM. In my opinion, the BiOp presents a reasonably thorough and accurate account of these fisheries.

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?*

The BiOp (Chapter 4) discusses a number of alternative hypotheses that could have contributed to the large-scale declines of SSL and that could currently be influencing the complex dynamics of the western population. None of these is given the weight afforded to the fisheries competition hypothesis (see below).

The first of the alternative hypotheses is the influence of regime shifts and environmental change on SSL. The conclusion of the BiOp about the effects of regime shifts on upper-trophic level predators does not appear to be based on a critical evaluation of the evidence and is at times contradictory. For example, on the one hand the BiOp concludes that environmental changes ought to have little effect on SSL, but on the other the BiOp states that *"The carrying capacity of the North Pacific for Steller sea lions likely fluctuates in response to changes in the environment* …" The BiOp goes on the state that it is *"unclear whether this environmental variability and the associated diet shifts were outside the limits of natural variability in the history of Steller sea lions in the North Pacific and were principal factors in their population decline."* [BiOp 156] I would argue that it is not only unclear, but simply unknown. However, it would seem reasonable to presume that over the course of their evolutionary history of several million years, SSL might have experienced quite a broad range of environmental conditions in the North Pacific.

Potential impacts of killer whale (*Orcinus orca*) predation on SSL trends are discussed in the Section 4.2.3. Based on new research, it is now clear that there are sufficient numbers of transient killer whales, whose distribution overlaps that of SSL, to exert significant predation mortality on SSL. Several modelling studies have shown that it is feasible for killer whale predation to have significant population-level effects on SSL (Barrett-Lennard *et al.* 1995; Springer *et al.* 2003; Williams *et al.* 2004). Progress toward obtaining better estimates of killer whale diets has been made using fatty acids and stable isotopes (e.g., Herman *et al.* 2005, Krahn *et al.* 2007). The development of a LHX tag also promises another approach to estimating predation mortality on SSL (Horning and Mellish 2012). Despite substantial progress, we are still some distance from having sufficient data to estimate how killer whale predation mortality might vary throughout the western population of SSL. The effect of killer whale predation on the dynamics of SSL remains unknown.

The effects of nutritional stress on SSL are discussed in Section 3.1.14. The text is organized around studies in the 1980s, a period of rapid decline, and 1990s a period of slower decline. Nutritional stress resulting from inadequate prey might be caused environmental change, inter-specific competition, foraging responses to risk of predation, lack of appropriate food (junk-food), fisheries removals or combinations of these factors.

The BiOp summarizes the evidence for 32 potential responses of SSL to nutritional stress in Table 3.17. These 32 responses basically represent a finer-scale breakdown of the 17 responses listed in Fig. 4.25 (the response framework) along with reduced survival responses. As noted by Bernard *et al.* (2011), Table 3.17 is hardly referred to in evaluating the evidence for nutritional stress, but inspection of the Table, reveals that the only evidence of nutritional stress is the inferred reduction in natality. Other responses that were judged to have occurred (such as changing in survival) could have been caused by other factors (e.g., predation). Therefore, I conclude that there is little evidence that SSL experienced nutritional stress in the past and essentially no evidence that SSL are currently experiencing nutritional stress.

The BiOp summarizes the section on nutritional stress by stating that *"The general conclusion from these physiological studies comparing the eastern and western DPS during the 1990s has been that nutritional stress was not evident in adult females or pups from the western DPS. Whether this was due to inherent biases in the study design or other confounding factors is not known."* [BiOp 115] I believe that poor experimental design and confounding factors probably made it difficult to test this hypothesis. But the fact remains, no evidence for nutritional stress is evident from multiple studies. I disagree with the conclusion which states *"…while other demographic evidence (Holmes et al. 2007, Holmes and York 2003, Fay 2004, Fay and Punt 2006) suggests a lingering chronic impact…"*. As I have noted previously, the evidence for this *"lingering chronic impact"* is weak. Furthermore, reduced natality during a period of high juvenile survival seems unexpected as it implies that relatively inexperienced juveniles are foraging more successfully than experienced adult females.

Another food-stress hypothesis, referred to as the junk-food hypothesis (Rosen and Trites 2000, Trites and Donnelly 2003), is discussed in Section 3.1.14.3 of the BiOp. This hypothesis states that SSL can experience too much of the wrong kind (i.e., low energy density) of food resulting in negative effects on growth and survival. Over time this hypothesis has been refined, based on extensive experimental studies on captive SSL (review by Rosen 2009, Castellini 2002, Calkins *et al.* 2005). The current hypothesis is that adult SSL can compensate for lower energy prey by increasing the amount of food they eat, whereas animals < 1 year old, due to a smaller stomach capacity, cannot satisfy their energy requirement if they consume only low-energy density prey (Rosen 2009). Although the experimental evidence indicates that too much low caloric-density food can be detrimental to young SSL (< 1 year old), we simply do not know if the consumption of such "junk food" has influenced the dynamics of SSL. Furthermore, given that young SSL are dependent on milk for much of the first year of life, the junk-food hypothesis would not seem to have much relevance as an explanation for nutritional stress in SSL.

Inter-specific competition could also reduce prey availability to SSL. As noted in the BiOp, Section 4.2.4, there are a number of fish and mammals species that might compete with SSL for prey, particularly the very abundant Arrowtooth flounder. Despite the opportunity for inter-specific competition provided by other predators, no evidence is presented in the BiOp that competition has or is continuing to have negative effects on SSL abundance.

Finally, there seems general agreement that diseases, parasites, and contaminants are unlikely to have played a significant role in causing SSL to decline. I agree with this assessment.

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

Direct mortality from entanglement in fishing gear and shooting were important sources of mortality prior to the 1990s, after which these sources of mortality were largely eliminated. The account of direct mortality in the BiOp appears to be both thorough and accurate.

Indirect effects of fishing could be caused by harvesting of SSL prey species or could be mediated through ecosystem effects on species interactions affecting SSL food availability. Several approaches are used in the BiOp to evaluate the effect of fishing on SSL prey. One approach was to calculate a foraging ratio and to compare forage ratios in areas fished with trends in SSL. However, when forage ratios did not support the assumed effect, the BiOp dismisses their value. In doing so, the BiOp leaves the impression that the metric simply did not capture the effect that must be there since the SSL population is declining. An alternative conclusion is that a high forage ratio in an area of SSL decline suggests that food is not limiting. This alternative was not considered in the BiOp.

Another approach used the observation that SSL populations were stable or increasing where fishing for Atka mackerel was prohibited within critical habitat, but continued to decline in areas where fishing was permitted. However, in my opinion, this coincidence cannot reasonably be interpreted as evidence without controlling for other factors that might accounted for the observed differences (such as predation).

A third approach tested for negative correlations between fishing variables, such as catch and effort, and trends in SSL counts. These analyses are central to conclusions about the likelihood that continued fishing may cause JAM. A number of studies have attempted to test the "fishery-effects" hypothesis using correlation analysis. Many, but not all, of these studies are referenced in the BiOp, but nowhere in the document is there a comprehensive review of what these studies have found. The lack of critical review of these studies is concerning given their importance to the findings and conclusions of the BiOp.

The BiOp (p 237-238) largely dismisses these studies as uninformative. An independent panel conducted a scientific review of all 10 studies (Bernard *et al.* 2011). Although a few significant negative relationships were found prior to 2000, none were found after 2000. Several studies specifically tested for relationships in the Aleutian Islands (areas 541-543) where SSL continue to decline (Trites *et al.* 2010, AFSC 2010, Hui 2011). These studies failed to detect significant negative relationships. Bernard *et al.* (2011) noted that the studies published after 2000 had a longer time series of data and therefore had more statistical power to detect significant relationships.

Although these studies are not truly independent (they all use the same SSL count database and similar fisheries catch and effort data), they do represent the best test of the hypothesis that fisheries negatively affect SSL trends. On the basis of my review of these studies and the independent review of Bernard *et al.* (2011), I conclude that these studies provide no evidence for the hypothesized negative effects of fishing. All three approaches used in the BiOp to establish evidence for the effects of fishing on SSL have failed to provide convincing 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?

The BiOp marshalls diverse sources of information to construct a weight-of-evidence argument for the likelihood that fisheries for Alaska pollock, Pacific cod, and Atka mackerel jeopardize or adversely modify critical habitat of the western population of SSL. A summary of the weight of evidence for JAM is given in the Working Model of Effects, Section 4.7.2, and again in Section 7.4.3 of the BiOp. It is my opinion that the weigh of evidence as presented provides little support for the findings and conclusions of the BiOp.

Figure 4.24 of the BiOp summarizes the decision framework that NMFS used to determine if SSL where exposed to the Action (i.e., effects of commercial fishing), whereas Figure 4.25 summarizes the potential responses of SSL and critical habitat exposed to the effects of commercial fishing. These two decision trees appear to serve as the basis for constructing the weight-of-evidence for JAM. I found this framework incomplete, ambiguous, and also rarely referred to in the BiOp.

The first decision point is to determine if there is direct interaction with the fisheries of concern. This is determined on the basis that the fished species accounts for >10% FO of the SSL diet. Thus, the first decision is made from an unreliable estimate of the contribution of prey species to the energy requirements of SSL. The next decision points evaluate exposure of SSL habitat (food) to the fisheries based on overlap. The BiOp evaluates overlap qualitatively (yes, no), however, quantitative analyses to support decisions about exposure overlap of SSL habitat to fisheries would have been more informative.

The BiOp then goes on to evaluate the exposure of SSL to the Action. At this stage in Figure 4.24, there are five habitat responses and four exposure variables to consider. Each of these appeared to be scored qualitatively, but I could not determine how these SSL response and exposure variables were evaluated with respect to the decision framework.

Localized depletion of prey is one means by which fisheries are thought to negatively affect SSL. Evidence for the localized depletion of prey is discussed in Section 4.5.3.3. Overall, it is difficult to conclude much from the experiments on Atka mackerel, Pacific cod and pollock. The results appear to depend on the spatial scale of fishing, fish movement, and the exploitation rate. These findings are neither unexpected nor do they tells us much about how SSL might be affected by fishing. The BiOp concludes that:

> *The potential impact of competition with fisheries, through a reduction in the biomass and quality of Steller sea lion prey species, is a highly debated topic among the scientific community. .... The effect of fisheries on the distribution, abundance, and age structure of the Steller sea lion prey field, at the spatial scale of foraging Steller sea lions and over short and long temporal scales, is largely unknown.* [BiOp 259]

Despite this clear and, in my opinion, accurate statement, the BiOp then concludes that fisheries are likely to jeopardize and adversely modify critical habitat of SSL.

Section 5.1.5 of the BiOp presents evidence for the responses of SSL to reduced prey availability caused by the proposed action (i.e., fishing), but similar, if not the same, responses would be expected if prey availability were reduced for other reasons (for example see Bowen *et al.* 2001, NMFS 2008). Although Fig. 4.25 is introduced as the framework for evaluation of SSL responses to reduced prey availability, it is not referred again. Therefore, it is not clear if the framework was used. As most of the text in Chapter 5 of the BiOp has little to do with the evaluation of evidence for food limitation, I conclude that the framework was not used in any meaningful sense.

The BiOp summarizes the weigh of evidence in a "Working model of effects on SSL". This working model acknowledges that multiple extrinsic factors have influenced the dynamics of SSL over the past 40 years. In my opinion, the working model basically concludes that we are no closer to understanding why the western stock of SSL declined so rapidly during the 1980s and 1990s, continues to decline in part of the central and western Aleutian Islands, and has increased over the remainder of the stock's geographic range.

Reasonable and prudent alternatives

Chapter 8 describes the RPAs proposed to remove jeopardy and summarizes the scientific evidence leading to the determination of jeopardy. To justify the development of the RPA, the BiOp carries forward 10 lines of evidence (BiOp p 359-360). Although I have concerns about each of these, the fourth line of evidence is remarkable, in so far as it states that as no other mechanism (stressor or combination of stressors) has been identified, then prey removals (i.e., fishing) which **could** (my emphasis) result in chronic nutritional stress **could** be a factor underlying the declines. The BiOp states that "…dozens of field and captive Steller sea lion studies…" provide support for prey removals causing chronic nutritional stress. Having read the BiOp, I am unaware of such field studies and note that the captive studies show only that calorie restriction causes loss of body mass and condition.

Overall, I conclude that there is no evidence for the hypothesized indirect effects of the identified fisheries on the availability of food to SSL. As this is the only hypothesized effect on SSL, there is no reason to expect that the RPAs proposed for management areas 541-543, which will reduce or eliminate fisheries for Atka mackerel and Pacific cod, would have positive effects on SSL population trends in those areas.

Conclusions and Recommendations

It is my opinion that findings and conclusions of fisheries effects on SSL are not supported by the evidence presented in the BiOp and that reviewed at the panel meeting in Seattle. There is no direct evidence that by removing fish, these fisheries compete with the western population of SSL in the central and western Aleutians or elsewhere. The only indirect evidence advanced to support the effects of fishing on SSL is the suggestion of reduced natality as a result of nutritional stress, also noted by Boyd (2010a). Thus, the weigh-of-evidence argument for JAM rests on speculation.

I found that distilling the evidence for the weight-of-evidence argument for JAM was difficult given the sheer length and redundant nature of the text. Therefore, I **recommend** that future BiOps be limited to 250 pages with relevant figures and tables included in the text. Length does not strengthen the case for or against the determination of JAM.

In evaluating scientific evidence, it is important to be explicit about the assumptions and sources of uncertainty associated with the data. In other words, there needs to be a critical and unbiased evaluation of the evidence. To a large extent, the BiOp fails in this regard.

The metric used (i.e., FO) to estimate importance of SSL prey in the diet is known to be unreliable, overestimating prey with robust hard parts (e.g., Pacific cod) and underestimating those with fragile ones (e.g., salmons, herring). Yet throughout the BiOp, FO estimates of importance are used without qualification. As a consequence, it is almost certain that conclusions about the species composition of SSL diets are seriously biased and that the importance of pollock, Pacific cod and Atka mackerel as prey of SSL have been overestimated. I **recommend** that NMFS explore better ways (e.g., Tollit *et al.* 2003, 2007) to express the diet of SSL to reduce, in so far as possible, the biases caused by the partial and complete digestion of prey hard parts. Further research on the use of quantitative fatty acid and prey-DNA methods might also prove valuable.

Along with diet, the spatial ecology of SSL is central to understanding how SSL response to environmental variability and to anthropogenic stressors. Nevertheless, although hundreds of SSL that have been satellite tagged, a synthetic, quantitative analysis of SSL movement data has yet to be completed. I **recommend** that a quantitative analysis be conducted with the twin objectives of providing the best description of the use of space by the SSL population and guiding future movement studies.

Research aimed at extracting more information from SSL counts should be encouraged. However, it is important to gain a better understanding of the statistical properties of counts and ratios derived from those counts. The BiOp makes strong inferences from the ratio of pups to non-pups, for example about natality, without any indication of the sampling error associated with such ratios or the sources of bias and how those biases might change over space and time. As population monitoring is at the core of the SSL program, I **recommend** research to better understand the statistical properties of SSL count data and to test the hypothesis that pup to non-pup ratios are a reasonable proxy for natality. Efforts by NMFS to establish programs in the AI to estimate vital rates are important. Given the importance attached to inferences about changes in vital rates (e.g., natality) over space and time, I **recommend** the initiation and continuation of long-term mark-resighting research to empirically estimate vital rates in several regions of the western population.

Initial results from LHX tags indicate that it should be possible to obtain independent estimates of survival rates and to make strong inferences on the sources of mortality in free-ranging SSL. These vital rate data could be quite important testing alternative hypotheses about the factors influencing SSL dynamics. Therefore, I **recommend** further development of the LHX tag and implantation techniques to permit it to be used throughout the range of SSL. Efforts to enhance the tag to estimate the reproductive status of females should also be pursued.

Experience over the last several decades, involving quite considerable field effort, has shown that without some form of experimentation we have little hope of rigorously testing the "competitive-interaction" or alternative hypotheses that have been advanced. Attempting to test alternative hypotheses using multiple response variables has been largely uninformative because so many of the response variables are predicted to respond in a similar way under different hypotheses (see Bowen *et al.* 2001, NMFS 2008, BiOp Table 3.17). Without experimentation of some form, speculative qualitative statements, made weaker by the frequent use of "likely", "could", and "possibly", will be continue to form the weigh of evidence for fishery effects.

Continued increases of SSL counts in many regions will raise the quite reasonable question as to when mitigation measure might be relaxed or removed in these areas. Therefore, I **recommend** that NMFS give serious consideration to a field experiment whereby mitigation measures are selectively removed from some areas that are stable or increasing. SSL counts and estimates of vital rates could be used to test the impact of removing measures. Such an experiment will not be easy, will take time, and will require careful planning in terms of statistical power and confounding variables (see below). Nevertheless, now that several regions are experiencing sustained population growth, it may be time to revisit the value of conducting a field experiment.

Multiple factors undoubtedly were responsible for causing the decline of SSL. Direct mortality from entanglement in fishing gear and shooting were important sources of mortality prior to the 1990s, after which these sources of mortality were largely eliminated. Environmental change has had significant impacts on the structure and functioning of the ecosystems that support SSL and could have affected the food available to SSL. There are several hypotheses for how those changes in food availability might have negatively affected SSL, but we do not know if they actually occurred. We also know that transient killer whales eat SSL and that there are sufficient numbers of transients to have population-level effects on SSL. We do not know if killer whale predation occurred at a rate sufficient to account for some of the observed declines. Although we do not what role these factors have played in the declines of SSL, it will be important to attempt to account for these factors if the decision is made to conduct field experiments to further test the competitive fishery-interaction hypothesis.

CHAPTER 1

Background

In November 2010, the National Marine Fishery Service (NMFS) released a Biological Opinion (BiOp), prepared in compliance with section 7 of the Federal Endangered Species Act (ESA) (NMFS 2010). The BiOp documents the consultation on the effects of the authorization of groundfish fisheries in the Bering Sea and Aleutian Islands region (BSAI) and in the Gulf of Alaska (GOA), including parallel groundfish fisheries in Alaska state waters on both the western and the eastern distinct population segments of Steller sea lions (SSL, *Eumetopias jubatus*). The objective of this formal consultation was to determine if the groundfish fisheries in the BSAI, GOA and the State of Alaska are likely to jeopardize the continued existence of SSL and/or are likely to destroy or adversely modify designated critical habitat.

Description of the Individual Reviewer's Role in the Review Activities

This report constitutes my independent scientific review of the November 2010 BiOp for the Center of Independent Experts. The report has two chapters. The first reporting my desktop review of the evidence presented in the BiOp for jeopardy and adverse modification of critical habitat (JAM) of SLL. Appendix 1 lists the bibliography of documents cited in my review, some of which are also listed in Annex 3 of the Statement of Work (Appendix 2). The second chapter incorporates new information gather during a panel review meeting that I attended in Seattle, WA on 1-2 August, 2012 and from scientific publications that have become available subsequent to release of the BiOp.

The Terms of Reference for both chapters of the review are provided in Annex 2 of the Statement of Work (Appendix 2). Appendices 3 and 4, respectively, list the persons and organizations participating in the panel review meeting and the meeting agenda.

Summary of Findings

The BiOp brings together extensive information of the biology of Western Distinct Population Segment (WDPS) of SSL. Throughout the report I use SSL to refer to this population segment unless otherwise stated. The BiOp also describes the ecosystems supporting this population, and potential and realized threats to the population, including commercial fishing for groundfish. On the basis of this information, the BiOp concludes that ongoing fisheries for Alaska pollock (*Theragra chalcogramma*), Pacific cod (*Gadus macrocephalus*) and Atka mackerel (*Pleurogrammus monopterygius*) are likely to jeopardize the survival and adversely modify critical habitat of SSL.

While accepting the challenges faced by NMFS in attempting to understand the factors influencing the dynamics of this population segment and the need to give the benefit of the doubt to the endangered species, it is my opinion that the conclusion of JAM is not supported by the weight of evidence presented in the BiOp.

Understanding the population dynamics of any pinniped species is challenging and attempting to determine the factors responsible for the observed changes in SSL numbers is pushing the ecological envelope quite hard. Nevertheless, in my opinion, the BiOp is marred by a lack of critical evaluation of research findings that underpin many of the statements about our knowledge of the biology and population dynamics of SSL, environmental variability, and hypothesized fisheries effects, which in turn are used as evidence in the exposure analysis to evaluate the likelihood that various stressors have or continue to negatively impact SSL demography. Results are often stated without drawing attention to the nature of sampling, associated assumptions, and details of the methodology used. On the whole, the weight-of-evidence argument developed in the BiOp is weakly supported by qualitative statements rather than quantitative syntheses of the available data.

Notwithstanding requirements under the ESA, from the point of view of this reviewer, many sections of the BiOp are poorly organized, repetitive, and contain text that appears to have little relevance to the determination of JAM. I strongly recommend that NMFS reconsider their approach to developing future BiOps. I have not commented on sections that serve mainly to provide background information on the biology of SSL, and the biology of Alaska pollock, Pacific cod and Atka mackerel and their associated fisheries. I believe this background text fairly represents current understanding of those topics. My review focuses on topics pertaining more directly to the determination of JAM of SSL.

Terms of Reference

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?

Chapters 3 and 4 of the BiOp review the extensive research that has been conducted by NMFS and others to better understand the biology of SSL. In each of the sections below I evaluate the extent to which the "*best available scientific information*" has been used in the BiOp.

Population size and trends

SSL populations are monitored by conducting counts (pups ~ 1 month of age on rookeries and older animals on rookeries and haulouts) during the breeding season at a large number of consistently surveyed sites (BiOp Section 3.3.3). Similar approaches are commonly used to monitor pinniped population trends elsewhere. There have been some changes in the methods used to obtain counts (Snyder *et al.* 2001, Fritz and Stinchcomb 2005, Fritz *et al.*, 2008), but the new methods appear to have been calibrated against the old so that the adjusted time series of counts should be consistent over time. Nevertheless, as noted in the BiOp *"Estimating trend by analysis of individual haulouts … focuses attention on a single haulout which is known to be highly variable (much more so than a rookery) rather than the region in question. Coefficients of variation associated with sub-area non-pup totals range between 5-15% (NMFS, unpublished data)."* Although not stated explicitly, it appears that in the analysis of trends, the coefficient of variation and bias in the counts are assumed to be constant over time and space and are not used to estimate the uncertainty in the associated trends. NMFS should consider additional research to test assumptions about the time- and space-invariance of measurement error and bias.

The description of the regional trends in the western DPS is consistent with previous analyses. Non-pup survey data from 2000-2008 indicate that all sub-areas have increased or fluctuated without trend with the exception of the western and the western-part of the central Aleutian sites (BiOp Fig. 3.7).

The BiOp also presents an alternative approach to the estimation SSL trends based on the observation by Fritz *et al.* (2008) that smaller scale trends within region were lost when counts were aggregated to the entire region. They divided the population into 11 Rookery Cluster Areas (RCAs) to better reveal smaller-scale patterns. Region boundaries were chosen based on similarities in population trends of sea lions at sites within each region, and bottom trawl survey strata that were used in the creation of spatially explicit groundfish biomass time series (AFSC 2010). The rationale for dividing the population in this way seems clear, but it is not clear if the data actually support this approach as no further details are available. The variability in counts at individual sites does reveal just how dynamic counts can be which may provide insight into movements that are obscured by the RCA approach. Nevertheless, conclusions about overall trends in the abundance of non-pups do not appear to be dependent on the approach used.

Pups have also been counted on rookeries, but less frequently than non-pups. Pup counts are considered less reliable because pups are more difficult to count and because not all pups will have been born at the time of the survey (surveys are timed to minimize this effect). Although pups counts are not used in trend analysis, they are used to estimate pup/non-pup ratios and those ratios have been used to making inferences about vital rates (e.g., BiOp 84-85). Therefore, the reader needs to know more about the measurement error and possible sources of bias associated with pup counts and the estimated ratios. Those ratios have been used to draw conclusions about changes in natality over time (Holmes *et al.* 2007, see below) and over space. For example, *"Pup to non-pup ratios based on data collected in 2009 are consistent with the interpretation that natality rates of western DPS Steller sea lions are lower than those in southeast Alaska (DeMaster et al. 2009)"* [BiOp 85]. However, before we can use these observations to make inferences about natality, we must ask if there are other explanations for differences in pup to non-pup ratios. One such difference might be maternal foraging trip duration which could alter the ratio independently of natality. Another is suggested in the BiOp, *"The extent to which sub-adult males and other weaned juveniles haul out on rookeries will also affect pup to non-pup ratios and can vary between rookeries independent of differences in natality."* Therefore, without an understanding of the uncertainty associated with pup/non-pups ratios, and how those uncertainties might vary across the populations range, it is difficult to place much confidence in inferences about natality rates based on pup/non-pup ratios. The determination of JAM rests heavily on inferred changes in natality from pup to non-pup ratios.

Vital rates and population modelling

An understanding of how the vital rates (i.e., birth and survival) change over time and space provides insight into the mechanisms underlying population change. However, without additional information, changes in vital rates, per se, tell us little about the underlying causes of those changes. The BiOp reviews modelling studies and independent analyses of sighting data of marked individuals that have been conducted to estimate vital rates (BiOp Section 3.1.4). Although conclusions from recent modelling studies (Holmes and York 2003, Holmes *et al.* 2007) would be strengthened by a better understanding of the precision and accuracy of pup/non-pup ratios, the temporal pattern of change in estimated juvenile survival from those studies is similar to that determined from independent mark-resighting data (Pendleton *et al.* 2006), lending support to the conclusion that juvenile survival has changed over time. Similarly, Pendleton *et al.* (2006) found that adult survival during the 1990s and early 2000s was greater than in the 1970s (York 1994). Together these changes in survival have been mainly responsible for the decline and subsequent increase in the population.

Research on the vital rates of SSL over the past several decades has generated some impressive results. Nevertheless, as noted in the BiOp *"… information about changes in survival of adult and juvenile Steller sea lions in Alaska applies only to the populations breeding from southeast Alaska (eastern DPS) through the eastern Aleutian Islands (170°W) in the western DPS. There is no information specific to the declining Steller sea lion populations breeding in the central and western Aleutians regarding temporal changes in survival."* [BiOp 88] This clear statement about the limitations of research findings is welcome. Similar statements should have been used more frequently throughout the BiOp.

Changes in body growth and reproduction are used as evidence to support hypotheses concerning food limitation as the mechanism underlying population declines. Therefore it is important to determine if there is good evidence for such changes. Section 3.1.4.2 reviews evidence for changes in late-term pregnancy rates (a proxy for birth rate) and body growth. Reproductive rates are estimated from shot samples of adult females taken form the Gulf of Alaska, from age-structured population models, and from inter-annual observations of marked females. Conclusions about historical changes in reproductive rates rest heavily on two small shot samples of adult females taken the 1970s and 1980s from the Gulf of Alaska (Pitcher *et al.* 1998). Of these, only 30 and 42, respectively, were sampled during late gestation (April and May). Based on these data, late-term pregnancy rates were low, but did not differ significantly between samples, as noted in the BiOp. These data also suggested that lactating females in the 1980s had lower pregnancy rates than did lactating females from the 1970s, taken as evidence of reduced nutrition. However, this difference was not significant ($p=0.06$), or at best provided only weak support for there having been a real difference between decades, but the BiOp concludes throughout that the pregnancy rate of lactating females was significantly lower.

Age-structured models have also been used to estimate reproductive rates of SSL. These models are fit to time series of non-pup counts, pup to non-pup ratios, age structure (Winship and Trites 2006; Fay and Punt 2006) and, in the case of Holmes and York (2003) and Holmes *et al.* (2007), also to estimates of the juvenile fraction of the non-pup counts. Despite the importance of the modelling studies in providing an insight into how vital rates changed during the decline, the BiOp provides little detail on the structure, assumptions, and results of these models. The BiOp summarizes the modelling results by stating that they:

> *… suggest that declines in reproductive performance of females in the western DPS continued into the 1990s in some or major parts of the Alaskan range (Holmes and York 2003, Fay 2004, Fay and Punt 2006; Holmes et al. 2007), but may have increased in the late 1990s and 2000s in some areas (Winship and Trites 2006). Holmes et al. (2007) estimated that natality rates in the central GOA were 36% lower in the period 1998-2004 than in the mid- 1970s (Figure 3.16). In addition, they present time series of pup and non-pup counts in the western GOA and eastern Aleutians that suggest that reductions in natality between the mid-1970s and 2005 may be more widespread within the western DPS than the detailed modeling of the central GOA population indicated.* [BiOp 89]

The approach used in the Holmes analyses is an innovative attempt to extract more information from the count time series, however, the juvenile fraction of the non-pup counts are point estimates without any indication of how representative they might be of the true juvenile fraction in the population. This is important because, as noted in their paper, the use of these juvenile estimates strongly influence the inferences drawn from the analysis. It is also not clear from the BiOp that Holmes *et al.* (2007) represents, in some sense, a global analysis as the matrices of survival and fecundity assumptions explored in the other studies were also investigated by Holmes using the longer time series of counts from 1976-2004. They found that model fits indicated a steady decline in the per capita natality in the central Gulf of Alaska from the late 1970s through 2004, while survivorships of adults and juveniles initially declined, but since the late 1980s to early 1990s both increased. As noted in the Holmes *et al.* (2007) paper, for natality to be steadily declining in the face of increasing survival seems an unexpected result. Thus, independent estimates of natality are needed to confirm the model results. Although fitting to SSL pup and non-pup counts produces estimates of survival that are consistent with independent estimates from the resighting of branded individuals, there are reasons to expect that pup to non-pup ratios are not good proxy for natality. For example, it seems unlikely that the same proportion of non-pups would be hauled out at all sites. Differences in foraging behaviour (diet, distance to foraging locations) and age structure could affect this ratio independent of natality rate.

Although there is a degree of consistency in the results from modelling studies, it is important to remember that they are *not independent*, as noted in the BiOp (p95), as they all use the same SSL count data as input to the models and therefore are subject to similar effects of measure error and bias in those counts. Further research is needed to better understand the accuracy and precision of SSL count data.

Model-based estimates of natality can be compared to estimates from a longitudinal study of marked females at the Chiswell Island, in the eastern Gulf of Alaska. Remote video cameras were installed at Chiswell Island in 1999 to study the reproductive performance of individually identified SSL females. Parker *et al.* (2008) used these observations to estimate pupping site fidelity in multiparous females. They reported that multiparous females exhibited pupping site fidelity 37% of the time. However, the take home message for me was not that site-fidelity occurs sometimes, but that most females that gave birth on Chiswell Island did not show fine-scale site fidelity. This presumably has implications for the estimation of natality.

Maniscalco *et al.* (2010) extended the Parker study by recording the reproductive histories of 151 individually marked females that met criteria for maturity and sightings in at least two years. Estimated natality, from a mark-resighting model, was 69.2% for all years combined. This estimate is higher that the model estimates (43%) for the central Gulf of Alaska for a slightly earlier time period (Holmes *et al.* 2007). A previous draft of the Maniscalco paper was reviewed by Holmes (2009, unpublished) and by Johnson (2009, unpublished). In both cases, problems were identified and re-analysis suggested a lower natality rate than found by Maniscalco *et al.* (2010). Holmes found that the Maniscalco analysis suffered from pseudo-replication by using year rather than female as the sampling unit and failed to account for trailing zeros, both errors would tend to overestimate natality. Her re-analysis yielded a natality estimate of 52% rather than 69%. Johnson also reanalyzed the data with a Bayesian hierarchical model that produced an estimate of average natality of 53% with 95% credible limits of 44% to 61%. The Maniscalco draft manuscript was not available and there is insufficient detail in the published paper to determine if the concerns of Holmes were addressed.

It is not clear which of the estimates of natality from the Chiswell data is correct at this point. This does need to be sorted as conclusions in the BiOp with respect to continuing nutritional stress rest heavily on model estimates of depressed natality and low pup to non-pup ratios. I have questions concerning the selection criteria for including a female in the Maniscalco study. For example, they state that "*Identified females were considered for this analysis if they were present on the Chiswell Island rookery during the pupping and breeding season…"*. Thus, it is not clear how they treated females that had been sighting in a previous year but where not observed in a subsequent year(s), i.e. trailing zeros identified by Holmes. If data from these absent females were ignored, their estimate may represent a biased sample of high quality females and perhaps older females that are more likely to have distinguishing marks. Finally, without data from other sites, there is no way to know how representative Chiswell Island might be of other rookeries in the Gulf of Alaska and elsewhere.

Movement and foraging

The way in which SSL use the marine environment is presumably influenced by food availability, intra- and inter-specific competition for food, the risk of predation, and intrinsic factors such as age, sex, and reproductive status. Habitat use and foraging ecology might also be influenced by human activities such as fishing.

The BiOp qualitatively summarizes (BiOp 97-100) what has been learned about the movements and distribution of SSL from telemetry studies. The conclusions from telemetry studies are that *"… available data suggest two types of distribution at sea by Steller sea lions: 1) less than 20 km from rookeries and haulout sites for adult females with pups, pups, and juveniles, and 2) much larger areas (greater than 20 km) where these and other animals may range to find optimal foraging conditions once they are no longer tied to rookeries and haulout sites for nursing and reproduction."* [BiOp 96]. This conclusion, however, needs to be qualified with the observation, noted in the BiOp, that few adult females have been studied and that most of what we know about SSL movements comes from studies of dependent pups and juveniles. Given the importance of these data to assessing overlap with fisheries, I was disappointed by the lack of a quantitative synthesis of the results on movement and distribution, particularly with respect to juveniles. The qualitative description may well be an accurate reflection of the data, but there is no way to judge this from the presentation in the BiOp.

Limited information on the foraging ecology of adult females, as noted in the BiOp, is a noteworthy gap, but nowhere in the BiOp could I find out how many females had been studied (apparently 29 older than 3 years of age, Boor and Small 2012). The BiOp acknowledges that there is much to learn about adult female foraging behaviour. At the same time, it is not clear how additional data on female foraging, or for that matter other sex and age classes, would be used to explicitly test alternative hypotheses about the factors affecting SSL demography. Therefore, careful thought will be needed in planning further movement studies of adult females and other components of the population.

To attempt to overcome the lack of information on the distribution of sea lions, Boor and Small (2012) (based on Boor 2010) analyzed opportunistic sightings of SSL in the Platforms of Opportunity (POP) database. The Boor and Small model provided estimates of effort-corrected SSL encounter rates using an effort index. This "platformday" effort index assumed that *on average* the platform-day index corresponds to a fairly consistent quantity of true effort. This is an important assumption which has not been tested. Nevertheless, the BiOp concludes that:

> *A recent analysis of opportunistic sightings of Steller sea lions (the Platforms of Opportunity, or POP database) yielded results consistent with the limited telemetry information available for the western and central Aleutian Islands.* [BiOp 96]

and that:

> *NMFS assumes at-sea use inferred from telemetry and POP information summarized in AFSC (2010b) and Boor (2010) reflect the at-sea use of Steller sea lions in the respective regions as they are the best data available.* [BiOp 98]

Both statements are problematic. The limited data in the first quote refer to only 3 juvenile males that were tagged elsewhere and moved into RCA 1and 2 for part of the period tracked. With regard to the second quote, while it is true that areas used by SSL, as indicated by satellite telemetry, overlap with those identified in the Boor analysis, the predicted spatial distributions and areas of high use are quite different. In fact, Boor and Small conclude that *"… our findings contradict the dominant picture of Steller sea lion use-patterns derived from telemetry studies…".* The BiOp appears to accept the Boor analysis at face value, although noting that lack of POP data in some areas and seasons confounds inferences about the use of space by SSL. The idea of attempting to extract information of the spatial distribution of SSL from the POP data is an interesting one. However, I have a number of concerns, including the one noted in the BiOp about the lack of coverage in some areas. The platform-day index that forms the basis of the Boor and Small (2012) analysis [Boor's thesis was not available to this reviewer] assumes that there is some coverage over the area of interest. It also assumes that, on average, SSL were observed if present even though there is no way of knowing the actual effort or competency of the observers (ranging from those with little training to those highly trained). Observer bias is suggested given that almost 70% of the sightings of mammals where of more than one individual, perhaps indicating that larger groups where more likely to the sighted. Finally, while acknowledging that this in an innovative approach, the method has not been validated and therefore it seems premature accept the results in the determination of jeopardy. One approach to validation might be to use effort-based, multi-species marine mammal survey series to test the method.

Diet

Section 3.1.8 starts out with the questionable use of the term “preference”. Preference implies selection of certain foods over others based on certain prey characteristics. We know little about the diet preferences of SSL or most other pinnipeds. Steller sea lions are generalist predators that eat a variety of fishes and cephalopods distributed from nearshore demersal to epi-pelagic habitats, but how they make decisions about what to eat is largely unknown.

The discussion of diet would have been more valuable had it been organized along ecological lines such as changes over time and space, seasonal variability, demographic influences, and prey availability and if more attention had been given to the quality of the estimates (i.e., sources of variability and bias). The lack of attention to quality is particularly troubling as the most common metric used to describe the SSL diet (see BiOp Table 3.14) is frequency of occurrence (FO) – well known as the least informative estimate of what is consumed. Experimental studies show that estimates of diet composition are biased by the effects of digestion of prey hard parts (e.g., Tollit *et al.* 2007). SSL are noted for being particularly “hard” on their foods such that few diagnostic hard parts survive digestion (Tollit *et al.* 2007). Therefore, an “all-structures” approach to identifying SSL prey has been used, but this approach will not eliminate bias due to the erosion of prey hard parts. Without correction for the effects of digestion, FO tends to systematically over-estimate prey species with robust hard parts (e.g., gadoids like pollock) and under-estimate those prey species without or with fragile hard parts. The diet of SSL often contains a mixture of prey species, some with robust hard parts and others without. Therefore, FO will provide a biased view of the diet even if the population has been representatively sampled.

For the most part, we can never know if we have a representative sample from a wild population. We attempt to sample over the species range and at different times of the year to make the sample as representative as logistics and money permit. This has been done rather well in the case of SSL, but there are still gaps from the most remote parts of the population’s range. However, once the sample has been collected, the method used to express the importance of prey species can play a large role in our impressions of what species are most important. This is critical in the context of the BiOp and the determination of JAM.

While recognizing the difficulties and added cost (both time and money) associated with attempting to reduce known sources of bias, it is my opinion that the estimates of SSL diet composition reported in the BiOp cannot be considered accurate. It is quite likely that the importance some species has been under-estimates and others, such as Alaska pollock and Pacific cod (both with robust otoliths) over-estimated. It is not possible to estimate the magnitude of the bias in the context of this review, but the experimental studies with captive SSL could be used to investigate this (Tollit *et al.* 2003, 2007).

The BiOp also concludes that *"Steller sea lions are opportunistic predators which rely on seasonal aggregations of prey resources in predictable locations and quantities …."* [BiOp 199]. Concluding that SSL are opportunistic predators is an explicit statement about the foraging strategy exhibited by this species. However, this foraging strategy is not defined in the BiOp. How does an opportunistic predator forage? My criticism might be considered pedantic if it were not for the strong implications these words carry for how SSL forage and thus how they might be affected by a change in either the species composition of the prey field, its abundance, or spatial distribution. The fact is we have relatively little understanding of the overall foraging strategy of SSL or how this strategy is realized through the foraging tactics used by individuals.

Marine Critical Habitat

Based on estimates of diets, the BiOp identifies the following list of important prey species: Walleye pollock, Atka mackerel, Pacific cod, Arrowtooth flounder (*Atheresthes stomias*), rockfish, herring (*Clupea pallasi*), capelin (*Mallotus villosus*), sand lance (*Ammodytes hexapterus*), other forage fish, squid, and octopus. I might have expected to see salmon also listed here, as they appear to be seasonally important in several areas and are undoubtedly underestimated by FO. Although the above species and species-groups are listed as important prey species, only four species are considered "principal" prey species: Atka mackerel, walleye pollock, Pacific cod, and Arrowtooth flounder.

I have several concerns here. The first is that FO is not a reliable basis for determining the energy contribution of prey species to the diet. In fact, there is recognition in the BiOp that FO is not a good measure of diet as indicated by the following:

> *Inferences on the relative importance of prey to Steller sea lions using the occurrence in scat data is misleading, as dietary value is determined by biomass consumed and the energy content of that fish (at the time it was taken). … therefore, number or occurrence is not necessarily the best indicator of dietary value and may underestimate the importance of larger, or more energy rich prey.* [BiOp 235]

The second concern relates to 10% threshold as the basis for identifying the principal prey species, also noted in the review by Bernard *et al.* (2011). Sinclair and Zeppelin (2002) paper is cited as the source for this criterion, but the 10% criterion is not mentioned in the paper. Although the 10% rule is arbitrary, even if we accept it as reasonable, it is not clear how only four species were identified as principal prey as salmons and Irish lord (*Hemilepidotus* sp) would also qualify as principal prey and Arrowtooth flounder would not (see range-wide totals, Table 1 in Sinclair and Zeppelin).

I conclude that the basis for the identification of both important and principal prey species of SSL is flawed as no attempt has been made to correct for known biases associated with the use of FO as a measure of importance. All four species identified as principal prey are subject to commercial fishing and it could be argued that an assessment of their abundance and distribution would be warranted even if they were not identified as principal prey. However, highlighting those species as principal prey serves to draw attention away form other prey species that may be as important in the diet of SSL. Thus, the identification of important prey species carries a very sizable measure of doubt, which is not expressed in the BiOp.

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?

Chapter 2 and Chapter 4 (Section 4.3) of the BiOp provide a detailed description of the fisheries, management strategies, and the procedures used to assess stock status and to provide harvest advice. Also discussed are the various mitigation measures applied to the pollock, Atka mackerel, and Pacific cod fisheries which have been previously taken to avoid JAM of Steller sea lions. In my opinion, the BiOp presents a thorough and accurate account of these fisheries.

The status of the three groundfish species is also described in Section 4.2.5 of the BiOp under the heading of important SSL prey resources. Included in this section, for the first time, is a discussion of Pacific herring. There is no rationale given for the inclusion of herring in this section. Pacific herring is identified as an important prey but, so too are salmons, which are not discussed.

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?

The BiOp (Chapter 4) discusses alternative hypotheses that could have contributed to the large-scale declines of SSL and that could currently be influencing the complex dynamics of the western population, in which most areas are stable or increasing while others continue to decline.

Regime shift and environmental change

Chapter 4 begins with a description of ecosystem dynamics in the Action Area. This section does a good job in characterizing seasonal, interannual and longer-term changes, such as regime shifts, in the ecosystems supporting SSL. However, the Section ends with the argument that the effects of regime shifts on upper-trophic level predators should be modest. The BiOp cites two papers in support of this conclusion. Pascual and Adkison (1994) found that of the hypotheses they simulated, long-term or catastrophic change in environmental conditions affecting juvenile survival was most likely to have caused the decline. This finding does not support the text of the BiOp. The second paper compared SSL with three other species "*in similar variable environments*" and concluded since only SSL had declined and the other species also experienced variable environments, some cause of than environmental change must be responsible for the SSL declines (Shima *et al.* 2000). Perhaps this is an interesting observation, but it is hardly a test of the hypothesis the upper-trophic level predators are generally not affected by environmental variability. Also, the BiOp failed to critically review the analysis of the possible effects of regime shifts and climate variability on the dynamics of SSL (Trites *et al.* 2007). The conclusions of that paper are quite relevant to the issue of alternative scientific explanations for the decline of SSL.

The BiOp's conclusion about the weak effects of regime shifts on upper-trophic level predators seems unwarranted. In fact, this conclusion is contradicted by the brief section on changes in the carrying capacity of SSL which states that *"The carrying capacity of the North Pacific for Steller sea lions likely fluctuates in response to changes in the environment* …" Most ecologists would agree with this statement, but the BiOp goes on the state that it is *"unclear whether this environmental variability and the associated diet shifts were outside the limits of natural variability in the history of Steller sea lions in the North Pacific and were principal factors in their population decline."* [BiOp 156] I would argue that it is not only unclear, but simply unknown. However, it would seem reasonable to presume that over the course of their evolutionary history of several million years, SSL might have experienced quite a broad range of environmental conditions in the North Pacific.

The BiOp then states that:

> *… the magnitude of the change to the North Pacific ecosystem caused by the 1976-77 regime shift is thought to be larger than previously experienced by Steller sea lions during the 1900s. If it were within the normal range and Steller sea lions have a high likelihood of occasionally declining more than 80%, modeling suggests that they would have likely gone extinct given their life history characteristics (NMFS 2008a).* [BiOp 259]

I find it difficult to penetrate this text. The first sentence is clear, but how the first sentence relates to the second sentence and what the reader is to take from this passage is not clear. This text is followed by:

> *It is likely that although oceanographic and atmospheric conditions have changed over the last several decades, those changes have not been outside the range of natural fluctuation previously experienced by Steller sea lions. …Thus the potential impact of environmental variability on recovery in the near term is minimal.* [BiOp 259]
>
> *Fishing could have exacerbated the regime shift related impacts through relatively high local harvest rates of Steller sea lion prey species, increasing their foraging costs. …. It is likely that environmental change, coupled with fishery impacts, affected Steller sea lion at the population level during the decline and is currently a stressor to consider in Chapter 5.* [BiOp 259]

The logic here escapes me. The effects of environmental change are minimal, but together, undocumented fishery-effects and environmental change are currently stressors on SSL. How can this be and what is the evidence? There is none.

Killer whale predation hypothesis

Potential impacts of killer whale (*Orcinus orca*) predation on SSL are discussed in the Section 4.2.3. Over the past decade, considerably more has been learned about the abundance and distribution of killer whales in the BSAI and the GOA (e.g., Durban *et al.* 2010). Based on these new data, it is now clear that there are sufficient numbers of transient killer whales, whose distribution overlaps that of SSL, to exert significant predation mortality on SSL. Several modelling studies have shown that it is feasible for killer whale predation to have significant population-level effects on SSL (Barrett-Lennard *et al.* 1995; Springer *et al.* 2003; Williams *et al.* 2004). There has been considerable debate (see BiOp 170-171 for references) over the role of killer whale predation in the population declines of SSL, particularly regarding its role in the megafauna collapse hypothesis (Springer *et al.* 2003). Despite the results of these feasibility studies, the effects of transient killer whale predation on the dynamics of SSL remain unknown.

Although there are several lines of evidence that killer whales eat SSL, I am not aware of evidence to indicate that they "specialize" on SSL, as concluded in the BiOp:

> *Analyses presented by Holmes and York (2003) is contradictory to top-down stressors in the region of Kodiak Island where killer whales are known to specialize on Steller sea lions, yet adult and juvenile survival rates are high.* [BiOp 262]

Based on the evidence, it would be more accurate to conclude that killer whales are known to *consume* SSL, yet adult and juvenile survival rates are high. The difference between "specialize" and "consume" in the context of the sentence has huge implications for the strength of the contradictory evidence implied in the BiOp.

Although the importance of killer whale predation is still unknown, progress toward obtaining better estimates of killer whale diets has been made using fatty acids and stable isotopes (e.g., Herman *et al.* 2005, Krahn *et al.* 2007). The development of a LHX tag also promises another approach to estimating predation mortality on SSL. Horning and Mellish (2012) implanted LHX tags in 36 juvenile SSL in Prince William Sound, Alaska from 2005 to 2011. Causes of mortalities in these juveniles were inferred from temperature data collected by the tag. Gradual cooling and delayed extrusion indicated non-traumatic deaths (i.e. disease or starvation), whereas precipitous drops to ambient temperature indicated acute death by massive trauma. Data from 12 mortalities were transmitted to Service Argos via LHX tags. Ten events exhibited precipitous temperature drops indicative of predation and another was suggestive of predation. Identity of the predator is not known, but killer whales and sleeper sharks are presumed sources. Despite this progress, we are still some distance from having sufficient data to estimate killer whale predation mortality on SSL and how it might vary throughout the western population.

Nutritional-stress hypothesis

The effects of nutritional stress on SSL are discussed in Section 3.1.14. The text is organized around studies in the 1980s, a period of rapid decline, and 1990s a period of slower decline. Defined as *"the result of a species being unable to acquire adequate energy and nutrients from their prey resources "…* nutrition stress can be manifested through *"acute nutritional stress (e.g., emaciation, rapid mortality through starvation, large scale breeding failures) and chronic nutritional stress (e.g., reduction in fecundity, reduced body size, higher juvenile and adult mortality, increased predation risk) (Trites and Donnelly 2003…"* [BiOp 112] Nutritional stress resulting from inadequate prey might be caused environmental change, inter-specific competition, foraging responses to risk of predation, lack of appropriate food (junk-food), fisheries removals or combinations of these factors.

The BiOp summarizes the evidence for 32 potential responses of SSL to nutritional stress in Table 3.17. These 32 responses basically represent a finer-scale breakdown of the 17 responses listed in Fig. 4.25 (the response framework) along with reduced survival responses. As noted by Bernard *et al.* (2011), Table 3.17 is hardly referred to in evaluating the evidence for nutritional stress, but inspection of the Table, reveals that the only evidence for nutritional stress is the inferred reduction in natality. Other responses that occurred (such as changes in survival) could have been caused by other factors (e.g., predation). As noted earlier, I have concerns about the data and methods used to estimate natality from population models and from longitudinal observations for uniquely marked individuals. Therefore, I conclude that there is little evidence that SSL experienced nutritional stress in the past and essentially no evidence that SSL are currently experiencing nutritional stress.

Experimental studies on captive SSL have revealed the kind of responses that are expected to occur when SSL are nutritional stressed (Rosen 2009), however, direct evidence of nutritional stress has been difficult to find in the field. Much of the research since the early 1990s compared sites of contrasting trends in abundance to test for nutritional stress (BiOp p 114-115). Many of these studies produced results that were contrary to what would be expected in animals experiencing acute nutritional stress. The design of those comparative studies made two strong assumptions. The first was that the only difference between sites exhibiting contrasting demographic trends was the abundance of prey and the second was that measurements made in early lactation would be sufficient to determine if food was limiting. In retrospect, there was little *a priori* reason to expect the first assumption to be true for any given pair of sites (i.e., sites presumably differed in ways other than food, and food availability was not measured) and food conditions would have had to have been very bad for the second assumption to have revealed a positive finding so early in lactation. Even longer-term studies of juveniles over several years failed to find evidence for reduced nutrition in the western compared to the eastern population of SSL (Fadely *et al.* 2004).

The BiOp summarizes the discussion on nutritional stress by stating that *"The general conclusion from these physiological studies comparing the eastern and western DPS during the 1990s has been that nutritional stress was not evident in adult females or pups from the western DPS. Whether this was due to inherent biases in the study design or other confounding factors is not known."* [BiOp 115] I believe that poor experimental design and confounding factors probably did make it difficult to test the hypothesis. But the fact remains, no evidence for nutritional stress has been found despite multiple studies. The summary paragraph states that *"…while other demographic evidence (Holmes et al. 2007, Holmes and York 2003… Fay and Punt 2006) suggests a lingering chronic impact…"*. As discussed above, the evidence for this "lingering chronic impact" is weak. Furthermore, reduced natality during a period of high juvenile survival seems unexpected as it implies that relatively inexperienced juveniles were foraging more successfully than experienced adult females.

Under the food-stress hypothesis individual SSL cannot obtain sufficient food to meet requirements resulting in reduced growth, natality or survival. Another food-stress hypothesis, referred to as the junk-food hypothesis (Rosen and Trites 2000, Trites and Donnelly 2003), is discussed in Section 3.1.14.3. This hypothesis states that SSL can experience too much of the wrong kind of food resulting in negative effects on growth and survival. Over time this hypothesis has been refined, based on extensive experimental studies on captive SSL (review by Rosen 2009). The current hypothesis is that adult SSL can compensate for lower energy prey by increasing the amount of food they eat (e.g., Castellini 2002, Calkins *et al.* 2005), whereas animals < 1 year old, due to a smaller stomach capacity, cannot satisfy their energy requirement if they consume only low-energy density prey (Rosen 2009). Although the experimental evidence indicates that too much low caloric-density food can be detrimental to young SSL (< 1 year old), we simply do not know if the consumption of such "junk food" has influenced the dynamics of SSL. Furthermore, given that young SSL are dependent on milk for much of the first year of life, the junk-food hypothesis would not seem to have much relevance as an explanation for nutritional stress in SSL.

Inter-specific competition

Inter-specific competition could also reduce prey availability to SSL. As noted in the BiOp, Section 4.2.4, there are a number of fish and mammals species that might compete with SSL for prey. Wilderbuer *et al.* (2009a) found that Arrowtooth flounders are important predators of pollock and other fishes in the Gulf of Alaska and the Bering Sea and their large and increasing biomass makes them potentially important competitors of SSL. Despite the opportunity for inter-specific competition provided by other predators, no evidence is presented in the BiOp that competition has or is continuing to have negative effects on SSL abundance.

Disease, parasites and contaminants

There seems general agreement that diseases, parasites, and contaminants are unlikely to have played a significant role in causing SSL to decline. I agree with this assessment, with the caveat noted in the BiOp and the revised Recovery Plan (NMFS 2008) that not much is known about the effects of these factors on SSL.

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

Direct mortality from entanglement in fishing gear and shooting were important sources of mortality prior to the 1990s, after which these sources of mortality were largely eliminated. For the most part these effects on SSL have been described in detail in previous BiOps and in the revised Recovery Plan. The current account appears to be both thorough and accurate.

Indirect effects of fishing could be caused by harvesting of SSL prey species or be mediated through ecosystem effects on species interactions affecting SSL food availability (also see Terms of Reference e below). Section 4.5.4.6, Ecosystem Effects of Fishing, sets up the expectation of negative effects on SSL. Certainly, the potential for competition should be investigated, but the text of the BiOp seems to lead the reader to be pre-disposed to a conclusion for which there is no evidence. For example, the conclusion from a discussion of increasing predation mortality on age 3+ pollock, is that:

> *…increasing competition for larger prey is consistent with the parallel declines of halibut, cod, and Steller sea lions.* [BiOp 251]

It is unclear how the BiOp comes to this conclusion. No evidence of competition has been presented. What has been indicated, from model results, is that the consumption of age 3+ pollock has increased. Increased consumption does not equal increased competition. Although use of the word "consistent" does not imply causation, it is used in such a way as to lead the reader in that direction.

Fishery effects on Critical Habitat are presented in Section 5.1.6. The value of critical habitat in the context of the BiOp essentially relates to the availability of adequate prey to support SSL. To estimate this value, NMFS has calculated forage ratios (estimates of prey-assemblage biomass/estimates of prey consumption by SSL). Having calculated forage ratios, NMFS then seems to conclude that they are not useful:

> *NMFS has found difficulty in interpreting this metric because the forage available to SSL based on all species (and not just pollock, cod and Atka mackerel-Ianelli et al. 2010b) indicates that the there was on average (2002 to 2006) over 700,000 mt of forage in RCA 1 for SSLs. … Given the long-standing decline in abundance of SSL in RCA 1, it is clear that a high forage ratio alone is not sufficient for understanding trends in abundance.* [BiOp 291]

The preceding text is one interpretation, but another is that the high forage ratio in an area of decline suggests that food may not be limiting. Therefore, the BiOp seems to conclude that the metric does not capture the effect that must be there since the SSL population is declining. Admittedly the forage ratio metric is a rather crude measure of the food available to SSL, but nevertheless, the conclusion of the BiOp seems problematic. Later in the BiOp (p 296-299), the authors estimate forage ratios at different spatial scales, and then seem to dismiss them when they do not provide the anticipated answer. This apparent bias toward food limitation and fisheries is also evident in reference to the Removal of Prey from Critical Habitat, Section 5.1.6.4. The BiOp states that '*In some areas, management measures to mitigate these fishery impacts … may not be equally effective …".* [BiOp 292] The use of "impacts" here seems unwarranted as it presumes the effect on SSL without providing any evidence. The BiOp goes further by stating that:

> *Steller sea lion populations in areas where directed fishing for Atka mackerel in critical habitat is prohibited (RCAs 4 and 5) have generally been stable or increasing slightly since 2000. However, populations in areas where fishing inside critical habitat is permitted (RCAs 1-3) have continued to decline. This correlation is consistent with the hypothesis that Atka mackerel fishing within critical habitat areas west of 178°W may have contributed to the continued decline of Steller sea lion numbers in RCAs 1-3. As noted later in this document, we believe this management history can be used in an adaptive management sense …* [BiOp 293]

The logical fallacy here is in assuming that coincidence equals causation. However, this coincidence cannot reasonably be interpreted as causation without controlling for other factors that might account for the observed differences (such as predation). Further on, demographic trend is again used as the basis for concluding that food is insufficient and fishing is the cause of food limitation:

> *In critical habitat areas (RCAs 1-3) where fishing has occurred at varying levels since the inception of the Atka mackerel fishery (since 2002 up to 60% of the TAC in critical habitat), Steller sea lion numbers continued to decline. No change in population trajectory was observed after the enactment in the early 1990s of prohibitions on direct mortality. Thus, it is likely that prey availability in critical habitat in RCAs 1-3 has not been sufficient to sustain a sufficient forage value of habitat in this RCA to a point that Steller sea lion numbers can stabilize and recover.* [BiOp 293]

> *Catches in critical habitat of pollock and cod within RCA 6 through the 1990s were higher than those of the 2000s. The observed association of lower catches in critical habitat in the 2000s with a robust recovery of Steller sea lion abundance in RCA 6 indicates that recently enacted management measures in this region may have improved prey availability for Steller sea lions in this region.* [BiOp 294]

There is little basis for these conclusions. Although 60% of the TAC may have been taken from critical habitat, the exploitation rate of Atka mackerel was <10% of a large biomass. Again, without consideration of other factors that could account for the demographic trends within these RCAs, it is erroneous to conclude that forage was insufficient or that management measures increased the forage value.

Certainly if the intention is to use forage ratios as a measure of food availability for SSL, it is important to provide an indication of the quality (accuracy and precision) of the estimates. However, the BiOp states:

> *… it should be noted at this time NMFS has no reliable way to ascertain the uncertainty associated with a forage ratio estimate for a given area and for a given time period.*

Nevertheless, they go on to conclude that:

> *… the amount of forage biomass in critical habitat alone in areas 543, 542, and 541 appears to be insufficient by itself to support efficient foraging (i.e., relative to forage ratios of 20-50; see above).* [BiOp 299]

Although the estimated forage ratios in the areas listed on p 298 are low relative to required ratios of 20-50, this range is arbitrary:

> *… NMFS believes that the quantitative tools are not available at this time to reliably determine a threshold for this [forage] value that would necessarily be consistent with sufficient forage biomass to promote and achieve recovery of the western DPS of SSL …* [BiOp 298]

Correlations between fishing and SSL

Analyses that test for the negative effects of fishing on trends in SSL abundance are central to conclusions about the likelihood that continued fishing may cause JAM. A number of studies have attempted to test the "fishery-effects" hypothesis using correlation analysis. Although not the only approach to testing this hypothesis (Wolf *et al.* 2006), it is the most commonly used approach. Many, but not all, of these studies are referenced in the BiOp, but nowhere in the document is a there a comprehensive review of what these studies have found. The lack of critical review of these studies is both surprising and problematic, given their importance to the conclusions of the BiOp. The evaluation of these studies is provided in one paragraph on page 282. The text would seem to indicate that these studies have not been informative. According to the BiOp, the only indication of negative fisheries effects comes from the correspondence of fisheries management measures and SSL trends. As I have noted above, such correspondence is not convincing evidence.

The BiOp concludes that:

> *Statistical and correlative analyses of fishery effort/catch with trends in local Steller sea lion populations have yielded equivocal results, some indicating a positive and some a negative relationship between catch and Steller sea lion population trends (Loughlin and Merrick 1989, Ferrero and Fritz 1994, Dillingham et al. 2006). The utility of these analyses is diminished by issues of temporal and spatial scale mismatch between the treatment (magnitude of fish catch around a rookery) and response (population trend at that rookery), since animals breeding at a particular rookery range much farther during the year than the area encompassed by the catch data.* [BiOp 237-238]

The last sentence is somewhat difficult to understand as it seems to imply that the effect of fishing near the rookeries is not strong enough to measure and only if we used data from the entire foraging range of SSL could we expect to see the effect. But the intended result of most of the mitigative measures (Section 2.5.2) has been to reduce presumed negative effects of fishing from the local areas where SSL are thought to be most vulnerable (i.e., rookeries and haulout sites). Thus, by dismissing these studies the BiOp appears to be concluding that the mitigative measure may not have been entirely appropriate.

Although the BiOp largely dismisses studies that have attempted to test for negative effect of fishing on SSL population trends, what do those studies actually indicate? Loughlin and Merrick (1989) conducted the first of these studies. Using data from 1976-1986 they compared SSL counts and pollock catches in seven areas surrounding 8 major rookeries. Of 36 tests, 32 were non-significant, 2 were positive and 2 were negative. Ferrero and Fritz (1994) repeated the analysis of Loughlin and Merrick with updated SSL counts and pollock catches to 1991 and conducted a second set of tests at 13 rookery sites using estimating pollock catches within 20 nmi around the major rookeries. Only 3 of 74 non-parametric tests were significant – 2 negative and 1 positive. The authors concluded that random chance alone could account for the significant results, given the number of tests conducted.

Sampson (1995) used principal components analysis on to SSL counts from 1979-1990 at 25 rookeries and groundfish (including pollock, cod, Atka mackerel) catches and effort data from 1980-1989, again within 20 nautical miles (nmi) of rookeries, to test for negative fishery effects. Using all groundfish effort and catches, only 2 of 80 correlations were significant – 1 positive and 1 negative. Separate sets of tests were then done for pollock catches (1 positive of 40 tests), Atka mackerel (1 positive and 1 negative of 40 tests) and cod (4 negative of 40 tests). The analysis was repeated with pup counts – with only 4 significant correlations of 200, all positive. Sampson noted that many of the significant correlations were with third or fourth principal components, which accounted for little of the variation (5%-15%), but overall there was little evidence for a fisheries effect.

Hennen (2006) conducted an analysis of temporal trends in the rates of SSL population changes for 32 rookeries using three time series differing in the first year counts were included. Hennen found 10 of 18 correlations were negative between the number of fishing hauls and SSL population trend with the three time series all ending in 1991. For the period after 1991 through 2002, there were no significant negative correlations and three positive correlations. Although there were negative correlations before 1991, the inference that the difference in the pattern of correlation was caused in part by fisheries mitigation measures is problematic. First, as noted by Hennen (2006), the measures were introduced around all rookeries at the same time, so that a comparison of protected vs unprotected rookeries was not possible. Second, the proportion of the catch taken from SSL critical habitat in the BS declined only modestly for pollock and cod, but did not decline for Atka mackerel or Arrowtooth flounder after the measures were introduced during the period 1991-2008 and did not decline at all in the GOA, except for the small catches of Atka mackerel (Table III-1, Figure III-6, III-7, Appendix 3 BiOp).

Dillingham *et al.* (2006) examined fine-scale indirect interactions between SSL trends and fishery catches and effort from southeast Alaska to the Aleutian Islands from the period 1976 to 2002. The rate of change in SSL counts were estimated for two periods using a spline regression. Commercial catch data (all trawls within 40 nmi (74 km) of rookeries) on Alaska pollock, Pacific cod, Atka mackerel and Arrowtooth flounder were obtained from NMFS for the period 1983-2002. Fisheries effort data were obtained for the period 1990-2002. Regression models indicated a negative relationship between SSL counts and pollock density, but the effect accounted for a small portion of observed changes in SSL counts. Similar results were obtained with cod, but when year was added to the model, the result was no longer significant. No significant relationships were found with Arrowtooth flounder or Atka mackerel. Statistically significant positive relationships were found between SSL counts and trawl effort, but the small effects were presumably of little biological significance. There were no significant relationships between SSL counts and longline effort. The authors concluded that neither commercial groundfish abundance nor commercial fishing effort could explain the large historical declines in SSL.

Calkins (2008) investigated indirect interactions between the Pacific cod longline fishery in the BSAI and SSL counts at 44 rookeries during the period 1996-2004. Trends in SSL counts were estimated using spline regression with a fixed hinge point in the year 2000. For the period form 1996-2000, there were some negative correlations between SSL trends and longline fishing, and positive relationships from 2000-2004. However, few of these relationships were statistically significant. There was evidence over most distances from rookeries for a negative association between SSL trends and CPUE during the period 1996-2000. That is, where fisheries were efficient (high CPUE) but had low fishing effort rates of SSL decline were lower. However, over the entire time period the relationship was positive making interpretation difficult, but suggesting that something in the marine environment changed between the 1990s and 2000s (Calkins 2008). The study concluded that their results were consistent with the hypothesis that longline fishing and SSL population trends were "largely independent of each other".

AFCS (2010) undertook an analysis to understand relationships between regional changes in SSL populations from 1991 to 2008, the spatial-temporal distribution of sea lion prey species, fisheries for these prey species, and oceanographic variables of the North Pacific. Estimated harvest rates of Atka mackerel, Pacific cod and pollock were separately regressed against the rate of change in SSL non-pup counts for three time periods. What distinguishes this analysis from all others was the decision to set the significance level at $\alpha=0.25$ rather than the conventional level of $\alpha=0.05$. This liberal level made it much more likely to find significant regressions between SSL trends and harvest rate than would have been the case using the conventional level. For example, AFSC (2010) found 9 significant relationships (3 negative and 6 positive) of 18 tested for the three fish species in the BSAI at the $\alpha=0.25$ level, whereas none were significant at the conventional level of $\alpha=0.05$. In fact, using the conventional level, no significant relationships between SSL trends and fisheries were detected in the study. A number of oceanographic variables were significant. The paper concluded that "*Our results suggest that oceanographic processes are more important to SSL population growth rates in the Aleutians Islands than in the GOA, and that the western Aleutians may be an unproductive and harsh environment for sea lions. This apparent sensitivity of SSL populations to oceanographic processes coupled with the negative effects of harvest rate may contribute to the continuing negative rates of of SSL population growth in the Aleutian Islands, particularly in the western islands.*"[p19] The conclusion about negative effects of fisheries on SSL is based only on the very liberal criteria used for judging statistical significance. Use of the conventional criteria would have lead to the very different conclusion (i.e., no detectable relationships).

Trites *et al.* (2010) investigated the relationship between the decline of SSL non-pup counts and the Atka mackerel fishery in central and western Aleutian Islands (Fishery Management Areas 541, 542 and 543) from 2000-2009. Models were fit to the frequency of trawling (number of hauls) and amounts of fish caught within 10, 20 or 40 nmi of sea lion rookeries and haulouts. They found significant relationships between longitude and catch, and between longitude and number of hauls, for Atka mackerel for some of the models, suggesting that these metrics could not distinguish a fishing effect from some other geographically influenced variable on SSL counts. For the remaining models—none detected a negative relationship between fishing (number of hauls and total catch) and sea lion numbers. Their findings were not consistent with the *a priori* expectation that lower sea lion numbers should be associated with greater fishing effort.

Finally, Hui (2011) examined relationships between sea lion population trends, fishery catches and the prey biomass of walleye pollock, Pacific cod and Atka mackerel potentially accessible to sea lions around 33 rookeries from 2000-2008. Of the 304 statistical models constructed to compare accessible prey biomass and catch to SSL population trends, only three relationships were significant, all positive. Thus, there was no evidence that the availability of pollock, cod or Atka mackerel was negatively affecting SSL population trends.

Bernard *et al.* (2011) also reviewed these studies (their Table 3.1). They noted that the studies published after 2000 used a longer time series of data and therefore had more statistical power to detect significant relationships. Although a few significant negative relationships had been found prior to 2000, none have been found after 2000. Several studies have specifically tested for relationships in the AI (areas 541-543) where SSL continue to decline (Trites *et al.* 2010, AFSC 2010, Hui 2011). These studies have failed to detect significant negative relationships. Although they are not truly independent (they all use the same SSL count database and similar fisheries catch and effort data), they do represent the best test of the hypothesis that fisheries negatively affect SSL trends. On the basis of these studies, I conclude that there is no evidence for the hypothesized negative fisheries effects.

Section 5.1.7.6 of the BiOp summarizes attempts to test for the effects of fishing with the following paragraph:

> *In this Biological Opinion we have shown that fisheries cannot be excluded as a factor that affects Steller sea lion population dynamics and their habitat on a number of levels. However, it should be noted that several analyses failed to show statistically significant impacts of commercial fisheries on the western DPS of Steller sea lion. Nonetheless, short-term effects are described in Section 4.5.3 and suggest a potential interaction between the immediate effects of fishing and Steller sea lions life history and population dynamics. [BiOp 300]*

This conclusion is inconsistent with my reading of the literature. If anything, the lack of evidence contained in those papers suggests that fishery effects can reasonably be excluded as a factor. Furthermore, material presented in Section 4.5.3 does little more than “suggest a potential interaction”; hardly the type of evidence upon which to draw conclusions about jeopardy.

The bullets on page 301 (Box below) of the BiOp summarize the key conclusions with respect to the effects of fisheries on SSL. The first bullet is incorrect based on my reading of the literature. Some experiments show that fishing can have short-term effects on the prey field whereas others do not (see below). The second bullet is also incorrect as it concludes that negative fishery effects have been shown to occur, although few to none have been found. In the fourth bullet, contaminants, dismissed earlier in the BiOp, are now a confounding factor. The final bullet rejects the results of numerous studies that have failed to find evidence of negative correlations. I believe the preponderance of evidence at this time indicates just the opposite.

- Fisheries have short-term effects on the prey field for Steller sea lions both within and outside of critical habitat including reduction in local prey amounts from what would have existed without fishing, and thus fisheries may have disproportionate impacts on a local scale relative to the global harvest rate due to localized depletions and spatial heterogeneity of prey habitat
- Fisheries have long-term effects on the prey field for Steller sea lions both within and outside of critical habitat due to changes in prey size, distribution, and productivity
- Oceanographic conditions vary across the Steller sea lion range and may exacerbate or ameliorate the implementation of the fisheries in the short-term
- Fishery impacts are likely confounded by regionally specific factors, such as predation and contaminants.
- 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.

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?

The BiOp marshalls diverse sources of information to construct a weight-of-evidence argument for the likelihood that fisheries for Alaska pollock, Pacific cod, and Atka mackerel jeopardize or adversely modify critical habitat of SSL. A summary of the weight of evidence for JAM is given in the Working Model of Effects, Section 4.7.2, and again in Section 7.4.3 of the BiOp. It is my opinion that the scientific weigh of evidence as presented provides little support for the findings and conclusions of the BiOp.

Fishing has the potential to affect SSL dynamics in several ways: large-scale reduction of prey abundance, local depletion of prey abundance, and reduced quality (size, age and caloric value) of prey by selective removal of higher quality individuals (Goodman *et al.* 2002, Trites *et al.* 2007). The argument for JAM begins with Section 4.5 of the BiOp where the indirect effects of fishing on SSL habitat are discussed. To conclude jeopardy and adverse modification *"NMFS must demonstrate that the listed species or their designated critical habitat are exposed to the effects of an action. To establish exposure we determine the spatial and temporal overlap between listed resources and the direct or indirect physical, chemical, and biotic stressors of an action."* [BiOp 198]

Figure 4.24 of the BiOp summarizes the decision framework that NMFS used to determine if SSL where exposed to the Action (i.e., effects of commercial fishing), whereas Figure 4.25 summarizes the potential responses of SSL and critical habitat exposed to the effects of commercial fishing. These two decision trees appear to serve as the basis for constructing the weight-of-evidence for JAM. I found this framework incomplete, ambiguous, and also rarely referred to in the BiOp. For example, food-web dynamics is identified as in indirect stressor in Fig. 4.24, but it does not appear to be evaluated in the context of exposure to the fisheries of interest.

The first decision point in Figure 4.24 is to determine if there is direct interaction with the fisheries of concern. This is determined on the basis that the fished species accounts for >10% FO of the SSL diet. Although 10% is arbitrary, even if accepted as a reasonable threshold, the use of FO is problematic because this metric an unreliable estimate of the contribution of prey species to the energy requirements of SSL. Using >10% FO as the decision point has the effect of setting an unreliable bar rather low with respect to significant interaction.

On the basis of the 10% threshold, the following fisheries are identified in the BiOp as being of concern: pollock (trawl), Atka mackerel (trawl), Pacific cod (trawl, hook-&-line, pot), Rock sole in the BSAI (trawl), shallow water flatfish assemblage in the GOA (trawl), salmon fisheries, and Arrowtooth flounder (trawl). These species qualify based the diet data presented in Sinclair and Zeppelin (2002) and summarized in BiOp Fig. 3.23, but the apparent importance of these species varies markedly by season and by SSL region, with FO falling below the 10% mark in a number of cells. As a result, it is not clear how the decision criterion was applied.

The next decision evaluates exposure of SSL habitat to the fisheries. Habitat in this context is taken as food. Therefore, overlap in the size of prey taken by the SSL and fisheries, the spatial (including depth) and temporal pattern of consumption and concentration of removals “compressed fisheries” are used as decision points. Three of the five must be scored yes to continue. However, the discussions of prey size and depth overlap between SSL and fisheries in Sections 4.5.3.1 and 4.5.3.2 do not provide the reader with any quantifiable basis for a yes/no decision. Information on neither depth of diving by SSL, Sections 3.6 and 3.7, nor the depths of fishing is synthesized to address the degree of overlap. In any case, the BiOp should have presented quantitative analyses to support decisions about overlap exposure, given the nature of these overlap variables.

It is not clear from the BiOp which of these overlap criteria passed, but I presume three or more did as the BiOp then goes on to evaluate the exposure of SSL to the Action. At this stage in Figure 4.24, there are five habitat responses and four exposure variables to consider. Each of these appeared to be scored qualitatively using six “Description of Exposure” factors. I could not determine how these SSL response and exposure variables were evaluated with respect to the decision framework in the BiOp.

One of the SSL habitat responses in Figure 4.24 is localized depletion of SSL prey hypothesized to occur during groundfish fishing. Evidence for the localized depletion of prey is discussed in Section 4.5.3.3. The ability of fishing to reduce the local or regional abundance of fish has been known for more than 50 years (DeLury 1947). Nevertheless, it was important to determine the spatial and temporal characteristics of prey depletion to begin to evaluate the likelihood that such depletions might negatively affect SSL. Lowe and Fritz (1997) showed the potential for fisheries to reduce local abundances of Atka mackerel, but the magnitude and duration of depletion were site-specific, so it was difficult to make general conclusions. Furthermore, the exploitation rates used to achieve the reduced fish abundance were considerably higher than target harvest rates for managed fisheries. On the other hand, experiments on Pacific cod (Conners *et al.* 2004, Conners and Munro 2008) indicated no within-year differences between trawled and untrawled sites over three years. Fritz and Brown (2005) showed that Pacific cod abundance in the area north of Unimak Island declined significantly at the scale of hundreds of nautical miles. However, this is not exactly the kind of depletion imagined to affect SSL at a local scale. A multi-year pollock fishery interaction study was conducted off Kodiak Island. The authors concluded that the results from two of the study years did not provide support for a localized depletion of pollock (Walline *et al.* 2012). Overall, it is difficult to conclude much from these experiments. The results appear to depend on the spatial scale of fishing, fish movement, and the exploitation rate. These findings are neither unexpected nor do they tells us much about how SSL might be affected by fishing.

The BiOp concludes that: *"… the critical link between fisheries removals (time, rate, location, etc.) and the effects on Steller sea lions is poorly understood and we cannot determine the relationship between these catch rates and the impacts on prey except that higher catch rates would be more likely to result in localized depletions."* [BiOp 240] This conclusion captures quite well what we know. But it is difficult to see how this formed the basis for an assessment of the exposure of habitat to fishing.

Section 5.1.5 presents evidence for the responses of SSL to reduced prey availability caused by the proposed action (i.e., fishing), but similar, if not the same, responses would be expected if prey availability were reduced for other reasons (for example see Bowen *et al.* 2001, NMFS 2008). Although Fig. 4.25 is introduced as the framework for evaluation of SSL responses to reduced prey availability in Section 5.1.5, it is not referred again. Therefore, it is not clear if the framework was used to determine if SSL had or were continuing to experience nutritional stress from reduced food. As most of the text in Chapter 5 of the BiOp has little to do with the evaluation of evidence for food limitation, I conclude that the framework was not used in any meaningful sense.

In summarizing the Indirect Fisheries Effects, Section 4.7.1.2, the BiOp begins with a concise and, in my opinion, fair statement of the hypotheses concerning fisheries effects and the lack if evidence for such effects on SSL:

> *The potential impact of competition with fisheries, through a reduction in the biomass and quality of Steller sea lion prey species, is a highly debated topic among the scientific community. .... The effect of fisheries on the distribution, abundance, and age structure of the Steller sea lion prey field, at the spatial scale of foraging Steller sea lions and over short and long temporal scales, is largely unknown.* [BiOp 259]

However, this paragraph is followed by the rather surprising conclusion that "*Fisheries are likely to lower Steller sea lion carrying capacity.*" [BiOp 259] No evidence is provided for this conclusion. The language in this section assumes that there are negative effects of fishing on SSL, despite having stated in the opening paragraph that "*The effect of fisheries … is largely unknown.*" In my opinion, one could argue that an even stronger statement is warranted namely, the effect of fisheries on the population dynamics of SSL is unknown, despite repeated attempts to determine those effects.

The BiOp goes on to state that:

> *Two stressors were likely to have affected the prey field for Steller sea lions: (1) climate induced changes … and (2) fishery-induced changes …. Both climate change and fisheries induced changes in prey communities likely have affected the condition of Steller sea lions…, but the relative importance of each is a matter of considerable debate.* [BiOp 263]

My concern here is with the use of "likely" to characterize the probability that those two stressors, particularly fishery-induced changes, acted on the SSL population. Studies that tested for negative effects of fishing on SSL essentially concluded that none were found (Bernard *et al.* 2011). My review of those studies concurs with this conclusion. Thus, at least in the case of fisheries, it would appear that a more appropriate adverb to describe our understanding is "unlikely".

Working model of effects on SSL

The BiOp presents a "working model" (BiOp 264-265) of factors that may be contributing both to the lack of a robust recovery of SSL, and the continuing declines in abundance in the western and central Aleutian Island sub-areas. In my opinion, the working model basically concludes that we are no closer to understanding why the western stock of SSL declined so rapidly during the 1980s and 1990s, continues to decline in part of the central and western Aleutian Islands, and has increased over the remainder of the stock's geographic range. Several decades of research has resulted in substantial gains in our understanding of the physiology, foraging ecology, diet, life history traits, and sources of mortality of SSL. Continued monitoring of pup and non-pup counts has provided a basis for our understanding of the regional dynamics of SSL. Unfortunately, none of this new knowledge has provided a clear understanding of the factors affecting the dynamics of SSL.

For the most part, I agree with the conclusions contained in the working model. However, I differ with the working model on the issue of the extent to which fisheries may have impacted SSL through indirect effects on their food supply. I am unaware of any convincing evidence of fisheries effects on SSL, only the repeated statements about hypothesized effects and their unquantified likelihood are brought forward as evidence. Nevertheless, the BiOp concludes that:

> *Much of the preceding discussion on the potential for competition between the Steller sea lion and BSAI and GOA groundfish fisheries has focused on exploitative competition; that is, it examines competition that occurs when fisheries remove prey and thereby reduce prey availability to Steller sea lions. In addition to exploitative competition, fisheries may affect Steller sea lions through interactive competition*. [BiOp 246]

By "interactive competition" I believe the BiOp is referring to what is known in the ecological literature as interference competition. The notion here is that fishing disrupts the spatial distribution, school size or depth of fish thereby making it more difficult for SSL to forage, resulting in reduced net energy gain. The BiOp concludes that these types of competitive interactions cannot be tested with existing information and further states that:

> *The strategies used by fishing vessels likely alter schooling dynamics and important features of target schools such as their number, density, size, and persistence (recent field studies on the effects of fishing on fish school structure are described below). If Steller sea lion foraging strategies are adapted to take advantage of prey aggregations or schools, then trawling may result not only in exploitative competition through removal of prey, but also in interactive competition through disruption of schools or aggregations and their normal dynamics. For example, the removal of a portion of a fish school by a trawl net must create at least a temporary localized depletion (i.e., a gap in the prey school). How long that gap persists and the responses of the remainder of the schooling prey to trawling are unknown.* [BiOp 246]

Although most of the above text is speculation, the final sentence contradicts the results of depletion studies conducted by NMFS (see above). While I agree that more could be done to understand how fishing affects fish school structure, the results of existing studies would seem to suggest that effects are small and transient and that further study may not be warranted.

The BiOp ends this section about the response of SSL to competition with fisheries with several conclusions:

> *A predator faced with competitive pressure would normally shift its diet (if possible). Steller sea lions, however, would then have to compete with fisheries for Pacific cod, yellowfin sole, flatfish, Pacific salmon, herring, rockfish, and other species which are commercially harvested (both directly and as incidental catch).* [BiOp 247]

We do not know how SSL respond to competition, but the statements in the BiOp give the impression that SSL are caught between a rock and a hard place – moving away from one commercially harvested prey species would serve only to increase competition with other harvest prey species. Although this could happen, there is no evidence for competition in the first place and therefore it is unjustified to conclude the SSL would be boxed in to the extent indicated in the BiOp.

Reasonable and prudent alternatives

Chapter 8 describes the RPAs proposed to remove jeopardy and summarizes the scientific evidence leading to the determination of jeopardy. To justify the development of the RPA, the BiOp carries forward 10 lines of evidence. These are a mixture of observed trends in SSL counts for the western and central AI and repeated speculations about the hypothesized effects of fisheries for Atka mackerel and Pacific cod on SSL (BiOp p 359-360).

The first three lines of evidence are the estimated trends in pup to non-pup ratios, counts of pups, and counts of non-pups in the AI. The counts trends are uncontroversial (although still reported without a measure of uncertainly), but they do not provide evidence for any particular underlying cause. The BiOp once again assumes, without evidence, that pup to non-pup ratio are an index of depressed natality in the AI. The fourth line of evidence is remarkable, in so far as it states that as no other mechanism (stressor or combination of stressors) has been identified, then prey removals (i.e., fishing) which **could** (my emphasis added) result in chronic nutritional stress **could** be a factor underlying the declines. The Biop states that "…dozens of field and captive Steller sea lion studies…" provide support for prey removals causing chronic nutritional stress. Having read the BiOp, I am unaware of such field studies and note that the captive studies show only that calorie restriction causes loss of body mass and condition. They provide no evidence the prey removals in the wild cause chronic nutritional stress. The fifth line of evidence simply states that the forage biomass of key SSL prey have been harvested in the AI. Although, noting that "key" is based on an unreliable measure of importance in the diet, this is factual, but not evidence of a fishery effect on SSL. The sixth line of evidence is that harvest rates have been high within SSL critical habitat, particularly of Pacific cod. It is true that a large fraction of the catch has been taken from critical habitat, but in the case of Atka mackerel the harvest rate has been low and the biomass rather high. In the case of Pacific cod, the harvest rate has been higher, but only a small fraction of the cod stock inhabits the AI. The seventh line of evidence concerns the importance of Atka mackerel, Pacific cod and pollock in the diet. Given the use of FO, the importance of these species is almost certainly overestimated. The eighth line of evidence is based on habitat use, as determined from satellite telemetry, within critical habitat, but pointing the greater use of areas beyond critical habitat that may need to be protected. The ninth line of evidence draws on the results of the POP analysis by Boor and Small to highlight areas of importance to SSL outside of critical habitat. As the method has not been validated, it seems premature to use the results of this analysis as the basis for RPA development. The final line of evidence points to the correspondence between regional differences in mitigation measures in the AI and trends in SSL counts. As noted above, this coincidence is not evidence of an underlying causation.

Overall, I conclude that there is no evidence for the hypothesized indirect effects of fisheries on the availability of food to SSL. As this is the only hypothesized effect on SSL, there is no reason to expect that the RPAs proposed for management areas 541-543, which will reduce or eliminate removal of Atka mackerel and Pacific cod, would have positive effects on SSL population trends in those areas.

With respect to the stated intention of the RPA, the BiOp states that "*… NMFS has determined through the weight of evidence that competitive interactions between commercial fisheries and the western DPS of Steller sea lions for important prey species could affect survival and natality rates to the point that it prevents the western DPS from achieving survival and recovery goals.*" [BiOp 373] However, as shown above, the weight of evidence for competitive interactions with fisheries amounts to little more than a series of weakly supported speculations about what is likely or possible. Studies that have tested for negative correlations between fishing variables and trends in SSL (i.e., competitive interactions) have overwhelmingly found little or no evidence (Bernard *et al.* 2011, this review). In fact, positive correlations have more frequently been found that negative correlations. Thus, the weight of evidence would suggest that fisheries do not compete with SSL for food. If competitive interactions do occur, they have been too weak to be detected.

CHAPTER 2

Background

New information on SSL and fisheries that that might be relevant to the determination in the November 2010 BiOp was presented at a public meeting held at the Alaska Fisheries Science Center, Seattle, August 1-2, 2012. Chaired by Dr. David Fluharty, the meeting was attended by more that 50 participants from NMFS, the fishing industry, NGOs, and both Canadian and American universities (Appendix 3). The meeting agenda is provided in Appendix 4.

Summary of Findings

In accordance with the Statement of Work, this chapter re-evaluates each of the Terms of Reference considered in Chapter 1 in the light of new information that has become available since the release of the BiOp.

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?*

Presentations from NMFS updated information on trends, vital rates, movement and distribution, and recent diet estimates of SSL. Additional surveys of pups in 2011 and non-pups in 2012 confirm the overall trends presented in the BiOp, namely that SSL counts in the central and western Aleutian Islands continue to decline, whereas other areas are either stable or increasing. Updated information on trends of SSL in Russia provided a broader context for evaluating trends of the WDPS. While the number of non-pups declined on the Commander Islands during the 1980s and then stabilized, pup counts increased though the 1990s, but have subsequently stabilized. Throughout this period, the Commander Islands have been protected by a 30 nmi no-fishing zone, although effective management of the zone was not put into place until the 1980s (V. Burkanov, pers comm., this meeting). More comprehensive comparison of trends in western SSL counts with those Russia could be useful in distinguishing local or regional effects from large-scale environmental forcing.

Resighting data through 2009 and 2011, respectively, from branded SSL pups in southeast Alaska and the eastern Aleutians and central GOA were used to update estimates of survival. These analyses indicated that there were no regional differences and that survival rate of both juveniles and adults are high. These updated estimates do not change our understanding from that presented in the BiOp. There are no estimates of vital rates in the declining regions of the AI, but NMFS is attempting to fill this gap. Estimates of survival of SSL juveniles that have been implanted with LHX tags appear similar to estimates from resighting of branded SSL. Although a larger LHX sample is needed to confirm initial findings, this is an encouraging result suggesting that the use of LHX tags may have broader application, both to estimate of vital rates and to determine sources of mortality in free-range sea lions.

As noted in the BiOp, the movements and foraging behaviour of relatively few adult females have been studied. NMFS reviewed the obstacles to studying adult females and provided some results on the development of better methods to remotely drug and capture females. These are encouraging developments that have resulted in 4 additional females being track for periods of 6-8 months in 2010 and 2011. This represents a significant improvement in the duration of tracking that should yield new insights. These longer tracks suggest that it might be possible to investigate foraging behaviour of SSL in relation to estimates of local or regional prey availability.

New diet estimates for the period 1999-2009 served to update the 1990-1998 estimates presented in Sinclair and Zepplin (2002) and used in the BiOp. Preliminary analyses indicate that the new data confirm seasonal and regional variability in the diet. Although there are some differences in the FO of some prey species in the new samples, these data do not change our understanding of SSL diets. A concern with the estimation of SSL diet is the continued use of FO to express the importance of prey species. Although the use of FO is reasonably justified to allow direct comparisons with Sinclair and Zeppelin (2002), it is critical that research be undertaken to reduce the bias toward species with robust hard parts.

Industry made several presentations which included an abundance estimate of WDPS in 2011 of 77,000 – 80,000 (with 52,000 in the US WDPS and 25,000-28,000 in Russia), based on a 2011 Steller sea lion survey report, (NMFS December 5, 2011). Using the 2011 estimates of pups and the NMFS multiplier of 4.5, they noted that the 2011 population estimate is 98% of the recovery plan population size downlisting criterion of 53,100 by 2015 (Recovery Plan 2008). They also noted that focus on subpopulations with respect to the determination of jeopardy is inconsistent with the approach to the PVA that used a single population. Similarly, Boyd (2010b) constructed a PVA for the WDPS, EDPS, and the combined SSL meta-population based on historical trends and found that none of the scenarios investigated predicted that the population was endangered. Together, these analyses suggest that the current attention to declines of a small fraction of the western population of SSL may be over-estimating risks to the population.

One of the industry presentations also noted that the BiOp contains little quantitative information on SSL movement between areas and sub-populations. Although a comprehensive analysis of various USA and Russian studies is needed, preliminary analysis by Industry suggests that the movement of SSLs may be greater than previously thought and natal fidelity appears to be less (even among females, also see Parker *et al.* 2008).

Horning and Mellish (2012) reported on the use of LHX tags to estimate SSL mortality rates and to infer the causes of mortality. Information on mortalities through 31 July 2012 was used to update those estimates. The new estimates are quite similar to the published estimates and indicate that none of the mortalities occurred near rookeries and that sea lions are most at risk 12-24 month post weaning. A population dynamics simulation model was used to explore how predation might be linked to the reproductive output of population and how this might affect other vital rates. Although this is an interesting approach, the assumptions about how density-dependent predation rate varies with age and sex seem unrealistic and therefore other forms of age-class specific, density dependence need to be explored.

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?*

Stock status of Pacific cod, Atka mackerel and Alaskan pollock were updated at the meeting. With respect to the BiOp, the most significant new data were the 2010 estimates of Atka mackerel survey biomass in the Western Aleutians which indicated a large increase relative to the 2006 estimate used in the BiOp to estimate forage ratios and RPAs.

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?*

Industry also drew attention to the findings of the Bernard *et al.* (2011) review noting that little evidence had been found to indicate that SSL were suffering from nutritional stress and the empirical estimates of natality (Maniscalco *et al.* 2012) which do not support model estimates of reduced natality in the central GOA (Holmes *et al.* 2007). The BiOp suggests that low pup to non-pup ratios in the western AI also provide evidence of reduced natality. However, the industry presentation noted that, as argued in the Bernard *et al.* (2011) report and here (see Chapter 1), use of pup to non-pup ratios as a proxy for natality requires a better understanding of sources of uncertainty and bias that could influence their interpretation, particularly when comparing ratios across time and space.

Both industry and the Bernard *et al.* (2011) review noted inconsistencies in the way forage ratios are used to justify the RPA and that the exploitation rate of Atka mackerel and biomass of Pacific cod and pollock in the AI are too low to have caused nutritional stress to SSL. They expressed concern that in the BiOp multi-species modelling results used to estimate the effects of fishing on SSL were dismissed in favour of single-species models that did not account for predator-prey relationships and that the use of FO is not an appropriate metric to assess the importance of prey in the diet of SSL (also see Chapter 1).

There was a brief summary of the so-called "junk-food" hypothesis noting that it is now clear that sea lions older that 1 year of age can adjust their intake to account for variation in the energy density of prey. Animals less than 1 year of age are limited by the gut characteristics (e.g., size) in their ability to respond to low quality prey. As such the hypothesis would seem to have little relevance to SSL as they continue to be fed milk until they are weaned, at the birth of the female's next offspring. As pinnipeds typically consume diverse diets, the negative effects of single-species, laboratory-fed diets are presumably overestimated relative what sea lions experience in the wild.

The final presentation concerned unpublished results from an experimental feeding study of SSL which showed that older juveniles gained body mass, mostly blubber, when fed pollock ad libitum. These results along with others confirm that a diet of pollock can provide most age classes of SSL with adequate nutrition. It seems that the time has come to abandon the junk-food hypothesis.

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

As noted in Chapter 1, Trites *et al.* (2010) examined evidence for negative effects of Atka mackerel fishing on trends in SSL counts in the AI. Based on comments received from NMFS, Trites *et al.* (2012) updated their analysis to test for the effects of localized depletion and overall availability of Atka mackerel caused by fishing. Of 129 models, only 3 found negative effects, 21 found positive effects and the remainder were non-significant. The updated analysis concluded that there is no evidence of negative effects of Atka mackerel fishing in the AI on SSL trends, confirming their initial findings.

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?*

Representatives from the States of Alaska and Washington described the rationale and process for undertaking the independent review of the BiOp by Bernard *et al.* (2011). The Bernard review focussed on the finding of JAM for groundfish fisheries in the BSAI management area; the likelihood that Reasonably Prudent Alternatives (RPAs) will result in recovery of SSL in the BSAI area; and the likelihood that among all possible RPAs that could result in recovery, the RPAs chosen would incur minimal economic and social costs. One of the more important contributions of this review was to critically evaluate studies that have sought to test for negative correlations between SSL trends and fisheries removals or effort. They found little evidence of negative effects. The predominant finding was no significant correlation between SSL counts and fishery-related variables. They also noted the lack of quantitative analysis of overlap, for example, in the size of fish taken by SSL compared to fisheries and inconsistencies with respect to the use of forage ratios to explain SSL declines in the western AI. The review identified a number of other problems with the exposure analysis and the likelihood that RPAs would remove jeopardy and adverse modification of habitat. Overall, I found that the review accurately characterized the limitations and weaknesses of the analyses used to support the determination of JAM and of the RPAs proposed to alleviate jeopardy.

NPFMC and the Council's Scientific and Statistical Committee provided comments related to the development, review, and implementation of the 2010 BiOp, the RPA developed with the BiOp, and the Interim Final Rule that established extensive fishery closures in the western and central Aleutian Islands. For example, the Council believes that the effectiveness of the conservation measures put in place in the 2000s was overstated, but was nevertheless used to justify expansion of conservation measures. We were informed that the "Council remains troubled by the tenuous nature of the link between commercial fisheries activities, hypothesized nutritional stress, and modeled reduction in reproduction (reduced natality determined from a single model in the GOA) in the wDPS upon which the Jeopardy and Adverse Modification (JAM) finding was fundamentally based." Based on my review of the evidence brought forward in the BiOp, I concur with the Council's concerns.

Oceana indicated support for management changes in the western Aleutian Islands outlined in the BiOp. However, they believe that NMFS has not taken action to mitigate the overall decline in natality, address local SSL declines in some regions, rebuild the historically viable rookeries in the Pribilof Islands, or address the effects of the past reductions of the prey base caused by commercial fisheries. They noted that the decline in birth rates compromises the recovery of the species, and it does not appear that the current fishing restrictions are adequate to address it. They also pointed out that lower birthrates are seen throughout the Western DPS, but the interim rule implemented no changes in management to address the broader problem. In their view the BiOp also failed to explain why management actions were not taken to address the failure of the sea lion population in the central Gulf to increase. Finally, they also encouraged NMFS to consider ecosystem needs of predators such as SSL explicitly in fisheries management decisions.

Oceana, Ocean conservancy, Greenpeace USA and World Wildlife Fund tabled comments that they had presented to NMFS prior to the release of the BiOp. Although stating that NMFS and the North Pacific Fishery Management Council have taken important steps to further sustainable fisheries, they were concerned that NMFS continued to authorize fisheries that removed large quantities of important SSL prey from the ocean. They believed that the continued decline and failure to recover the population of SSL was clear evidence that those fisheries, as currently managed, are not sustainable. Finally, they believe that there is an overwhelming weight of the scientific evidence that the groundfish fisheries are likely contributing to the continued decline and failure to recover.

Overall Conclusions and Recommendations

The BiOp brings together a large amount of information in attempting to understand the extent to which fisheries for several groundfish species, particularly in the AI, may compete with SSL for food. On the basis of a weight-of-evidence argument, the BiOp concludes that fisheries for Atka mackerel, Pacific cod and Alaska pollock are likely to jeopardize the continued survival or adversely modify critical habitat of the WDPS of SSL. It is my opinion that this determination is not supported by the evidence presented in the BiOp, and the new information reviewed at the panel meeting in Seattle. There is no direct evidence that by removing fish, these fisheries compete with the western population of SSL in the central and western Aleutians or elsewhere. Harvest rates for Atka mackerel are too low and the fraction of the Pacific cod stock in these areas is too small for a fishery on these species to result in nutritional stress. The only indirect evidence advanced to support the effects of fishing on SSL is the suggestion of reduced natality as a result of nutritional stress, also noted by Boyd (2010a). The weigh-of-evidence argument for JAM rests on speculation.

I found that distilling the evidence for the weight-of-evidence argument for JAM was difficult given the sheer length and redundant nature of the text. Therefore, I **recommend** that future BiOps be limited to 250 pages with relevant figures and tables included in the text. Length does not strengthen the case for or against the determination of JAM.

In the absence of compelling direct evidence, a weight-of-evidence approach was used in the BiOp to develop arguments in favour of one hypothesis (i.e., competitive fishery-interaction) over others. Apart from the schematic framework presented in Chapter 4, which did not appear to be used, it was difficult to determine exactly what the weigh-of-evidence was. In evaluating scientific evidence, it is important to be explicit about the assumptions and sources of uncertainty associated with the elements of the argument. In other words, there needs to be a critical and unbiased evaluation of the evidence. To a large extent, the BiOp fails in this regard. Results from studies are routinely presented without providing any indication of factors that might limit their interpretation.

Although there are many examples, the presentation of SSL diet composition and the identification of important prey species serve to highlight this failure. Accurate estimates of the energy contribution of prey to the diet of SSL are central to testing or building a weigh-of-evidence argument for the competitive fishery-interaction hypothesis. The metric used (i.e., FO) to estimate importance of SSL prey in the diet is known to be unreliable, overestimating prey with robust hard parts (e.g., Pacific cod) and underestimating those with fragile ones (e.g., salmons, herring). Yet throughout the BiOp, FO estimates of importance are used without qualification. As a consequence, it is almost certain that conclusions about the species composition of SSL diets are seriously biased and that the importance of pollock, Pacific cod and Atka mackerel as prey of SSL have been overestimated. I **recommend** that NMFS explore better ways (e.g., Tollit *et al.* 2003, 2007) to express the diet of SSL to reduce, in so far as possible, the biases caused by the partial and complete digestion of prey hard parts. Further research on the use of quantitative fatty acid and prey-DNA methods might also prove valuable.

NMFS and others have made considerable progress in understanding movements and the spatial and temporal distribution of SSL. Nevertheless, until recently, studies have focussed mainly on juveniles, and typically have collected only several months of data during summer. Improvements of tag performance and attachment methods are permitting longer records of movements which should result in a better understanding of habitat use. Recent efforts by NMFS to develop techniques to safely capture and handle adult females should help fill an important gap. Along with diet, the spatial ecology of SSL is central to understanding how SSL response to environmental variability and to anthropogenic stressors. Nevertheless, although hundreds of SSL that have been satellite tagged, a synthetic, quantitative analysis of SSL movement data has yet to be completed. I **recommend** that a quantitative analysis be conducted with the twin objectives of providing the best description of the use of space by the SSL population and guiding future movement studies.

Population monitoring, the estimation of vital rates, and population modelling provide the information needed to understand how SSL respond to environmental change and human impacts. Research aimed at extracting more information from SSL counts should be encouraged. But it is important to gain a better understanding of the statistical properties of counts and ratios derived from those counts. The BiOp makes strong inferences about the ratio of pups to non-pups, for example about natality, without any indication of the sampling error or the sources of bias and how those biases might change over space and time. As population monitoring is at the core of the SSL program, I **recommend** research to better understand the statistical properties of SSL count data and to test the hypothesis that pup to non-pup ratios is a proxy for natality. Efforts by NMFS to establish programs in the AI to estimate vital rates are important. Given the importance attached to inferences about changes in vital rates over space and time, I **recommend** the initiation and continuation of long-term, mark-resighting research to empirically estimate vital rates in several regions of the western population.

Initial results from LHX tags indicate that it should be possible to obtain independent estimates of survival rates and to make strong inferences on the sources of mortality in free-ranging SSL. These vital rate data could be quite important testing alternative hypotheses about the factors influencing SSL dynamics. Therefore, I **recommend** further development of the LHX tag and implantation techniques to permit it to be used throughout the range of SSL. Efforts to enhance the tag to estimate the reproductive status of females should also be pursued.

Although contemplated and even planned, a manipulative field experiment to test the hypothesis that fishery removals contribute to declining trends in SSL has not been done. In lieu of experiments, a number of studies now have tested for negative correlations as evidence for a fishery effect on SSL. Few negative correlations have been found and the overwhelming finding is that there is no detectable effect of fishing variables on SSL trends. Although these studies have used somewhat different methods, they are not independent as they use the same SSL count data and similar fisheries data on catches and effort. Nevertheless, they constitute the best test of the hypothesis at the present time.

Experience over the last several decades, involving quite considerable field effort, has shown that without some form of experimentation we have little hope of rigorously testing the "competitive-interaction" or alternative hypotheses that have been advanced. Attempting to test alternative hypotheses using multiple response variables has been largely uninformative because so many of the response variables are predicted to respond in a similar way under different hypotheses (see Bowen *et al.* 2001, NMFS 2008, BiOp Table 3.17). Without experimentation of some form, speculative qualitative statements, made weaker by the frequent use of "likely", "could", and "possibly", will be continue to form the weigh-of-evidence for fishery effects. However, there are now regions within the western population that are either roughly stable or increasing, but mitigation measures remain in place throughout the population. Continued increases of SSL counts in these regions will raise the quite reasonable question as to when mitigation measure might be relaxed or removed in these areas. Therefore, I **recommend** that NMFS give serious consideration to a field experiment whereby mitigation measures are selectively removed from some areas and not others that are stable or increasing. SSL counts and estimates of vital rates could be used to test if the impact of removing measures. Such an experiment will not be easy, will take time, and will require careful planning in terms of statistical power and confounding variables (see below). Nevertheless, now that several regions are experiencing sustained population growth, it may be time to revisit the value of conducting a field experiment.

As noted throughout the BiOp, and elsewhere (e.g. Trites and Larkin 1992, Bowen *et al.* 2001, NRC 2003, Trites *et al.* 2006, NMFS 2008), multiple factors undoubtedly were responsible for causing the decline of SSL. Direct mortality from entanglement in fishing gear and shooting were important sources of mortality prior to the 1990s, after which these sources of mortality were largely eliminated. Bottom-up forcing (i.e. climate regime shifts) has had significant impacts on the structure of the ecosystems that support SSL (reviewed by Trites and Donnelly 2003, Trites *et al.* 2006) and could have affected the food available to SSL. There are several hypotheses for how those changes in food availability might have negatively affected SSL (Trites *et al.* 2006), but we do not know if they actually occurred. We also know that transient killer whales eat SSL and that there are sufficient numbers of transients (Durban *et al.* 2010) to have population-level effects on SSL (e.g., Williams *et al.* 2004). We do not know if killer whale predation occurred at a rate sufficient to account for the observed declines. Although we do not what role these factors have played in the declines of SSL, it will be important to attempt to account for these factors if the decision is made to conduct field experiments to test the competitive fishery-interaction hypothesis.

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Appendix 2: A copy of the CIE Statement of Work

Attachment A: Statement of Work for Dr. Don Bowen

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.

<u>Prior to the Peer Review</u>: 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
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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):

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.

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:

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?

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.

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)?

5. 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).

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?

    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.

Tasks specific to Chapter 2 (panel review meeting):

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.

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.

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.

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

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Appendix 3: A list of persons and organizations participating in the panel review meeting, August 1-2, 2012.

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
	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
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

Appenidx 4: Agenda of the Seattle panel review meeting.

AGENDA

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

1. SSL abundance, vital rates, telemetry data, food habits (Tom Gelatt)

[Break]

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 available 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, Greenpeace, and the World Wildlife Foundation)

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