

**A Review of the May 2007
Draft Revised Recovery Plan for the Steller sea lion
(*Eumatopus jubatus*)
conducted for the Center for Independent Experts**

by

Professor John Harwood

29 June 2007

1. Executive Summary

1a. Impetus and goals for the review

The first Steller sea lion (SSL) Recovery Plan was completed in 1992 and provided recovery guidance to the National Marine Fisheries Service (NMFS) for the species. NMFS organized a new SSL Recovery Team in January 2002, and charged this Team with writing a revised Plan to reflect the current view of stock structure and the differences in stock status under the ESA. The Team completed its draft of the second Plan in February 2006. NMFS has since revised the Plan, and a new document, dated May 2007, was presented to the Center for Independent Experts (CIE) for an additional peer review. This report forms part of that peer review process.

1b. Main conclusions and recommendations

- Does the Plan thoroughly describe what is known about the potential threats to both the eastern and western populations of Steller sea lion? It does provide a thorough description of what is known about the potential threats to both populations. However, the way this part of the document has been written makes it hard to assess the weight of available evidence relating to each threat.
- Is the ecological and biological information presented in the Plan adequate, thorough and scientifically defensible? Yes, but more discussion of the current distinction between the two populations would be desirable.
- Does the Plan adequately present an ecologically and biologically defensible recovery strategy for the eastern and western populations of Steller sea lions? Not for the western population, because the factors involved in the recent decline have not been identified. Although only two of the four threats (Competition with Fisheries and Toxic Chemicals) identified as potentially high or medium in the Plan can be affected by management, the likely effectiveness of current and proposed conservation measures can only be evaluated if the relative importance of all four threats is known.
- Are the recovery actions appropriate to meet recovery goals? Not for the western population, for the reasons given above.

- Are the recovery tasks appropriately prioritized? Not for the western population, where the proposed recovery tasks seem to represent little more than a continuation of research activities that have been conducted for the last decade.
- Does the information in the Plan appropriately support the recovery criteria described in the Plan? Not for the western population. There is clear evidence in the Plan that this population may still have a relatively high probability of falling below the threshold identified by the Recovery Team, even when the criteria for a revised listing have been met. The Demographic Criteria for downlisting to threatened status for this population should to be rewritten in unambiguous language. The criteria for delisting of the eastern population are supported by the information in the Plan. The available data indicate that this population will probably meet these criteria in the near future.

2. Introduction

2a. Background

The first Steller sea lion Recovery Plan was completed in 1992 and provided recovery guidance to the National Marine Fisheries Service for the species, which at that time was listed range-wide as *threatened* under the Endangered Species Act. In 1997, NMFS recognized two Distinct Population Segments (DPS) of SSL on the basis of genetic evidence and population trends. The **western DPS** was relisted as *endangered*, whereas the **eastern DPS** retained the original listing of *threatened*.

NMFS organized a new SSL Recovery Team in January 2002, and charged this Team with writing a revised Plan to reflect the current view of stock structure and the differences in stock status under the ESA. The Team completed its draft of the second Plan in February 2006, when it was reviewed by five highly qualified experts. A revised Plan was submitted to NMFS and for public review in May 2006. Detailed comments were received from 18 parties or individuals. NMFS revised the Plan again, and a new document (NMFS 2007), dated May 2007, was presented to the Center for Independent Experts (CIE) for an additional peer review. This report forms part of that peer review process.

2b. Terms of Reference

The CIE reviewers were asked to focus on and address the following questions in their reports:

- Does the Plan thoroughly describe what is known about potential threats to both the eastern and western populations of Steller sea lion? Are there additional significant threats to the species? Does the evidence presented in the Recovery Plan support the threats assessment?
- Is the ecological and biological information presented in the Plan adequate, thorough, and scientifically defensible?
- Does the Plan adequately present an ecologically and biologically defensible recovery strategy for the eastern and western populations of Steller sea lion? Describe any shortcomings in the recovery strategy.

- Are the recovery actions described within the Plan appropriate to meet recovery goals? Are the recovery actions consistent with the Steller sea lion life history information, population dynamics and threats assessment presented in the Plan? Are there other recovery actions that have not been included in the Plan that should be included to achieve recovery?
- Are the recovery tasks in the Plan’s Implementation Schedule appropriately prioritized to facilitate recovery?
- Does the information in the Plan appropriately support the recovery criteria described in the Plan? Are the recovery criteria consistent with and do they meet the requirement of the ESA to ensure the conservation of the species (i.e., recovery and ultimate delisting: “conservation” as defined in the ESA 16 USC § 1532 (3))?

It is not easy to address these Terms of Reference (ToR) using the standard headings for CIE reports (Summary of Available Information, Review of Information used in the Assessment, Review of the Assessment Results, Review of Scientific Advice, Recommendations, Implications), and I have therefore adapted those headings to correspond more closely with the ToR. Their relevance to the ToR is indicated in brackets after each section heading.

3. Summary and Review of Available Information (Is the ecological and biological information presented in the Plan adequate, thorough, and scientifically defensible?)

3a. Overview of Recovery Plan

The Recovery Plan is divided into seven chapters:

- I. A general review of the ecology and biology of SSL. Under Feeding Ecology it includes a review of some of the evidence relating to one of the potential threats for SSL (Nutritional Stress) and, rather strangely (because critical reviews of other hypotheses are not found in this chapter), a section headed “Rejection of the Junk Food Hypothesis”. Finally there is a short section headed “Ecosystem Interactions” which contains brief paragraphs about the physical characteristics of the North Pacific, and some short statements about the potential complexity of the relationship between SSL and the other components of their ecosystem
- II. A short review of the conservation methods that have been undertaken to reduce already identified threats to SSL.
- III. A section entitled “Factors potentially influencing the population” which is essentially a description of potential threats.
- IV. The Recovery Team’s evaluation of the perceived importance of the threats identified in section III.
- V. A Recovery Plan for the western DPS.
- VI. The equivalent of Section IV for the eastern DPS.
- VII. A Recovery Plan for the eastern DPS.

In addition, there is an extensive Appendix that describes Population Viability Analyses for both the western and eastern DPS commissioned from Professor Dan

Goodman, and a valuable additional Appendix (also prepared by Professor Goodman) that deals with the concepts of density dependence and carrying capacity in the context of SSL population dynamics.

The style of the Recovery Plan is highly variable. This makes it rather difficult to read, because arguments and discussions are presented in many different ways. As a result, it is often impossible to understand how the Recovery Team arrived at a consensus view, or if such a consensus actually existed. In addition, many of the key references are not included in section VIII (LITERATURE CITED). This is a particular problem for papers relating to the effects of toxic substances (at least 10 of the papers cited on pp. 97-98 are not included in Chapter VIII), but also for the section on nutritional stress (Frid et al (2006), Rea et al (2003), Fay & Punt (2006) cited on p. 38 and p. 40), and even killer whale predation, where the key reference (Maniscalco et al (cited as “in press”, but published in April 2007)) is not included in Chapter VIII. This has also made it difficult to understand the basis for some of the Recovery Team’s conclusions. Finally, technical scientific terms are often used rather carelessly. This is particularly true of the terms “carrying capacity” and “density dependence”, as discussed in more detail in section 4a. Another example is the statement on p. 81 that there are “refuting studies” of the “Sequential Megafaunal Collapse” hypothesis. I’m no fan of this hypothesis, but it can’t be “refuted” on the basis of the available evidence. There have been no “studies” relating to this hypothesis. Rather, a series of papers have suggested that, when the available evidence is considered in detail, it provides less support for the hypothesis than its proponents have suggested.

3b. The ecological and biological information presented in the Plan
In this part of my report I focus on Chapter I.

The Plan does provide a comprehensive review of what is currently known about the ecology of SSL in both the western and eastern DPS. However, the way in which the available information is presented and reviewed is highly variable. I think readers are likely to be confused by the combination of fact, hypothesis and opinion that characterizes some subsections of this chapter. This is particularly true of the section on nutritional stress, which seems to be more appropriate for Chapter III. It includes a “rejection” of the junk food hypothesis. The junk food hypothesis suggests that the dominant role of pollock in the Bering Sea and Gulf of Alaska in recent decades has played a role in the decline of SSL because the nutritional value of this fish is lower than some other prey species. To my mind, this section actually makes a rather strong case that 1 year-old SSL may find it difficult to consume the quantities of pollock that are required for growth. This may result in an extended lactation period for their mothers, with consequent effects for natality rates in the local population.

I would also have liked to have seen more discussion about the current divisions between the two DPSs. Although the genetic evidence for historical separation is strong, the fact that females from the western DPS have been observed breeding in the eastern DPS suggests that some introgression is now occurring. In addition, I am surprised that the Recovery Team did not comment on the different ratio of pup counts to non-pup counts in the two DPSs. In the western DPS non-pup counts are 2-2.5x higher than pup counts, indicating that a significant proportion of the population is inaccessible to counting at the time of the survey (indeed this proportion is estimated by Holmes et al (in press)). The same is true in California.

But in Alaska and British Columbia, non-pup counts are 3-4x the pup counts, and total counts (pups + non-pups) are very close to the available estimates of total population size. One explanation of this is that all members of the eastern are hauled out at the time of survey, but this is unheard of for any pinniped. A more likely explanation is that a significant number of non-pups from the western DPS are using haulouts and rookeries in the eastern DPS. I would suggest that the proposed review of the listing of the eastern DPS includes a thorough analysis of the current distinction between the two DPSs.

3c. Effectiveness of current conservation measures

In this section, I will focus on Chapter II of NMFS (2007).

In general, current measures appear to have been successful in reducing many of the threats to the conservation status of the western DPS. However, I am not convinced by the arguments assembled here to show that conservation measures implemented since the late 1990's have provided greater protection for areas of critical habitat. Chapter II (p. 76) states that "The implementation of conservation measures in the 1990s and early 2000s are **correlated** (my bold text) with a reduction in the rate of decline of the western DPS", and this view is reiterated in the Executive Summary ("conservation measures implemented since 1990 are positively affecting the recovery ..."). However, there is no time series of conservation measures that can be correlated, in a statistical sense, with the rate of change of the western DPS. I think what the Team should have said is the introduction of additional conservation measures **coincided** with changes in the rate of change of the western DPS, because there is no evidence that these conservation measures actually altered the impacts of commercial fishing on the availability of prey to SSL. Indeed, both NMFS (2003) and the figures at the end of Chapter III indicate that these measures have had a rather small impact on the proportion of total fisheries catch that is taken within SSL critical habitat. Both NRC (2003) and NMFS (2003) note that the changes in counts that have been observed since 1999 are not entirely consistent with those that would be predicted if the main threat was Competition with Fisheries, because the largest changes have been in the counts of non-pups rather than pups. It is surprising that the Recovery Team did not comment on this.

I am also unconvinced by the methods that NMFS has used to identify critical habitat from telemetry data. These are described in detail in NMFS (2003), but have been criticized elsewhere. For example, both Bowen et al (2001), and the three reviews prepared for the CIE (Boyd 2004, Hindell 2004, McConnell 2004) are critical of the use of 6-hour dive summaries provided by the telemetry devices to identify foraging areas and therefore critical habitat. Again, I am surprised that the Recovery Plan does not comment on this. At least some of the satellite transmitters that have been deployed since 2003 do not suffer from this problem, and it would be highly informative to compare estimates of habitat use from these devices with those obtained up to 2003.

4. Review of Threat Assessment (Does the Plan thoroughly describe and identify the potential threats to SSL? Does the evidence presented in the Recovery Plan support the threats assessment?)

This section of my report focuses on Chapters III, IV and VI of the Recovery Plan.

4a. *Threat identification*

Chapter III is primarily concerned with threat identification. It does provide a thorough description of what is known about the potential threats to both DPS. However, the way this part of the document has been written makes it hard to assess the weight of available evidence relating to each threat. This may be because the Recovery Team included proponents and opponents of most of the major hypotheses for the decline of the western DPS. As a result, most of the sections consist of a statement in favor of the relevant hypothesis followed by a series of comments that appear to contradict the opening statement. There seems to have been little success in achieving a consensus within the Team on each of these issues, and the reader is left to evaluate a series of contradictory opinions.

This chapter frequently refers to fluctuations in “the carrying capacity of the North Pacific” (p. 82, p. 89, repeated on p. 119). However, as Goodman points out in Appendix B, carrying capacity is not a simple property of the environment, rather it is a consequence of the interaction between habitat quality, resource availability and predation pressure. In this section, however, it appears to be used to mean resource availability. Appendix B makes it clear that the population consequences of such changes cannot be evaluated in isolation from the other factors involved in determining equilibrium population size.

I was expecting to find some justification in this Chapter for the statement in the Executive Summary that “During this period (the 1980s), mortality incidental to commercial fishing was thought to contribute to perhaps as much as 25% of the observed decline” (p. 1), but I could not. The only potential source for this figure appears to be in the Appendix, where Table 2 lists estimates of incidental catch and entanglement, but these amount to less than 17,000 animals over the entire period 1977-1989. During this time the population declined by over 100,000 individuals.

4b. *Threat assessment*

Chapter IV describes the Recovery Team’s conclusions about the relative importance of the different threats identified in Chapter III. The total disagreement within the Team about the classification of the importance of Environmental Variability and Competition with Fisheries is clearly described. But an apparent difference of opinion about the ranking of Predation by Killer Whales is not well documented. The current version of the Recovery Plan says “The team had also ranked killer whale predation as a “potentially high” threat. However, after public review and comment, and as additional scientific information became available (e.g. Maniscalco et al in press), **NMFS concluded** (my bold text) that ... a Medium ranking was warranted” (p. 111). This seems extraordinary given the claim in the Executive Summary that the Plan was unanimously endorsed, and the fact that there is extensive discussion of the Maniscalco et al manuscript on p. 85 and p. 89 (unless these paragraphs have also been added in the revision to the report since February 2006). Given that the Maniscalco et al manuscript was received by the journal in which it has now been published on 13 April 2006, and that one of its authors (Atkinson) was a member of the Recovery Team, it seems extraordinarily unlikely that the information in this ms was not available to the Team when it decided to classify this threat as potentially high.

I could also find absolutely no justification for classifying Toxic Substances as a Medium threat (over Infectious diseases, for example). The Plan concludes that levels of these substances in SSL are relatively low, and provides no evidence of their

effects. The statement that “toxic substances may have indirect effects on ... vital rates” (p. 115) is true for any marine mammal population anywhere in the world.

The final pages of this Chapter imply that the Team agreed that bottom-up threats are now more important to the western DPS than top-down ones (p. 119). However, this could not have been true when some members of the Team wanted Killer Whale Predation classified as a high threat.

In addition, this same section correctly states that these conclusions are “in contrast to ... NRC (2003) which favored top-down controls as the primary factor”, but it goes on to say that “Much of the evidence considered here was not available to the NRC in 2002 ...” (p. 119). This statement does not bear close examination. The main “new” evidence in support of the bottom-up approach comes from Holmes et al (in press). However, this manuscript is essentially an extension of the analysis in Holmes & York (2003), with broadly similar conclusions (that natality rates in the central Gulf of Alaska have continued to decline). York made a presentation of her work to the NRC and the manuscript version of Holmes & York (2003) is cited by Bowen et al (2001). So, their work must have been known to the NRC panel. Although the analyses reported in Holmes & York (2003) and Holmes et al (in press) are excellent pieces of work, they are based on data from only one set of trend counts and their conclusions do not necessarily apply across the western DPS (although Holmes et al. do suggest that “declining birth rate may be problem across the Gulf of Alaska”). In addition, Holmes et al. admit that there is considerable statistical debate about the most appropriate way to compare the performance of complex demographic models such as theirs. They admit that their approach favors models with relatively large numbers of parameters. A different approach might result in different conclusions about trends in natality in the Gulf of Alaska. Thus, I think it is premature to assume that their conclusions apply to the entire western DPS.

By contrast, evidence in support of the Killer Whale Predation hypothesis seems to have accumulated since the NRC report, particularly the new data on the abundance of transient killer whales and their potential removals (Williams et al 2004). The fact that Maniscalco et al. (2007) observed lower than expected mortality around a small number of SSL sites in the northern Gulf of Alaska does not seem to me to provide critical evidence for or against this hypothesis. The fact that current estimates of killer whale predation “are lower than the 20% predation rate” (p. 89) is irrelevant. The important question is: has the mortality imposed by killer whales on SSL in the western DPS changed over the last two decades (i.e. could it have contributed to the continuing decline after 1990)? The evidence for or against this is still lacking.

5. Review of Scientific Advice (Does the Plan provide a defensible recovery strategy for both DPS of SSL? Are the recovery actions described within the Plan appropriate to meet recovery goals? Are the recovery tasks appropriately prioritized to facilitate recovery? Does the information in the Plan appropriately support the recovery criteria described in the Plan and do these meet the requirement of the ESA?)

In this section, I focus on Sections V and VII of the Recovery Plan.

5a. *Recovery criteria for the western DPS*

Initially, the Team used a PVA approach to assess extinction risks for the western DPS. As part of the specification for this analysis, the Team had to define the threshold level of risk that would trigger transition from endangered to threatened status under the ESA. They decided on a 1% chance of quasi-extinction in the next 100 years, where quasi-extinction was defined as the population falling below 4,743 animals (equivalent to an effective population size of 1,000). The source for this threshold is a review of genetic criteria by Allendorf & Ryman (2002) – a reference that is also missing from Chapter VIII. The Recovery Plan claims that this is “a conservative estimate beyond which a significant additional genetic variation is not expected” (p. 129). However, the appropriate section in Allendorf & Ryman (2002) indicates that “there is current disagreement among geneticists regarding how large a population must be to maintain ‘normal’ amounts of additive genetic variation for quantitative traits ... suggestions for the effective sizes needed to retain evolutionary potential range from 500 to 5,000”. So, 1,000 individuals are hardly conservative. In the end, Goodman found that the western DPS was only likely to meet this criterion in the next 30 or so years if the extrinsic factors involved in the decline observed between 1985 and 1989 would never recur.

The Plan states that the Team “decided not to develop criteria based exclusively on the model. Numerous limitations ... and issues pointed out during public and peer review cast doubt on the utility of the PVA alone” (p. 132). The only public and peer reviews mentioned in NMFS (2007) occurred after February 2006 and, as far as I know, the entire Team has not met since then. I hope the PVA process was reviewed before this; otherwise it casts some doubt on the claim in the Executive Summary that the Plan had unanimous support. It should be noted that, rather than being pessimistic, as the Plan implies, Goodman’s calculation may present an optimistic view of the future of the western DPS. He was requested by the subgroup responsible for the PVA to include “a relative schedule of prey-competition fishery effects, expressed as instantaneous per capita mortality” (Recovery Plan Appendix) for the period 1968-2000. These equate to an additional mortality of up to 6% in some years. This is a substantial additional mortality for a population whose dynamics are known to be particularly sensitive to changes in mortality (see NRC 2003, for example). However, I can find no justification for these precise levels (or, indeed, any **quantified** fishery effects) anywhere in the Recovery Plan. If these mortalities were not included in Goodman’s calculations, the variations in population growth rate would have been even greater, and the risks of extinction would have been higher.

Following (?) its decision about the PVA, the Recovery Team used a “weight of evidence” (p. 133) approach to develop the demographic criterion that should be used to decide that the western DPS could be considered for reclassification as threatened. This is if “the population for the US region has increased (statistically significant) for 15 years on average, based on counts of non-pups” (p. 136). I do not understand what this definition actually means, but I suspect that the criterion is that the population should show an average annual rate of increase that is statistically different from zero over a 15 year period. It is worth noting that a population that showed a rapid rate of increase for the first 10 years but was declining in the later part of the 15 year period would satisfy this criterion, and would not necessarily have shown its ability to cope with environmental fluctuations (as the Plan implies). The Executive Summary suggests that a non-pup count of approximately 55,000 animals in 2015 would meet this criterion, but Goodman’s analysis suggests that a population

of more than 60,000 in 2014 would have a 25% risk of quasi-extinction. This was reduced to 2.55% if the 1985-89 decline were ignored, but the Recovery Plan provides strong arguments why this decline should not be ignored. It is therefore hard to reconcile the proposed criterion for reclassification with the Teams own definition of endangered status.

The criterion for delisting from the ESA is a 3% increase maintained over 30 years (equivalent to 107,000 animals in 2030). Goodman's analysis suggests that the risk of quasi-extinction for such a population is 9.7%. Again, it seems hard to justify that such a population is no longer threatened with extinction, since it does not even meet the Team's criterion for downlisting from endangered to threatened.

Although the Team was unable to decide whether the threat posed by competition with fisheries was high or low, the Recovery Factor Criteria relate almost entirely to the potential impact of fisheries (all of Factors A and B, most of Factor D). I understand that this threat, if real, could be reduced by effective management, whereas those from environmental variation and predation cannot. However, a much greater emphasis needs to be placed on the research required to assess the nature and magnitude of the threat from fisheries, and the impact of conservation measures on it, before there can be any confidence that the proposed Recovery Plan will actually have any effect on the risks of extinction for the western DPS..

5b. Recovery criteria for the eastern DPS

The criterion for delisting the eastern DPS is exactly the same as that for the western one: A consistent increase of 3% for 30 years. This appears uncontroversial because this DPS has shown none of the large variations in annual growth rate that have been observed in the western DPS. Given that the eastern DPS has shown a 3% increase since 1985, it is obviously time to reconsider its classification, as proposed by the Recovery Team. However, as noted in section 3b of this review, the current status of the two DPSs needs to be carefully considered.

5c. Recovery plan for the western DPS

Again, this seems entirely uncontroversial.

5d. Prioritization of recovery tasks for the western DPS

The Recovery Plan lists "78 actions that are needed to achieve recovery of the western DPS" (p. 4). Thirty-five of these are identified as essential (Priority 1), or of primary importance (Priority 2a) on the basis of "the descriptions and approach required in the NMFS interim Recovery Planning Guidance" (p. 176). However, the reader is given no guidance about how this was done or exactly what the relevant criteria were. There is certainly no indication as to how the extensive and expensive research programs relating to medium or low threats will contribute to recovery. I am particularly concerned that experimental research on the effectiveness of conservation measures in critical habitat (Plan Task 2.6.8), which was recommended by Bowen et al (2001) and by NRC (2003), and whose importance is heavily stressed in other parts of the Plan (especially on p. 75), does not have "essential" priority. Instead, it will not actually be implemented for at least 3 years and has a rather modest budget of around \$2 million over the next 5 years. This research is absolutely critical for distinguishing the two preferred hypotheses for the post-1990 decline of the western DPS and must, surely, have priority over all other Plan Tasks except population monitoring.

- 5e. *Recovery plan for the eastern DPS*
Again, this seems entirely uncontroversial.

6. Recommendations and Implications

The decision to convene a Recovery Team whose members held such diametrically opposite views on the main causes for the reduction in abundance of the western DPS of SSL was a bold, but high risk one. Although I applaud this risk-taking, I am not entirely convinced that the Team has succeeded in developing an ecologically and biologically defensible recovery strategy for the western DPS. The criteria for revisions to the listing of this DPS under the ESA appear to be too weak, by the Recovery Team's own standards. The Executive Summary clearly states "the primary factors associated with the decline during this period (from the 1990s onward) have not been identified" (p. 2). Until these factors have been identified, there is no guarantee that the proposed recovery actions will meet the recovery goals. The proposed recovery tasks do not appear to be particularly well designed to distinguish between the factors that the Recovery Team (and other review panels) have identified as likely to be most important. Rather, they resemble very closely the research projects that have been funded since 2000 and which have signally failed to quantify the relative importance of these factors. In my opinion, a much more focused approach is required.

7. Literature Cited

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Appendix 1
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- National Marine Fisheries Service 2007. Draft Revised Recovery Plan for the Steller sea lion (*Eumatopius jubatus*). National Marine Fisheries Service, Silver Spring, MD. 305pp

Appendix 2
Consulting Agreement between the University of Miami and Dr. John Harwood
STATEMENT OF WORK
June 14, 2007

The first Steller Sea Lion (SSL) Recovery Plan was completed in 1992 and provided recovery guidance to the National Marine Fisheries Service (NMFS) for the species, which at that time was listed range-wide as threatened.

NMFS organized a new SSL Recovery Team in January 2002, and charged the new Team with writing a revised Plan to reflect the current view of stock structure and the differences in stock status under the ESA (eastern Distinct Population Segment (DPS) listed as threatened, and western DPS listed as endangered). The Team completed its draft of the second Plan in February 2006, at which time the Team sought an external peer review from 5 highly qualified experts (see Attachment 1).

Upon receipt of the peer reviewer comments, the Team revised the Plan and submitted it to NMFS. NMFS released the Plan for public review in May 2006 and received detailed written comments from 18 parties or individuals. Based on these comments and those of the expert reviewers listed above, NMFS revised the Plan into the document being presented to the Center for Independent Experts (CIE) for an additional peer review (document dated May 2007).

The CIE experts' comments will assist NMFS in making recovery decisions for the Steller sea lion based upon the best scientific and commercial data available (as required by the Endangered Species Act of 1973, as amended).

Reviewer Requirements

The CIE shall provide three expert reviewers. Each reviewer's duties shall require a maximum of six days of effort, including time to read the relevant document and to produce an individual written report consisting of his/her comments and recommendations. No travel is required; each reviewer shall work from his/her home location. Each reviewer's report shall reflect his/her area(s) of expertise, and no consensus opinion (or report) will be required.

As a group, the panel of CIE reviewers must possess expertise in the areas listed below.

* Familiarity with relevant sections of the Endangered Species Act (http://www4.law.cornell.edu/uscode/html/uscode16/usc_sup_01_16_10_35.html), and as applicable, the Marine Mammal Protection Act, and related wildlife management legislation (e.g., NEPA).

In particular,

* Experience as a Recovery Team member, contributor, or reviewer of Recovery Plans developed for other listed species; as a current or recently retired employee of a federal or state agency holding a position implementing ESA regulations; or from an academic position that has focused on ESA statutes and implementation.

* In depth expertise in the biology and management of marine and/or other large mammals; specifically population dynamics, reproductive and foraging biology and physiological ecology.

At least two of the reviewers must have in-depth experience with the ESA and recovery plans, and one reviewer must have in-depth knowledge of marine mammals. Former reviewers and former SSL Recovery Team members and support staff shall be excluded from consideration as reviewers of this document. See Attachment 1, below.

Specific Reviewer Tasks and Schedule

The Alaska Region shall provide the CIE with copies of the May 2007 draft revised SSL Recovery Plan for the review, or a link to it, by May 31, 2007. Delay in meeting this schedule will result in a minimum of an equivalent delay in delivering the final CIE reviews. The document to be reviewed will be approximately 200 pages in length.

1. The CIE reviewers shall read and assess the May 2007 draft revised Steller Sea Lion (*Eumetopias jubatus*) Recovery Plan.
2. The CIE reviewers shall focus on and address the following questions in their review reports:
 - Does the Plan thoroughly describe what is known about potential threats to both the eastern and western populations of Steller sea lion? Are there additional significant threats to the species? Does the evidence presented in the Recovery Plan support the threats assessment?
 - Is the ecological and biological information presented in the Plan adequate, thorough, and scientifically defensible?
 - Does the Plan adequately present an ecologically and biologically defensible recovery strategy for the eastern and western populations of Steller sea lion? Describe any shortcomings in the recovery strategy.
 - Are the recovery actions described within the Plan appropriate to meet recovery goals? Are the recovery actions consistent with the SSL life history information, population dynamics and threats assessment presented in the Plan? Are there other recovery actions that have not been included in the Plan that should be included to achieve recovery?
 - Are the recovery tasks in the Plan's Implementation Schedule appropriately prioritized to facilitate recovery?
 - Does the information in the Plan appropriately support the recovery criteria described in the Plan? Are the recovery criteria consistent with and do they meet the requirement of the ESA to ensure the conservation of the species (i.e., recovery and ultimate delisting: "conservation" as defined in the ESA 16 USC § 1532 (3))?
3. No later than June 29, 2007 each CIE reviewer shall submit a written report¹ to the CIE that addresses the points in item 2 above. See Annex I for additional details on

¹ Each written report will undergo an internal CIE review before it is considered final.

the report outline. Each report shall be sent to Dr. David Die, via email at ddie@rsmas.miami.edu, and to Mr. Manoj Shivilani, via email at mshivilani@rsmas.miami.edu

Submission and Acceptance of CIE Reports

The CIE shall provide the final individual reviewer reports for review for compliance with this Statement of Work and approval by NOAA Fisheries to the COTR, Dr. Stephen K. Brown (Stephen.K.Brown@noaa.gov), no later than July 13, 2007. The COTR shall notify the CIE via e-mail regarding acceptance of the reviewers' reports. Following the COTR's approval, the CIE shall provide pdf format copies of the reviewers' reports to the COTR.