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promoting sustainable fisheries to feed the world

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February 26, 2011

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RE: RIN 0648-BA31 Comments on SSL Interim Final Rule BiOp and EA

Dear Dr. Balsiger,

On behalf of the Marine Conservation Alliance, I am pleased to submit the following comments on the *Final Interim Rule Biological Opinion (BiOp) for the Bering Sea and Aleutian Islands and Gulf of Alaska Groundfish Fisheries* and the accompanying *Environmental Assessment /Regulatory Impact Review (EA).* The Marine Conservation Alliance is a broad-based coalition representing approximately 70% of harvesting and processing capacity engaged in federal fishing activities in the Bering Sea, Aleutian Islands and Gulf of Alaska. Also among its membership are fishing communities who will also be impacted by implementation of this Interim Final Rule BiOp.

The National Marine Fisheries Service has drafted an Interim Final Rule BiOp that describes additional and significant fishery restrictions it has implemented in the Central and Western Aleutian Islands to protect Steller sea lions (SSL) in time for the 2011 fisheries. These additional restrictions were developed by NMFS based on its ESA Section 7 analysis and conclusion that current fishery restrictions in the Central and Western Aleutian Islands were insufficient to support SSL population recovery rates recommended as guidelines in the Steller Sea Lion (SSL) Recovery Plan.

The use of an Interim Final Rule to implement the new restrictions is an unusual regulatory vehicle that seems an attempt to comply with the Administrative Procedures Act (APA) and the National Environmental Policy Act (NEPA) while allowing immediate implementation of the Final RPAs in time for the 2011 groundfish fisheries. However, MCA remains concerned that this new vehicle may not comply with the procedural requirements of NEPA, APA and the

Magnuson Stevens Fishery Conservation Act. We do not feel the agency has adequately explained the basis for proceeding in this way toward a final rule nor explained how this approach addresses the requirements of MSA, NEPA and APA. However, we hope the agency will take seriously use of the term "interim" which has been incorporated into this unusual decision-making process. It is our hope that the "interim" period will be used to improve the quality of these actions so that they are more responsive to the best scientific information available, various economic consequences as well as public and peer review. For these reasons, we support the letter sent from the North Pacific Fishery Management Council to NMFS on December 23, 2010.

We are particularly disappointed and concerned that the agency did not include analysis of the Council's proposed RPA which was the only RPA crafted as part of a transparent public process. We believe the Council's RPA proposal to be a viable and reasonable alternative approach and find it difficult to comprehend how exclusion of it in the range of analyzed alternatives meets the requirements of NEPA. We also remain concerned that alternative hypotheses examining the cause of declining SSL populations in the Western and Central Aleutian Islands were not adequately considered.

Please find attached to these comments on the Interim Final Rule BiOp our earlier comments on the Draft BiOp. Because few of the issues MCA identified in its earlier comments on the Draft BiOp were addressed in the Interim Final Rule BiOp, we believe those issues remain germane and so should become part of the public comment record on the Final Interim Rule BiOp. We have also included as an attachment, comments from Dr. Ian Boyd on both the Draft BiOp and changes made in the Final Interim Rule BiOp which we wish to include as an integral part of our submission.

Though we disagree with agency's conclusions as described in the Interim Final Rule BiOp, we appreciate the hard work NMFS has invested in its preparation. We look forward to working with NMFS in the further review and modification of the SSL BiOp and RPAs as part of the Interim Rule process.

Finally, we appreciate that the agency extended the public comment period 45 days. Please see our specific comments below. The approach taken in our comments is to note differences in the Draft and Final Interim BiOps and to raise continuing concerns.

<u>Specific Comments on the BiOp:</u>

1) <u>Issues not addressed:</u>

- MCA's comments on the Draft BiOp addressed many issues including nutritional stress, impacts on reproduction rates, killer whale predation, the use of recovery criteria guidelines as threshold rules in making a Jeopardy and Adverse Modification Section 7 determination, the use of many new sub-population definitions rather than larger biological sub-populations in assessing SSL population trends and the use of closures outside critical habitat. In drafting the Final Interim BiOp, NMFS did not address these issues. So our earlier comments on the draft BiOp stand as originally drafted and are attached here to be included into the public comment record on the Interim Final BiOp.

2) New information not included (or adequately considered) in the BiOp:

- Final BiOp continues to dismiss Dr. Ian Boyd's PVA analysis by stating it depends on there being only one DPS (see pages 95-96). Previous MCA comments still apply here.

- Trites et al 2010 (exploring relationship between Atka mackerel fishery removals and SSL sub-population growth) is represented as 'inconclusive' (page 265) when, in fact, the study found NO evidence of a relationship between Atka mackerel fishing and trends in SSL numbers with an exhaustive set of spatial groupings.

- The latest SSL survey (2010), as presented to the NPFMC, shows EDPS-tagged females breeding at sites in the WDPS. This is new information which the AFSC itself says 'will make determination of (the) overall Western stock trend more difficult.' (Note this is a comment attached to the 2010 SSL survey slide in Bill Karp's presentation). At the very least, this brings into question the issue of site fidelity, which makes trend data highly suspect.

- The final BiOp does not include new fisheries survey data from the 2010 Aleutian Islands survey, which shows a marked increase in Atka mackerel biomass particularly in Area 543.

- Subsistence. The BiOp states, "*The most recent year for which subsistence data have been summarized is 2007*." ADFG and the Alaska Native Harbor Seal Commission published *Technical Paper No 347, The Subsistence Harvest of Harbor Seals and Sea Lions by Alaska Natives in 2008 (Oct 2009)*. We further note that in Atka (Tech Paper 347): "*Figure 33 illustrates trends at Atka, one of the few communities in the state reporting increased sea lion harvests in recent years. As shown in Figure 33, the numbers of households hunting sea lions at Atka increased from 8 to 19 households from 1992 to 2003, and the numbers of households successfully harvesting sea lions increased from 6 to 18 households. Hunting households then fell to 10 households in 2008. Success rates for households show no clear trends: since 1992, annual mean harvests per household have ranged from 2.0 to 7.1 sea lions per successful hunter at Atka. The sea lion harvests of 35 animals in 2008 were lower than the recent 5-year average of 48 animals (2004-2008)*." In the Central Aleutian Area, NMFS reports an 11% decline from 2000-2008. A harvest of 48 animals/year by Atka represents a significant portion of the SSL count for Atka Island which has declined from 387 in 2004 to 71 in 2009.

- Killer Whale predation. The BiOp contains this statement "*Kuker and Barrett-Lennard (2010) reexamined data on sea otter mortalities and population decline in the AI region and the causative killer whale hypothesis, and found no empirical data to support that killer whales caused the decline.*" USFWS seems to have a different opinion in the Southwest Alaska Distinct Population Segment of the Northern Sea Otter Draft Recovery Plan (Aug 2010) "*the cause of the overall decline is not known with certainty, but the weight of evidence points to increased predation, most likely by the killer whale (Orcinus* *orca), as the most likely cause*." MCA's concerns about the agency's consideration of predation were not addressed in the Final Interim Rule BiOp. We remain especially concerned about the apparent significant (75%) undercount of transient killer whales in the GOA/BS/AI and the impact on predation impacts.

- The agency's lack of serious consideration of the killer whale predation hypothesis in the Interim Final Rule BiOp is troubling. Among other studies in support of this hypothesis, the National Research Council concluded in its study "The Decline of the Steller Sea Lion in Alaska Waters" (2003) that top-down sources of mortality such as killer whale predation appear to pose the greatest threat to recovery of Steller sea lion populations. "*Existing data on the current phase of the decline indicate that bottom-up hypotheses resulting in food limitation are unlikely to represent the primary threat to Steller sea lion recovery. Although no hypotheses can be excluded based on existing data, top-down sources of mortality appear to pose the greatest threat to the current population*." It is disconcerting that NMFS would seemingly disregard the conclusions of the nation's most prestigious science panel, the NRC, an arm of the National Academy of Sciences. We wish to introduce the NRC study and its finding into the public comment record on the Interim Final Rule BiOp by reference here. We have also included a summary of the NRC findings as attachment (2) to these comments.

3) <u>Untested methodology/uneven treatment/lack of responsiveness to SSC:</u>

- The SSC criticized the use of 'rookery cluster areas' (RCAs) in the draft BiOp (..'available data, particularly for patchily distributed Atka mackerel abundance, do not support apportionment at the scale of the RCAs.' SSC August minutes, page 6). The SSC was also critical of the 'footprint' analysis (cited as AFSC 2010a) which used non-standard methods to apportion fishery biomass by area. The final BiOp acknowledges these criticisms (page 283) and re-calculates the biomass by area based on methods approved by stock scientists, and states on page 284 that this new data 'replace(s) fish biomass estimates and harvest rates by RCA contained in the August 2, 2010 draft Biological Opinion.' However, the only real change in the BiOp appears to be insertion of the new numbers in the tables; the rest of the document continues to refer to high harvest rates by RCA (see in particular section 5.1.6.4) with few changes in the wording or the conclusions.

- The new fishery biomass calculations (5.1.4.1) result in dramatically lower harvest rates in the AI (e.g., 7% for Atka mackerel in 543 in 2008, compared to 27% reported in the draft BiOp). As above, NMFS appears to dismiss this new data and makes no changes to the conclusions.

- Additionally, the new forage ratios (page 298) are significantly higher for the AI in general and the WAI in particular. The document even states (page 299) that 'Forage ratios by fishery management regions are not correlated with SSL trends in abundance aggregated at this same scale.' However, on page 293 the final BiOp re-states the conclusion of the draft BiOp that '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.' Again, while the final BiOp does present fishery biomass as per standard protocol, and the new data shows a completely different situation than what was represented in the draft BiOp, there is no change to the final conclusions.

4) Basis for measures outside CH is flawed:

- Previous comments still apply. The telemetered animals outside CH (Fadley 201) were juvenile males in deep-water areas where prey is likely small pelagics. The 'platform of opportunity' study (Boor, 2010) shows general dependence on inside CH, with sightings outside CH largely in deep-water basins. Public comment on this issue seems to have been ignored.

5) Use of data:

- New data (breeding females from EDPS in the WDPS) and better use of existing data (re-calculation of forage ratios and harvest rates using AFSC protocol) both indicate the basic assumptions of the draft BiOp are flawed. NMFS appears to be unconcerned with the fact that the new data contradicts its conclusions in the draft BiOp, and aside from plugging new numbers in to the tables, has made no changes to the conclusions. Without adequately considering the new data, it gives the appearance that the BiOp's conclusions have been pre-determined.

Specific Comments on the EA:

1) The Jeopardy and Adverse Modification (JAM) determination and accompanying RPAs are significant and highly controversial. For this reason, MCA continues to believe that NEPA requires an Environmental Impact Statement rather than an Environmental Assessment.

2) The authors of the Nov. 2010 EA/RIR added substantial amounts of analysis to the Aug. draft. The new draft is generally responsive to the comments of the SSC. However, many issues of concern raised by the public and the Council remain unaddressed.

3) The central flaw of the current EA/RIR is not the analysis of the alternatives, but rather the alternatives themselves and the lack of analysis of "Alternatives Considered and Not Further Analyzed." The Council's proposed alternative received no analysis. The only explanation provided for rejecting the alternative was two sentences on page 2-38: *"The remaining features of the Council recommendations were found to not meet the performance standards of the final FMP BiOp (NMFS 2010a). The primary reasons for not meeting the performance standards are that the Council recommendation would allow amounts of Atka mackerel and Pacific cod harvests in a manner similar to historical practices or at amounts greater than allowed by the performance standards (NMFS 2010a)."* Clearly the Council's proposed alternative was significantly different than "historical practices or amounts," and should have been fully analyzed in the EA/RIR. The EA/RIR correctly states on page 1-9 that: *"All fisheries management* *actions need to be developed with consideration of the Magnuson-Stevens Act national standards, including the proposed action analyzed in this EA."*

4) Section 11 purports to evaluate consistency with the National Standards in a couple paragraphs. On page 11-2 it states: *"This action takes into consideration the requirements of national standards 5, 6, 7, and 8. The differences among fishery participants, their locations, fishing practices used for harvesting Atka mackerel and Pacific cod, impacts on the various sectors, amounts and locations of catch and the dependence on these harvests were all considered in the development of the RPA. NMFS developed the RPA to provide as much fishing opportunity while balancing the need to ensure the groundfish fisheries could be implemented by January 1, 2011, in compliance with the ESA."*

5) National Standard 8 requires taking *"into account the importance of fishery resources to fishing communities in order to (A) provide for the sustained participation of such communities, and (B) to the extent practicable, minimize adverse economic impacts on such communities."* This BiOp fails to accomplish these requirements. The EA/RIR has extensive analysis of the impacts on Atka and Adak and acknowledges: *"Of all the communities discussed here, Adak may have the most at risk from this action."* However, there is no discussion in the EA/RIR of what was done to "*provide for sustained participation of…(or)…minimize adverse economic impacts on such communities*."

6) The EA estimates of "revenue at risk" are based on the portion of the harvest that has occurred in the areas to be closed under the RPAs. In the case of cod, this overlooks a likely negative impact of the RPAs. While the residual open area accounts for 40-60% of the catch depending on gear type, it is a far smaller portion of the "real estate" in terms of open area. The result will likely be increased concentration of the fleet, resulting in gear conflicts and crowding as displaced effort seeks to make up catch in the remaining open area.

7) One area where the EA/RIR did not meet the SSC's recommendations is acknowledged on page 10-150. The SSC requested that "*values reported for changes in revenues, costs, and nonmarket values… should be expressed in similar time frames*." The EA/RIR presents 20 pages of Willingness to Pay (WTP) analysis in section 10-4 in a net present value context. Yet on page 10-125 the EA/RIR notes that it has dropped the present value calculations that were presented in the Aug. draft: "*Because of the difficulty of identifying an appropriate time frame for this action, this analysis does not include an estimate of the present value of the revenue at risk from this action."* The WTP presented in section 10-4 claims present value benefits approaching $100 billion over a 60 year time frame. If the time frame of the action is too uncertain to estimate the present value of revenue at risk, it should be equally inappropriate to use it for WTP benefits.

8) While the EA/RIP presents a highly detailed explanation of the Lew, Layton, and Rowe (LLR) model, the results are hard to take seriously. It is plausible that a randomly surveyed family might express a WTP of $100/year to benefit endangered seal lions. However, there are 415 different endangered animals listed under the ESA in the U.S. If

the same individuals were surveyed on their WTP for each of the animals on the list in alphabetical order, it is intuitively obvious "donor fatigue" would set in even for hypothetical donations long before reaching "SSL." In any case, the WTP survey was of randomly surveyed households. But most importantly, it is not they who will pay the cost of these closures, it is the participants in the affected fisheries.

9) Why are the results of this survey in the EA? Comparisons of real annual losses of revenue in the fishery to hypothetical donations which may or may not produce hypothetical benefits to sea lions is an insult to those footing the actual costs. The WTP survey is an excellent example of misdirected SSL research funds. Inclusion of these survey results in the EA is inappropriate and should be an embarrassment to the agency. Research funds should, instead, be applied to research designed to reduce uncertainty regarding the cause of impacts to SSL recovery trends. Lacking this information, Steller sea lions, fisherman and fishing communities pay the price for continued uncertainty. The EA does not do an adequate job of assessing those costs.

<u>Summary:</u> As noted in our earlier comments, the BiOp now treats the SSL Recovery Plan (SSLRP) Recovery Criteria less like guidelines and more like hard and fast requirements. This after-the-fact change in policy, in our mind, undermines the intent and spirit under which the SSLRP was first developed. So instead of celebrating a 12-14% increase in the SSL Western Distinct Population Segment (DPS) of approximately 70,000 animals since the last BiOp, we are engaged in a controversial JAM determination based on declines in the small, regional SSL sub-populations in the Western and Central Aleutian Islands. We do not believe this is consistent with the way the ESA is applied elsewhere. The result is tragic and, we believe, unnecessary. Further, the BiOp's conclusions on the cause of the localized declines do not seem supported by an objective evidentiary approach in assessing alternative hypothesis, or in crafting actions to mitigate potential impacts.

Finally, we look forward to a transparent and comprehensive peer review of this Biological Opinion that includes a review of the BiOp's conclusions as well as its RPAs implemented as part of the Interim Final Rule. We also look forward to participating with the agency in designing revised RPAs to be used in a Final BiOp based on incorporation of the most recent and best science available including new stock assessment data, SSL survey data, telemetry data, relevant publications and results of the BiOp peer review.

Sincerely,

Frank Kelty
President

Attachments:

(1) Dr. Ian Boyd's Comments on Draft and Interim Final Rule BiOp
(2) *The Decline of the Steller Sea Lion in Alaskan Waters: Untangling Food Webs and Fishing Nets* (2003)
(3) MCA Comments on Draft BiOp and EA
(4) Dr. Ian Boyd's Cover letter of May 6, 2010. *Assessing the effectiveness of conservation measures: resolving the "wicked" problem of the Steller sea lion.*
(5) MCA Letter of July 18, 2007, Draft Revised Steller Sea Lion Recovery Plan
(6) Some Former members of the Recovery Team Letter of August 20, 2007, revised Draft Steller Sea Lion Recovery Plan.

Copy: Governor Sean Parnell, State of Alaska
Governor Christine Gregoire, State of Washington
Senator Lisa Murkowski
Senator Mark Begich
Senator Patty Murray
Senator Maria Cantwell
Congressman Don Young
Honorable Gary Locke, Secretary of Commerce
Ms Jane Lubchenco, Undersecretary of Commerce for NOAA
Mr. Eric Schwaab, NOAA Assistant Administrator for Fisheries
Chairman Eric Olson, NPFMC

Final Biological Opinion
Groundfish Fisheries, Bering Sea and Aleutian Islands Management Area
US National Marine Fisheries Service
Review of response to views expressed by Professor I.L. Boyd

This task was undertaken to assess the extent to which comments made by me have been taken into consideration during the revision of the Biological Opinion Groundfish Fisheries, Bering Sea and Aleutian Islands Management Area by the US National Marine Fisheries Service.

The task was carried out by (1) assessing what changes had been made in those sections included in the original review (Section 3.1, 3.2, 4.3, 4.4, 4.5, 7 and 8.3.4; See Appendix I) and (2) examining the extent to which these changes had responded to those provided in my review (See Appendix I).

1. General response to comments

Overall, NMFS has made very few substantive changes to the parts of the document reviewed in detail by me. Most changes were of a minor editorial nature (use of capital letters for proper names etc). I do not intend to document all of these changes because most are cosmetic but have them recorded on a version of the Revised Biological Opinion if there is a wish to consult them.

2. Specific responses to comments

Comment #2 (Appendix I). This addressed the *ad hoc* concept of "recovery" and the confusion that exists in the Biological Opinion between biological evidence within the societal or individual values expressed in terms of the policy established by NMFS as a result of the deliberations of the SLL Recovery Team and the establishment of the Revised SSL Recovery Plan.

Response: NMFS has made no attempt to respond. The original text contained text in Section 7 relating to the Recovery Plan and its objectives. This has received slight modification but nothing that addresses the fundamental issue being raised. NMFS appears to be continuing to attempt to proceed towards a policy objective that, seen from a biological perspective, is likely to be unrealistic and unachievable.

Comment #3 (Appendix I). This asked NMFS to address the problem of the many assumptions there are in the underlying data.

Response: There is no evidence that NMFS has responded to this. In fact, they have removed a section from pp 196/197 of the original Biological Opinion that was a reasonably balanced exposition of the relative uncertainty in some of the factors causing the decline and the relative importance of nutrition in this (see Appendix II for the text of this section that has been removed).

Comment #4 (Appendix I). This concerned the justification for the population trends in Section 3.1.3.2 and Figure 3.7 of the original Biological Opinion.

Response: Although the comment was meant to be of a general nature using the section on trends in p81 of the original Biological Opinion as an illustration of NMFS tendency to question their own work less than they would question that of others, especially if it does not agree with their views, NMFS in this case appears to have responded by adding a new section titled "Regional Trends in Adult and Juvenile western Steller sea lion Counts" on p83 of the Revised Biological Opinion.

Comments #5, 6, 7 & 8 (Appendix I). The comments concerned the over-simplification by NMFS of the pattern of uncertainty in its knowledge of vital rates. Extensive illustrations were given of how NMFS had apparently used evidence in an unbalanced manner.

Response: As far as I can see, NMFS has made no changes relating to this comment. They did add a further comment to the end of the paragraph on P93 comparing between the Holmes and Maniscalco studies but this made no material difference to the way the arguments had been framed.

Comment #9 (Appendix I). This comment deals with the definition of the DPSs, which I have long suggested is misguided and only weakly justified.

Response: In its response to the MCA, NMFS addressed this point by suggesting that it was not relevant. NMFS has added a short paragraph on p77 that diverts attention away from this issue by suggesting that consideration of the subject should be addressed during periodic status reviews.

Comment #10 (Appendix 1). This comment tries to correct errors in the interpretation and representation of my paper published in Biological Conservation in 2010.

Response: As far as I can see, NMFS has made no changes as a result of my attempt to point out material inaccuracies. In addition, on p334, Section 7.3, 5th paragraph, there is a new response here to my 2010 paper which repeats and reinforces the inaccuracies stated in the original draft of the Biological. No change has been made to the text on pp 95/96 of the new draft and the change added on p334 simply reinforces the clear misunderstanding of the paper. It is as if NMFS has either not read the paper or they have intentionally misinterpreted its content. Moreover, the revised Biological Opinion continues to refer to the paper as "in press" when in fact it has been published for at least 6 months. Overall, there is evidence here that NMFS has not given proper consideration to the implications of this study. The additional text on p334 is "Others have followed this meta-population approach, and have conducted PVAs that combined the dynamics of the western DPS and the eastern DPS and demonstrated at that level of aggregation that Steller sea lions are likely not at risk of extinction (e.g., Boyd 2010). However, concerning the results of Boyd (2010), NMFS cannot rely on this finding because Steller sea lions are recognized as two distinct populations under the ESA."

Comment #11 (Appendix I). The comment relates to the *ad hoc* way in which rookeries are assigned to clusters.

Response: On p82, NMFS has provided an additional sentence to justify its definition of RCAs: "The RCA boundaries were determined based on demographic similarities of animals in groups of Steller sea lion sites, similarities in abundance trends among groups of sites, locations of

Steller sea lion survey region boundaries, and the current locations of fishery management subarea boundaries." The problem here is that there is a mixture of two types of information; one involves biological information about the populations and the other involves boundaries established for expediency in carrying out surveys and in management. The criteria defined by convenience probably have no instrinsic biological merit but are possibly justifiable if one knows with reasonable accuracy the extent to which emigration occurs between these regions. The biological indicators are not, however, justified. In fact these repeat a mistake made by York (1993) when she used population demographics to define the regions which were then analysed for their demographic properties. There is clearly a problem in doing this because one is then correlating a variable with itself, i.e. one is confusing independent and dependent variables in an analysis. This has particular implications for the definitions of "recovery" because, if regions are defined by their demographics, this will probably affect the probability of recovery (defined by demographic criteria) in the second standard used to judge recovery, which is based on patterns of recovery in different regions.

Comment #12 (Appendix I). This comment deals with the problems there are with the NMFS view that nutritional stress is present within the WDPS of the SSL.

Response: There is no evidence that NMFS has responded to this. In fact, as also stated above they have removed a section from pp 196/197 of the original Biological Opinion that was a reasonably balanced exposition of the relative uncertainty in some of the factors causing the decline and the relative importance of nutrition in this (see Appendix II for the text of this section that has been removed). NMFS has added a sentence to the 1st paragraph on p119 "Further, it is possible that chronic nutritional stress also may have delayed the age of sexual maturity in adult females, as well as increasing the average period of dependency of pups. These mechanisms would also be expected to reduce pup production in the population." It is unclear why this has been added because it is simply speculation and adds no evidence.

Comment #13 (Appendix I). Relating to the comparative evidence from other species for changes in carrying capacity and that it says nothing informative.

Response: NMFS does not appear to have responded to this comment.

Comment #14, 15 & 16 (Appendix I). These comments relate to the need for NMFS to ensure that its arguments are based upon evidence or on precautionary assumptions that are clearly stated, and not upon speculation (in Table 4.8) and to ensure that the balance of evidence supports their conclusions, which it does not in this case.

Response: NMFS does not appear to have responded to these comments. They have deleted a sentence from Section 4.7.1.2, but it is not clear if this is a response to my comment.

Comments #17, 18 & 19 (Appendix I). This comment relates to the standard with which the evidence within the document is drawn together in order for the Opinion to draw a logical conclusion. I comment that this has not been done in a manner that provides confidence that the Opinion is not an *ad hoc* approach by a subset of Federal employees with uncertain interests. I make the point that the assessment of the evidence is not formal, transparent or repeatable. I have also provided a detailed table analysing the evidence based upon the conclusions on p359 (Section 8.3.1).

Response: Very few material changes have been made. On p327, Section 7.1.1, 1st paragraph, the words "is likely" have been replaced by "is expected or not" with reference to the probability of jeopardy. This change is probably important because it moves the onus of a judgement away for what could be interpreted as a statistical likelihood to one based much more on judgement.

APPENDIX I

Biological Opinion
Groundfish Fisheries, Bering Sea and Aleutian Islands Management Area
US National Marine Fisheries Service
Views expressed by Professor I.L. Boyd

SUMMARY:

The main points being raised by these comments are:

- **The only evidence supporting effects of fishing on Steller sea lions is a suggestion of lower fecundity operating through nutritional stress. In some critical sections of the Opinion declining productivity is suggested to be caused by chronic nutritional stress whereas in other sections nearby declining productivity is used as evidence of nutritional stress. This is a logical inconsistency and it is impossible for both to be true.**
- **Almost all of the evidence referred in Section 8.3.1 (p357) which is used to support the management actions has little basis in fact.**
- **The document lacks a rigorous approach to the assessment of "evidence" and fails to use evidence consistently; information that has much associated uncertainty when first introduced in the analysis gradually drifts to information of high certainty as the document develops.**
- **Terminology is ill-defined and used inconsistently: for example, there is confusion between "prey biomass" and food availability for SSL. The two are not the same by any means and this reflects the many unstated assumptions made within this document.**
- **Whatever way one looks at the current historical data for SSL populations using the criteria defined by NMFS for classification as endangered, there is no support for the conclusion that the WDPS of the SSL is endangered. Continuing with such a classification simply brings the concept of nature conservation into disrepute and eventually endangers species that really do need protection.**
- **The most parsimonious conclusion from reading this Biological Opinion is that NMFS wishes, in principle, to constrain fishing in the Aleutian Islands but it has few levers to pull in order to achieve this other that the Endangered Species Act. Nevertheless, one should not condone the twisting of data to achieve what is, in essence, a political objective.**

DETAILED DISCUSSION POINTS:

1. These views relate mainly to Sections 3.1 and 3.2 relating to the status of Steller sea lions and their habitat, and to Sections 4.3, 4.4 and 4.5 relating to the human impacts on Steller sea lions. Some additional comments are provided on Sections 7 and 8.3.4. These views are also provided on the basis that they are NOT given to support the case for or against the management measures in the Opinion, but because there is a need to use evidence appropriately when providing scientific advice.

2. My comments are provided with an underlying view that, overall, the evidence to support the concept of "recovery" of the Steller sea lion to some *ad hoc* historical base line population size is poorly justified both within this document and many others produced by NMFS, including the Steller Sea Lion Recovery Plan. I conclude that the line of thinking from NMFS about the meaning of "recovery", and the implications this has had for their decisions, is a statement of policy from NMFS rather than one that has any biologically factual basis. There is much in this document that reflects a set of institutional values that tends to try to fit evidence to policies rather than tries to establish the evidence and allow the policies to be built around that evidence. This confusion between "values" and scientific evidence is endemic within this Biological Opinion and, as a result, my view is that the document needs to be completely re-written using a pre-determined framework for the presentation and assessment of evidence so that independent assessors who have no history within this subject area can draw a consistent conclusion from the evidence base. At present, the conclusions appear to be *ad hoc* and have the appearance of a set of Federal employees with specific sets of values driving their own personal agendas.

3. The analysis presented by NMFS contains many untested assumptions which are not mentioned or analysed in terms of their impact upon the conclusions drawn and the actions proposed. For example:

    a. What is the implication of the assumption of using "trend sites" as opposed to building an approach that uses all count data?
    b. What are the implications of assuming constant bias within survey data?
    c. What are the implications of assuming that the SSL population in certain regions is depleted by some unknown amount as opposed to being at or near carrying capacity?
    d. What are the implications of largely ignoring uncertainty and bias in the broad range of data being used, especially in those used to justify the view that the population has a low reproductive rate?
    e. What are the implications of assuming that models are more useful than real data (see p89, 4 lines from the bottom)?
    f. What are the implications of the assumptions that well-managed fisheries at close to MSY cannot enhance (or will normally reduce) the food availability for a predator if that predator takes only a small proportion of the overall prey biomass?
    g. What are the implications of the assumption that the pup ratio on rookeries means the same in different parts of the range?
    h. What are the implications of just using pup ratio on rookeries as opposed to the overall pup ratio?

None of these assumptions are considered in any detail or analysed to understand wherethe uncertainties lie.

4. NMFS is too quick to believe in the veracity of their own data. The analyses of trends are a particular case in point where, for example, there is little justification for the statements in Section 3.1.3.2 based upon the data presented in Figure 3.7. NMFS should be explicit in showing the uncertainty in their data, and incorporate this uncertainty in their trend analysis.

5. The analysis of vital rates data (Section 3.1.4) fails to express the considerable uncertainty there is around these estimates. These data can only be used, at best, as a general guide to processes and trends. In the first sentence of Section 3.1.4 the statement "Changes in the size of a population are ultimately due to changes in one or more of its vital demographic rates" illustrates the tendency to simplify the problem of uncertainty because this statement is only true if one assumes that the actual estimates of population are unbiased and that the bias is stationary through time. These are almost certainly an incorrect assumption for the Steller sea lion.

6. A further illustration of the kind of biased "opinion" expressed here appears in the concluding paragraph of p87 where differences in conclusions between a recent study by Maniscalco and conclusions of Holmes et al. are discussed. There is nothing wrong with the comments about the potential biases within the Maniscalco paper (although the critiques cited appear to be of the draft paper, not the final published version) but no equivalent analysis of the weaknesses of the Holmes paper (which comes from NMFS itself) are carried out. In fact the Holmes paper contains many weaknesses, not least of which is the "black box" nature of the model used to fit to data, the underlying effects of assumptions about how particular variables are distributed and the apparent use of data without assimilation of the true uncertainties. I do not say this to "rubbish" the Holmes studies but simply to make sure that there is an appropriate balance in this type of assessment and the use of evidence. In reality, we know very little about natality in the SSL.

7. This type of (presumably inadvertent) reflection of bias within the analysis as it is presented continues through p88. For illustration (but there are many other examples), at the top of paragraph 2 there is a statement "Declines in female reproductive performance may have been, and may still be, linked to body condition or growth". Although couched in terms of "may" and "linked", the data do not show, or even imply, cause and effect and the results of studies referred to subsequently in the same paragraph are based upon very small and most likely highly biased samples. Moreover, much of the evidence subsequently referred to further down the page contradicts the introductory sentence but there is an apparent reluctance to say "we don't know". Instead, the text is hedged around by statements like "The observed differences above indicate that at least this phase of reproduction may not be affected by whatever factors are limiting natality...". Of course, this very statement is linking one highly uncertain situation (that surrounding the interaction between reproduction and growth/condition) with another (the natality rate) and, by this stage in the document, it has become explicit that there is low natality when, in fact, this is far from certain and is probably dependent upon how one presents the data.

8. The foregoing point is just one case of how logical inconsistencies run through the document. In other words, within sections dealing with defined subjects there is a level of discussion of alternatives, even if that discussion is often biased and does not deal fully with uncertainty, but between sections there is a drift in the logic back to the central policy-driven issue associated with choosing the evidence that best-fits the policy rather than choosing the policy that best fits the evidence. This type of approach is most evident in the bullets on p90.

9. The second paragraph in the section 3.1.4.3 deals with possible emigration - a question I have raised in the past largely to question some assumptions -. My point is

that the data about the definition of a DPS have never really been collected with this hypothesis in mind and have largely been collected with the objective of verifying an extant classification which was convenient largely for geopolitical reasons. For example, I doubt very much that genetic sampling was stratified in a manner that could have tested the emigration hypothesis. NMFS is simply assuming the putative emigrants would have mixed freely and randomly with the resident population.

10. The final paragraph on p92 is particularly welcome but it is a pity that this dose of realism with respect to historical PVAs is not further reflected in Section 3.1.4.4. It is worth noting that the discussion in the final paragraph of p93 discussing the PVA that I undertook is materially inaccurate. It is unfortunate that NMFS chose to ignore this new information and analysis instead of incorporating it into the Opinion. This PVA took account of the fact that the circumstances of the 1980s are unlikely to be repeated because of the legislation and management that is now in place but it also did consider the cases of the DPSs, being completely distinct (i.e. in terms of the ESA listings) so the statements (at the end of the paragraph) that it cannot be used because of NMFS' legal commitments is incorrect. The analysis I undertook used the data collected about SSL population in a very different way from the traditional approach employed by NMFS (e.g. it used all data, it made no distinction between "rookeries" and "haulouts" because this distinction has never been justified objectively and it attempted to account for bias in the data). In other words, the data drove the analysis, not preconceived assumptions. The analysis resulted in a PVA that showed, whatever information based upon the history of the SSL population one used, the SSL population was not "endangered" based upon the criteria used by NMFS. This was the result for the population as a whole, and for each of the DPSs when evaluated individually. Specifically, the analysis:
    - Was based on all of the available historical data for all SSL sites, not selective "trend" sites.
    - Was performed for the population as a whole, as well as for each of the Distinct Population Segments.
    - Showed that for the WDPS, the overall population trend was generally positive. Under all scenerios, the WDPS met NMFS conservation objective of less than a 1% chance of extinction in 100 years.
    - That pup/non-pup ratios for the WDPS were similar to those for the EDPS when all sites were used to measure the EDPS, not just Southeast Alaska as is shown in the Opinion. The WDPS pup/non-pup ratios are close to long-term mean, and those for the EDPS are slightly higher.
    - Current population levels may be close to long-term mean.

11. NMFS has chosen to reclassify the population into rookery cluster regions. What are these, how have they been derived, on what basis are they being proposed and what value do they add? They appear to be yet another *ad hoc* complicating factor introduced with little biological justification. The whole issue about how NMFS defines sub-regions and then turns them in to apparently logical units of management seems again to reflect a drifting baseline through this document (as it did through the Recovery Plan), from a position in which information with high associated uncertainty achieves unjustified levels of certainty when introduced used to justify management actions.

12. Section 3.1.14. The nutritional stress hypothesis has been done to death with little or no conclusive supporting evidence. First, NMFS acknowledges (p.109/110) the probability of connecting historical trends to nutritional stress are as close to zero as makes no difference. Second, even if chronic nutritional stress is currently active in the population it will be extraordinarily difficult to observe. Individuals in any population that is experiencing chronic nutritional stress will be a very small proportion of the total population at any time and will likely be weeded out very quickly by predation. One cannot approach this by measuring the population mean or median for various parameters because nutritional stress will only be apparent at the extremes of the size distributions. Third, NMFS acknowledges that of the indicators for measuring nutritional stress, 13 were negative (no effect indicated) and 1 was positive (an effect indicated) and this was reduced birth rate (table 3.17). As noted above, there is considerable uncertainty surrounding the data used to determine trends in birth rate and there are conflicting results from modelling studies versus field studies. Here again is an instance where the analysis fails to appropriately address the uncertainties in the data and incorrectly moves from "uncertain information" to "certain causal factor" without justification.

13. Section 4.1.5 (last paragraph). The text here is more an expression of the history of the debate and does not say anything useful about changing carrying capacity for SSL. Just because it was being debated by scientists or because seabirds responded to something (although not necessarily anything to do with the fishery), does not mean that the increase in fisheries affected the carrying capacity for these predators. Overall, there is very little evidence from anywhere to support the view that generalist predators like the SSL are negatively affected by fisheries. In fact, some examples, especially from the North Atlantic suggest quite the opposite, i.e. that these predators actually flourish alongside groundfish fisheries.

14. Section 4.7.1.2, P254. The justification for the position taken on indirect effects of fisheries is provided, fundamentally, by the nutritional stress hypothesis. We have seen how weak that hypothesis is, with little objective data to support it. If NMFS wishes to exclude fisheries from critical habitat they should do this based upon their precautionary assumption, that there is an indirect effect of fisheries and not on poorly supported supposition or hypotheses. Consequently, I would suggest that a different classification is needed in Table 4.8 that reflects a precautionary assumption in the face of the lack of objective data, because there is little to support a "likely" classification for an indirect fisheries effect. (As an aside, in Table 4.8, there is a need to provide clear descriptors for the classes given so that this classification can at least be carried out consistently irrespective of the operator undertaking the classification.)

15. In the same section there is a statement: "The primary issue of contention is whether fisheries reduce Steller sea lion prey biomass...". I don't think this is the case. The primary contention is whether they reduce the potential intake rate of Steller sea lions (prey availability). Prey availability (fishery effects on the prey field) was the issue in the previous Opinion and this document does not explain why the emphasis has now changed. It is relatively easy to demonstrate reduced biomass of prey that might be exploited by SSL but this could be a very poor indicator of the food intake rate and, in some circumstances, it could be inversely related to food intake rate. I think few would contend that fisheries modify habitat but with respect to the SSL we do not know if this is adverse.

16. In the second paragraph of the same section there is the statement “Fisheries are likely to lower Steller sea lion carrying capacity”. Used here “likely” is a strong word. On the balance of probabilities they may reduce rather than increase carrying capacity but the difference might be quite small. Certainly, in the North Atlantic in those regions most affected by groundfish fisheries the balance of probabilities would be that fisheries have increased the carrying capacity of seals. In a well-managed fishery which is approaching MSY and where the yield taken by a predator population is a relatively small part of the total yield one can easily foresee a situation where fishing is either neutral or could even increase carrying capacity in a predator such as a SSL. In fact, in theory, it probably should. If NMFS is using an MSY approach to managing the fishery then it is logically inconsistent to then conclude that the fishery is detrimental to the SSL if they are also saying that there is a strong overlap between diet and the species captured by the fishery.

17. Section 7. While understanding the duty placed upon NMFS by legislation and that this section attempts to draw together all of the lines of evidence to derive an appropriate conclusion, I do not feel the narrative here gives confidence that this synthesis is being conducted in an objective manner. This section is not written to the standard expected of a document of this type in the present day and it follows the kind of approaches adopted more than a decade ago. A test of whether this is objective would be to give the information base to an independent set of consultants and ask them to follow the methodology used to derive the conclusions. In spite of its intentions – stated at the beginning of this section – this does not amount to a formal, transparent and repeatable assessment.

18. To illustrate this point, the bullet points on pp342-343 used in the “weight of evidence” are an *ad hoc* grouping that do not work through the issues systematically providing appropriate scores for each of the major “knowns” and “unknowns”.

19. P357, bullet “No other stressor...”. Nelson at the Battle of the Nile used his blind eye to look for signals he did not want to see. It is terribly easy to not find other stressors because one either does not wish to look or one does not have the capacity to look. Given the precarious nature of the nutritional stress hypothesis I suggest this is not a tenable piece of evidence. None of the “dozens” of experiments in the field or captivity have demonstrated that “prey removals will result in chronic nutritional stress”. What the captive studies have demonstrated is that if you reduce the prey intake rate you can induce a form of nutritional stress and this can be reflected in various response variables in the animals. The wild studies have not demonstrated any such connection, even in terms of a “smoking gun”.

ANALYSIS OF CONCLUSIONS:

On Page 357, a summary is provided of the evidence supporting the indicators of concern that form the basis for the conclusions of this Opinion. While these are only summaries, there are inconsistencies between the evidence as it has been used here and the evidence in earlier parts of this report. There are also material errors reflected in the statements of supporting evidence on page 357. Briefly, some of these inconsistencies and errors are as follows. Items labelled "opinion" are verbatim quotations from the report; items labelled "evidence" are analyses of that actual evidence provided within the report.

Opinion	a	Severe declines in counts of non-pups and pups in the western Aleutian Islands. Recent pup/adult female ratio lowest of all sub-regions
	b	Continued declines in abundance of non-pups in the central Aleutian Islands (specifically RCAs 2- 4).
	c	Continued declines in numbers of pups in parts of the central Aleutian Islands (RCAs 2 and 3).
Evidence	a	Figure 3.7 shows that declines occurred in this region throughout the 1990s but, given uncertainties in counts it is unlikely there has been any statistically significant decline since 2000.
	b	A similar conclusion can be derived from Figure 3.9.
	c	In Figure 3.10 (which has no units on the y-axis but is assumed to be total numbers), pup counts in the regions of concern are down but, when seen in relation to the overall pup counts for the population these are easily counteracted by increases elsewhere. Overall this is the kind of pattern one could expect for any population this is fluctuating around carrying capacity.
Conclusion		It is difficult to see how this opinion has been derived from the apparent evidence available in Section 3 of the Report

Opinion	a	No other stressor identified as leading mechanism for decline of Steller sea lions in the western and central Aleutian Islands; based on dozens of field and captive Steller sea lion studies, prey removals will result in chronic nutritional stress which in turn is the likely cause for a lack of a robust recovery in the western DPS.
Evidence	a	The absence of evidence is not evidence. Indeed the evidence of nutritional stress is as absent as any other form of evidence. NMFS's own analysis finds that 13 out of 14 indicators show no correlation. The absence of alternatives could just as easily be because we do not have the capacity to observe the causes, whatever they may be.
	b	None of the experiments or studies referred to in Section 3.1.14, or others that have been done on Steller sea lions, have demonstrated that "prey removals will result in chronic nutritional stress". A few experiments on captive animals suggest that reduced food intake can result in certain physiological and morphological responses but "prey removals" do not equate with reduced *per capita* food intake and may have no relationship at all with *per capita* food intake (see later comment)
	c	No evidence is presented in Section 3.1.4 (summarised in Section 3.1.14.5, p115) to link nutritional stress to population trajectory. In fact the connection in this section is only assumed. The argument being made in this section (3.1.14.5) employs circular logic. In the 1st paragraph of this section it is suggested that declining productivity is caused by chronic nutritional stress; in the 2nd paragraph declining productivity is used as evidence of nutritional stress. It is impossible for both to be true.
Conclusion		The opinion is clearly a very loose and inaccurate interpretation of the actual evidence present, but even the evidence presented contains logical inconsistencies.

Opinion	a	High fractions of available forage biomass have been harvested from the Aleutian Islands historically.
	b	High fractions of key prey species have been harvested inside Steller sea

		lion critical habitat in the Aleutian Islands (especially Pacific cod in RCAs 1 and 4).
	c	Diet information from Steller sea lion scats confirms the importance of Pacific cod, Atka mackerel, and pollock.
Evidence	a	Most of the evidence (Section 4.5.3) used to support these statements involves various types of overlap (spatial, species etc) between fisheries and Steller sea lions. However, high levels of overlap are not necessarily evidence of food deprivation. The narrative fails to examine other cases in which there is high overlap between seals and groundfish fisheries (e.g. grey seals in the North Atlantic) where those species have been easily able to sustain high levels of population increase.
	b	The evidence provided does not consider the possibility that a well-managed fishery should either be neutral or even tend to enhance energy flow through the exploited fish populations. As a result, it may not be surprising that predators foraging in the region of spatial overlap will also tend to feed on the same species as the fishery exploits.
	c	The evidence as presented does not consider the non-linearities of the functional response of the Steller sea lion. Evidence from other species shows the functional response to be highly non-linear (i.e. it takes very large reductions in prey biomass before any response is observed in the predator). The information about depletion due to fisheries is not sufficiently accurate to show whether it is sufficient to lead to any effect upon the Steller sea lion.
Conclusion		The opinion is not strongly supported by the evidence, and the evidence itself ignores certain basic biological and ecological principles.

Opinion	a	Foraging distribution as indicated by filtered telemetry data confirm disproportionately high use of 0-10nm zone of important terrestrial sites (rookeries and haulouts). However, Steller sea lion foraging distribution based on updated telemetry information shows movement patterns of tagged sea lions well outside of 20 nm. RCAs 1-3 have a large proportion of diving locations >4 m depth outside of the extent critical habitat (AFSC 2010b).
	b	Boor (2010) analysis of POP dataset shows substantial Steller sea lion foraging offshore in summer, especially south of Attu and Agattu Islands, and an even larger number of encounters offshore in winter throughout the Aleutian Basin. We recognize that this analysis includes sightings data from over the last 40 years. Nonetheless, this analysis suggests the potential importance of habitat outside critical habitat for Steller sea lion foraging.
Evidence	a	In Section 3.1.6. there is a biased analysis of the Boor study. The Boor study represents a difficult, as yet unpublished, analysis of data that was not collected for the purpose used by Boor. Distribution and density estimate information requires the collection of observer effort data which is not available for these historical data. The Boor analysis tries to correct for this but generally fails to do so because there really are almost no ways of recovering observation effort from data post *hoc*. Consequently, it remains possible, perhaps likely, that these data reflect the distribution of observers rather than Steller sea lions.
	b	The foraging distance from rookeries will follow a statistical distribution. The more data one adds the higher will be the probability of animals being observed outside the 20 nm definition of critical habitat. In addition, I understand that the additional data for area 543 (western AI) showing foraging outside CH was three young males from area 541, feeding off the shelf at water depths of 1000m or so. How relevant are these data?

Conclusion		The opinion uses dubious evidence.

Appendix II

Section excluded (italics) from Section "4.5.1 Important Steller Sea Lion Prey Species and Fisheries Which Potentially Affect Prey"

In summary, based on best available scientific and commercial data, the fisheries as authorized under the FMPs potentially compete with Steller sea lions for common resources. Fisheries and Steller sea lions both consume pollock, Atka mackerel, Pacific cod, salmon, arrowtooth flounder, and rock sole. The high degree of overlap between pollock, Atka mackerel, and Pacific cod fisheries, and to perhaps a lesser degree with the other fisheries listed above, and the foraging needs of Steller sea lions supports the hypothesis that competitive interactions may be occurring in the range of the western SSL DPS on a number of spatial and temporal scales.

Therefore, a working model of factors that may be contributing to the lack of a robust recovery of the western DPS of Steller sea lions, as well as the significant declines in abundance in the western, and in the western portion of the central, Aleutian Island sub-areas, is as follows:

1) *no one factor can explain the overall and local patterns in trends in abundance (Loughlin and York 2000, NRC 2003, NMFS 2008, Atkinson et al. 2008).*
2) *while a preponderance of scientific evidence does not support nutritional stress as one of the primary factors adversely impacting the recovery of this DPS, information on the pattern of decline in the reproductive rate and size at age of this population relative to the eastern DPS since the mid-1970s is consistent with the nutritional stress hypothesis. Therefore, nutritional stress cannot be dismissed as an important factor in understanding the dynamics of this population. Further, the estimated decline in the reproductive rate is not consistent across the range of the DPS.*
   - i) *acute nutritional stress does not appear to be an important mechanism in understanding the dynamics of this population (Trites and Donnelly 2003, Trites et al. 2006a, NMFS 2008, Atkinson et al. 2008).*
   - ii) *chronic nutritional stress, if it is occurring, is the most likely mechanism related to the lack of a robust recovery in this DPS. Chronic nutritional stress could very likely be responsible for the decline in reproduction reported by Holmes et al. (2007)(Rosen 2009); however, see paper by Trites et al. (2008).*
   - iii) *environmental forcing undoubtedly changes the prey field for SSL over time. In some cases, these changes could be beneficial (i.e., increase the carrying capacity) and in other cases, these changes could decrease the carrying capacity. Carrying capacity may differ markedly from one subarea to another; new studies suggest oceanographic conditions in some subareas may differ sufficiently to provide less favorable habitat for producing SSL prey (Lander et al. 2010). The information needed to ascertain which environmental conditions improve the prey field and which environmental conditions degrade the prey field is not available.*
   - iv) *it is possible that commercial fisheries have adversely impacted, and in the future could continue to adversely impact, the prey field of Steller sea lions, which could contribute to the conditions that support a poor prey field (i.e., chronic nutritional stress). Recent studies, however, show very inconclusive relationships between fishery removals of prey and SSL sub-population growth (AFSC 2010). Thus, it is likely that these conditions vary geographically within the range of the western DPS of Steller sea lion (NMFS 2001, NMFS 2003).*

3) *the information necessary to completely dismiss the hypothesis that contamination or disease are important factors in understanding the dynamics of this population is not available (NRC 2003, NMFS 2008, Atkinson et al. 2008). According to NMFS (2008) disease is less likely to be a significant factor than are contaminants.*
4) *while the information necessary to confirm the hypothesis that human caused-mortality was one of the primary drivers of the steep decline in abundance in the 1980s does not exist, there is a general consensus among experts that this is or could be the case (NRC 2003, Hennen 2006, NMFS 2008, Atkinson et al. 2008, Kruse and Huntington 2009).*
5) *predation by killer whales is likely to be an important factor in understanding the dynamics of Steller sea lions in some of the sub-areas (Horning and Mellish 2009, 2010a, 2010b), and in particular those subareas that have relatively small numbers of SSL (NRC 2003, Guinette et al. 2010, Durban et al. 2010). Nonetheless, there is compelling evidence to seriously question the hypothesis that killer whale predation was the primary factor driving the overall decline of this DPS in the 1980s and 1990s (see Springer et al., 2003, Trites et al. 2006, Springer et al. 2008, Wade et al. 2008).*
6) *fishing has occurred in the action area for decades, starting in the 1960s with very large catches in thriving foreign groundfish fisheries throughout the BSAI and GOA (Ketchen, 1968, Buck 1973), on the order of catch levels in recent decades. The action area thus has been a fished ecosystem before, during, and after the SSL decline, and continues in this state today while the overall western DPS begins an apparent rebound. Uncoupling commercial fisheries in the action area from the multi-faceted stressors likely acting on the western DPS is not possible to attain with much clarity. However, it is possible that fishing patterns can be examined in light of SSL subpopulation growth patterns and other vital rates trends as described in the following sections.*

In Summary...

The Decline of the Steller Sea Lion in Alaskan Waters

Untangling Food Webs and Fishing Nets

Photo by Rolf Ream, courtesy NOAA/NMFS/NMML

Steller sea lions live in the North Pacific with about 70% living in Alaskan waters. The number of Steller sea lions in Alaskan waters has dropped by more than 80% in the past three decades. The decline resulted in their protection under the Endangered Species Act (ESA) since 1997 for the population west of Cape Suckling. A precipitous population decline from 1985-1990 – about a 15.6% decrease per year - indicated that Steller sea lions were subject to a threat that spurred the decline but had ended or abated by the 1990s. Since the early 1990s (through 2001), the population has continued to decrease but at a more gradual rate of 5.2% annually and individual rookeries show different population trends.

BACKGROUND

Under the ESA, federal agencies must ensure that actions they authorize are not likely to jeopardize the survival or recovery of protected species or damage the protected species' critical habitat. This requirement has made it imperative to identify human activities that may contribute to the decline of Steller sea lions so that regulatory actions can be adjusted to address threats to the western population's survival. In response to a request from Congress, the North Pacific Fishery Management Council asked the National Academies to examine possible causes of Steller sea lion decline and the potential efficacy of new management measures.

HYPOTHESIZING CAUSES OF DECLINE

The several hypotheses that attempt to explain the decline of the sea lions can be divided into two categories. The "bottom-up" hypotheses include potential causes that would limit the amount or quality of food available to the sea lions such as: 1) Large scale fishery removals reducing the availability or quality of prey species 2) A climate/regime shift changing the abundance or distribution of prey 3) Non-lethal disease and 4) Pollutants contaminating fish eaten by sea lions.

"Top-down" hypotheses encompass factors that kill sea lions independently of the capacity of the environment to support the sea lion population. These include: 1) Predator switching by killer whales (or sharks) to target sea lions, 2) Increasing incidental take (or disturbance) through capture or entanglement in fishing gear, 3) Subsistence harvesting of sea lions taking more than estimated, 4) Underestimation of sea lion shooting, and 5) Increasing mortality from pollution and disease, independent of nutrition.

Existing data on the current phase of decline indicate that bottom-up hypotheses resulting in food limitation are unlikely to represent the primary threat to Steller sea lion recovery. Although no hypotheses can be excluded based on existing data, top-down sources of mortality appear to pose the greatest threat to the current population. It is important to remember that a combination of both types of factors may contribute to the decline. Also, geographic variation may mean that different factors are responsible for mortality in different parts of the sea lions' range.

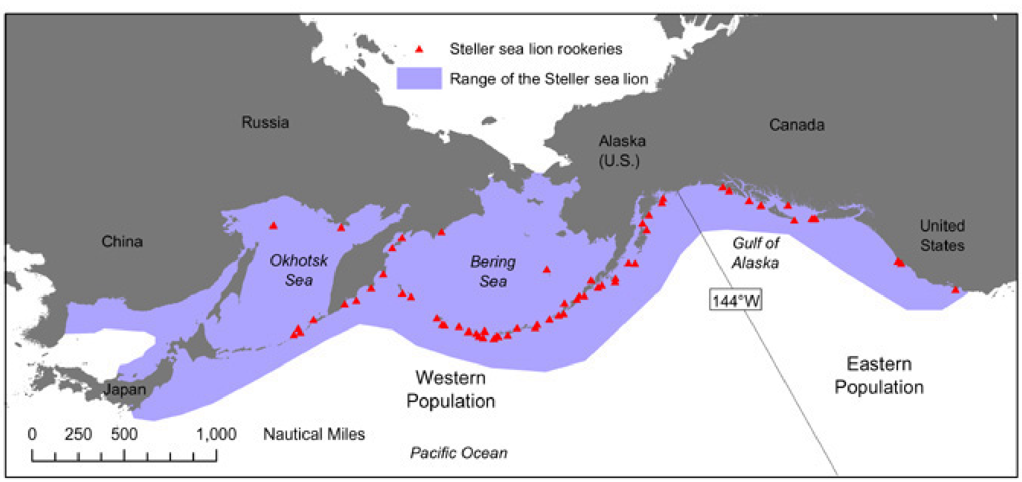

Figure 1. The range (in blue) and rookeries (in red) of Steller sea lions. 144° W defines the boundary between the eastern and western populations. Source: National Marine Fisheries Service, Alaska Fisheries Science Center.

THE NATIONAL ACADEMIES
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THE NEED FOR EVIDENCE

Although most evidence indicates that groundfish fisheries are not depleting the food resources necessary to sustain the current western population of sea lions, there is insufficient evidence to fully exclude fisheries as a contributing factor to the decline. Sea lions may get ensnared in fishing gear because of the ample food available around fishing operations. Attraction of killer whales to these same vessels could increase the vulnerability of sea lions to predation. **Investigations of top-down sources of Steller sea lion mortality should be increased to evaluate the proportionate impact of these factors on population decline.**

MONITORING AND MANAGEMENT

The report recommends using adaptive management to uncover the effect of the fisheries on Steller sea lion survival. Because of potential interference of the Alaska groundfish fisheries with the recovery of endangered Steller sea lions, the fisheries have been increasingly restricted as the sea lion population has continued to decline. In an adaptive management experiment, the western population could be divided into several treatment units, with "closed" and "open" areas centered on rookeries. The "closed" areas would be subject to local closures and the "open" areas would have all fishery restrictions related to Steller sea lions removed.

The approach is germane to the problem because it directly tests the involvement of the fishery in the decline and reduces the possibility that regulation of the fishing industry is perpetuated without demonstrable benefit to the Steller sea lion population. The removal of all sea lion-related fishing restrictions in the open areas creates opportunities for the industry and provides a contrasting management treatment necessary for comparison with closed areas. The approach controls for changes that are unrelated to fishing, such as ecological effects related to climate variability.

Research and monitoring should be directed towards measuring the vital rates and response variables most indicative of the status of the Steller sea lion population, including:

Population trends. The current program for monitoring the juvenile and adult population by aerial survey should be continued along with the direct pup counts at selected rookeries.

Vital rates. Vital rates, last measured in the mid-1980s, urgently require updating. Measurements should include fecundity, age at first reproduction, age distribution, juvenile survival, adult survival, and growth rates.

Critical habitat. The telemetry program on at-sea distribution of sea lions and related foraging activity used to define critical habitat should be expanded to more areas. Stomach telemetry tags that monitor temperature shifts associated with ingestion of prey should improve correlations of at sea distribution with feeding. The activity and impacts of fisheries should be documented, including studies to determine if fisheries cause localized depletion of the various groundfish stocks.

Environmental monitoring. Assessment of various ecological features of the sea lion environment will provide a broader context for evaluating sea lion population trends, including assessments of oceanographic conditions, plankton composition, forage fish abundance and distribution, and monitoring of harmful algal bloom frequency.

Predator feeding habits and population size. Much more information is necessary to evaluate the impact of predation. Better estimates of killer whale diet, population size and distribution throughout Alaska are required to estimate potential predation mortality, and observer programs should be instituted to record killer whale feeding behavior in different regions.

Committee on the Alaska Groundfish Fishery and Steller Sea Lions: Robert T. Paine (Chair), University of Washington; **Daniel W. Bromley**, University of Wisconsin; **Michael A. Castellini**, University of Alaska, Fairbanks; **Larry B. Crowder**, Duke University; **James A. Estes**, U.S. Geological Survey/University of California, Santa Cruz; **Jacqueline M. Grebmeier**, University of Tennessee; **Frances M.D. Gulland**, The Marine Mammal Center; **Gordon H. Kruse**, University of Alaska, Fairbanks; **Nathan J. Mantua**, University of Washington; **James D. Schumacher**, Two Crow Environmental, Inc.; **Donald B. Siniff**, University of Minnesota; **Carl J. Walters**, University of British Columbia, Vancouver; **Susan Roberts**, Study Director and **Nancy A. Caputo**, Senior Project Assistant, Ocean Studies Board.

The Decline of the Steller Sea Lion in Alaskan Waters: Untangling Food Webs and Fishing Nets is available from the National Academy Press, 500 5th Street, Washington, DC, 20001; (800) 624-6242 or http://www.nap.edu.

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September 3, 2010

<u>BY ELECTRONIC MAIL</u>
Dr. James Balsiger
Regional Administrator
National Marine Fisheries Service
Juneau Federal Building
709 West 9th Street, Room 420A
Juneau, AK 99802

Attention: Ellen Sebastian

Draft Steller Sea Lion Biological Opinion

Dear Dr. Balsiger:

On behalf of the Marine Conservation Alliance (MCA) and its members, I am pleased to submit the following comments on the *Draft Biological Opinion (BiOp) for the Bering Sea and Aleutian Islands and Gulf of Alaska Groundfish Fisheries* and Environmental Assessment/Regulatory Impact Review (EA), as released on August 2, 2010. MCA is a broad based coalition of harvesters, processors, coastal communities, Community Development Quota organizations, and support service businesses involved in the groundfish and shellfish fisheries off Alaska. MCA was formed to promote the sustainable use of North Pacific marine resources by present and future generations. MCA supports research and public education regarding the fishery resources of the North Pacific and seeks practical solutions to resource conservation issues. Our members collectively represent approximately 70% of the production of North Pacific fisheries which in turn accounts for over half the nation's fishery production.

Preparation of this Biological Opinion and adoption of a reasonable and prudent alternative (RPA) constitute a significant action. This action will establish important conservation measures designed to address the continuing recovery of the western stock of Steller sea lions (SSLs), but with substantial economic consequences for federal and state fisheries and the coastal communities that depend on them. MCA recognizes the goal of the National Marine Fisheries Service (NMFS) to complete this task in time for new management measures to be in place prior to the 2011 fishery. Therefore, MCA's comments are offered in the spirit of improving the draft BiOp before it is finalized, and ensuring that important procedural issues are considered.

MCA would also like to thank you for honoring our request for more time to prepare these comments. The opportunity for review and comment on the draft BiOp and EA is severely limited, but the extra week was very much appreciated.

<u>Summary:</u>

The draft BiOp is uneven in its treatment of data, findings, and conclusions regarding the status of SSLs and their recovery. The draft BiOp appears to be based on a preconceived conclusion with an analysis shaped to support that conclusion. It does not present the several hypotheses regarding factors affecting recovery, and then test each of these hypotheses evenly and let the data determine the conclusion. MCA believes that the draft is flawed and therefore needs to be revised, and that such revisions to the document will result in a more scientifically and legally defensible final BiOp. Specifically, revisions to the draft BiOp should:

- Include important new scientific research and information that is currently overlooked or discounted in the draft, in order to be based on the best scientific information available.

- Present an even treatment of research used in the analysis, including data and findings of important research that may be contrary to NMFS' position.

- Be more explicit about the data and assumptions used in the analysis. The draft BiOp relies on highly uncertain data and untested assumptions, many of which are not fully described in the analysis but which heavily influence the conclusions.

- Re-evaluate data regarding SSL use of Critical Habitat (CH) and prey availability, be explicit about the limitations of the data sources used, and correct inaccuracies or misinterpretations.

- Be based on methodologies that have been thoroughly vetted or peer reviewed for assessing fishery impacts to SSL prey availability. The draft BiOp methodologies deviate significantly from accepted approaches used to assess and manage North Pacific fisheries.

- Be explicit that the RCAs are for analytical purposes only and that the BiOp is not attempting to establish new management subdivisions. The analysis should include the scientific basis for the specific configuration of the RCAs.

- Be revised to address concerns and comments raised by the Scientific and Statistical Committee (SSC) of the North Pacific Fishery Management Council (Council).

- Thoroughly analyze and adopt the RPA proposed by the Council. MCA believes this is a viable and reasonable alternative approach that meets the intent of the ESA.

In addition, there remain serious concerns regarding the ability of the public, the scientific community, the Council and the SSC, and other parties to review and provide meaningful public comment on the draft BiOp, the EA and related documents. This may have implications for implementation of the RPA under the Magnuson-Stevens Fishery Conservation and Management

Act (MSA), as well as the National Environmental Policy Act (NEPA) and the Administrative Procedure Act (APA). MCA believes that it is incumbent upon the agency to explain with greater precision and clarity the basis for its conclusion that proceeding by way of direct final rule and EA is fully consistent with the requirements of the MSA, NEPA and the APA.

Status of the Western Distinct Population Segment of Steller Sea Lions

To put this action into context, it is important to keep in mind the status of SSLs overall, as well as the status of the Western Distinct Population Segment (WDPS).

The population of SSLs worldwide is estimated to be around 133,000 animals. For the purposes of the Endangered Species Act (ESA), SSLs are divided into two distinct population segments: an eastern segment (EDPS) that stretches from southeast Alaska to California; and a western segment (WDPS) that goes from Prince William Sound in Alaska all the way to Russia and Japan. The WDPS is listed as “endangered” under the ESA.

Current NMFS survey data put the U.S. population of the WDPS at around 50,000 animals in Alaska, and 20-25,000 in Russia, for a total of roughly 70-75,000 animals for the WDPS as a whole.

NMFS trend analyses indicate that the WDPS population of adults and juveniles (non-pups) has grown from between 12% to 14% since 2000 when comprehensive fishery restrictions first went into place. This translates into an annual growth rate of around 1.4-1.7% per year. Johnson (2010) has the annual growth rate at roughly 1.5%. Surveys of SSL pups show a similar trend, with an overall increase of 14%, or approximately 1.7 % per year, between 2000/2001 and 2009.

The criterion set out in the Steller Sea Lion Recovery Plan (SSLRP) for downlisting is a U.S. population of approximately 53,100 animals based on counts of non-pups, or a population growth rate of 1.5% per year for 15 years beginning in 2000. The trends in non-pups need to be consistent with this annual growth rate, and no two adjacent areas can be declining significantly. The downward trend in the western Aleutian Islands may be considered significant, and NMFS has indicated the trend in the central Aleutian Islands is equivocal. The SSLRP does not define “declining significantly,” so what actually constitutes a “significant” decline is a matter of interpretation. Importantly, the population in the area of decline is a relatively small segment of the endangered stock, representing approximately 3-4% of the WDPS population in the U.S. It is even a lower percentage if the Russian component of the WDPS is included.

What this means is that the WDPS is not going extinct, and may in fact be on a trajectory for downlisting in 5 years or so. The question for this action is the rate of recovery of the WDPS overall and sub-regional differences for the population. As such, the draft BiOp focuses on the western regions of the Aleutian Islands where SSL populations continue to decline.

Comments on the draft Biological Opinion

MCA believes that the draft BiOp, while presenting important information, is not up to the usual rigorous scientific standards found in most analyses used to make management decisions in the North Pacific. The draft BiOp does not include important new information, and in some instances misrepresents important new analyses. As such, it is likely the analysis is not based on the best scientific information available. The draft BiOp could also benefit from a more explicit evaluation of the assumptions used in the analysis, as well as a more balanced assessment of the available data, including an explicit assessment of the surrounding uncertainties with that data.

<u>1. The draft Biological Opinion does not incorporate important new scientific information.</u>

MCA is concerned that the draft BiOp does not incorporate new scientific information and therefore may not be based on the best scientific information available. We cite several examples.

MCA is puzzled by the treatment of Dr. Ian Boyd's Population Viability Analysis (PVA) in the draft BiOp. The draft BiOp devotes only a single paragraph to his paper at page 93. The draft BiOp does not include the PVA prepared by Dr. Boyd or any of the information he provided to NMFS from that analysis. Dr. Boyd first presented his analysis to NMFS in the fall of 2009, then to the Council's SSL Mitigation Committee and NMFS staff in January of 2010, and finally to the Council in April of 2010 as a publication in the peer reviewed journal *Biological Conservation.*

Dr. Boyd's PVA is a straightforward analysis by a well regarded international marine mammal expert. The draft, in effectively dismissing his work, states that his analysis is based on "simple empirical indicators," implying that this is not a serious scientific inquiry. This is a curious statement, in that the use of empirical data is usually a preferred way to proceed with an analysis. In that regard, Dr. Boyd's analysis is based on all the available current and historical data regarding all SSL sites, not just "trend" sites.

NMFS also dismisses Dr. Boyd's analysis as not relevant because the agency apparently believes that Dr. Boyd only looked at the population as a whole. NMFS contends that his analysis did not address the ESA structure of two distinct population segments. Dr. Boyd's analysis looks at the SSL population as a whole *and* at the EDPS and WDPS individually. His paper and cover letter are very clear on this point, as were the several presentations he made to NMFS. Among his findings are the following:

- Future scenarios based on historical information suggest that the SSL meets conservation objectives as a whole, and for the EDPS and WDPS individually.
- For the WDPS, the population meets the conservation objective (less than 1% chance of extinction in 100 years) under all scenarios.
- The pup/non-pup ratio suggests current population productivity is close to or above the long-term mean.
- Although there are differences, pup production rates based on the pup/non-pup ratio are similar between the EDPS and the WDPS when all sites are considered. The WDPS appears to be at the long-term mean, while the EDPS is somewhat higher.
- Current population levels may be close to the long-term mean, depending on interpretation of pre-1980 counts.
- Long-term stability suggests the SSL population may be close to carrying capacity.

MCA would also like to point out that the draft BiOp misrepresents one of Dr. Boyd's findings where it selectively states that he found that management actions "taken since 1990 have probably been effective." The actual statement in his cover letter to the Council is that "past measures to prevent decline and promote population increase have been either neutral or successful at preventing further decline." The conclusions in his paper are similar, stating "conservation actions undertaken to date have either been successful or neutral in their effect." In other words, conservation measures have done no harm to SSLs, and may have done some good, but the data are not sufficient to determine if they have had a positive effect. See paragraph 7 below for additional comments by the SSC on this topic.

The draft BiOp further leaves out another important statement from his report, that "in the case of the Steller Sea Lion continuation of current management would appear to be justified but the addition of new management bringing additional social, economic, or implementation costs would appear not to be justified."

We have attached a copy of Dr. Boyd's publication and cover letter to the Council to these comments and request that they be treated as an integral part of our submission. MCA believes that the final Biological Opinion needs to include a more thorough discussion of Dr. Boyd's analysis and findings in its evaluation of factors affecting recovery, the likelihood of the WDPS going extinct, and the possibility of the WDPS perhaps re-orienting itself around a new carrying capacity brought about by environmental change.

Another example of the draft BiOp not including important new information is the absence of the fixed-gear/marine mammal interaction study performed for NMFS by Calkins in 2008. This study was submitted to NMFS in April 2008, and is entitled *Fixed-Gear Marine Mammal Interactions Study.* This report was brought to the attention of NMFS and the Council at the August special meeting, and MCA would like to stress the need to include its analysis, findings and conclusions in the final Biological Opinion's evaluation of the effects of longline fisheries on SSLs. This study was funded by NMFS (and apparently accepted by NMFS as a final product) to determine whether or not there is a correlation between longline fishing effort and trends in SSL population growth. The report concludes that the results are consistent with the hypothesis that longline fishing and SSL population trends are largely independent of each other. MCA has a hard time understanding why NMFS has left this important research out of the draft BiOp's assessment of factors affecting SSL recovery.

The draft BiOp further inappropriately cites various authors to discredit the notion that ecosystem changes resulted in changes in SSL prey composition which in turn affected SSL productivity and population status. In this regard, the draft BiOp inaccurately cites results from various authors regarding the so called "junk-food hypothesis," which is another way of looking at the effects of ecosystem change brought about by changing ocean regimes. For example, the draft states, "Feeding experiments at the Alaska SeaLife Center have shown no negative consequences to juvenile Steller sea lions fed only pollock (Calkins et al. 2005)" [Page 156] and "Calkins et al. (2005) conducted feeding experiments with 3 juvenile sea lions and concluded that Steller sea lions were able to compensate for lower quality prey (without reaching satiation as described by Rosen and Trites 2004)" [Page 114]). Calkins *et al.* (2005) fed pollock to three *adult* SSLs and report findings that are completely consistent with the feeding experiments by Rosen and Trites on adult sea lions.

Further, the draft BiOp relies on Fritz and Hinckley (2005), noting that they "found little evidence that Steller sea lion diets shifted to junk species after the regime shift or that diets with a high frequency of gadids were necessarily detrimental to pinniped fitness and survival," to bolster the notion that the regime shift had no effect on SSLs. There is a large body of evidence to the contrary in the scientific literature, but for the most part those findings are not included in the analysis. For example, the BiOp does not include an analysis of the relationship between changes in sea lion numbers and diversity of diet that was first reported by Merrick *et al*. (1997) and tested by Trites *et al*. (2007). The findings of Merrick *et al*. (1997) and Trites *et al*. (2007) are consistent with the hypothesis that a change in carrying capacity (*i.e.*, the "junk food" hypothesis) affected SSL prey composition which in turn had impacts on SSL populations. At best, Fritz and Hinckley (2005) show that this hypothesis is controversial among sea lion researchers, even though similar hypotheses are used by other researchers to explain the dynamics of other species in marine ecosystems (see Wanless *et al.* 2005; Romano *et al.* 2006; Grémillet *et al.* 2008; Osterblom *et al.* 2008; Whitfield 2008; Beaugrand and Kirby 2010; Spitz *et al.* 2010).

<u>2. The draft Biological Opinion relies on untested and unexplained assumptions, and unevenly treats data to support its conclusions.</u>

The draft BiOp relies on numerous underlying, unstated and untested assumptions which are not presented or analyzed in terms of their impact upon the draft BiOp's findings or recommended actions. For example, we have already noted NMFS' dismissal of Dr. Boyd's PVA, which NMFS attempts to justify in part because Dr. Boyd did not base his analysis only on "trend sites." Dr. Boyd was also explicit in describing levels of uncertainty around survey data to present a more comprehensive picture of what is known about SSL population status over time. The final Biological Opinion should describe the weaknesses of the assumption of using "trend sites" compared to the strengths of building an approach that uses all count data, and should be explicit about the implications of assuming constant bias within the survey data.

Similarly, the draft BiOp refers to the various "methods" used to determine actual survey counts, including the 3.64% reduction applied to more recent counts using high resolution vertical photographs. The rationale has been that while this technique yields more accurate counts, it is easier to discount the results of these surveys than to go back and recalculate the numbers from older (and presumably less accurate) surveys that predated this technology. MCA believes it would be a more accurate depiction of status and trends to recalibrate the older numbers upward to address the differences, rather than to reduce the newer more accurate counts. To our knowledge, NMFS has not conducted such a recalibration, and we suggest that NMFS should provide such an analysis in the final Biological Opinion. This seems particularly appropriate in that the draft BiOp incorporates recovery goals into its findings and conclusions. The use of inaccurate population numbers seems inappropriate when informing the public of actions that are highly consequential to the fisheries and communities affected by these findings and conclusions.

The final Biological Opinion should also be explicit regarding the assumptions that are used to support the findings regarding reproductive rates and pup production. The analysis largely ignores the levels of uncertainty and bias in the data being used to justify the view that the population has a low reproductive rate. What are the implications of the assumption that the pup ratio on rookeries means the same in different parts of the range or the implications of just using pup ratios on “rookeries” as opposed to the overall pup ratio from all sites? With the difficulty NMFS has had with obtaining accurate and consistent pup counts in the western Aleutian Islands, what are the implications of the nature of these data for determining pup/non-pup ratios? What is the level of uncertainty with these data?

All of these factors have an important effect on the analysis and conclusions of the draft BiOp. The document needs to be revised to be more explicit in its assumptions, biases, and their effects on the findings and recommended RPA.

Similarly, the draft BiOp treats models and field work inconsistently, especially when those results conflict with NMFS conclusions. This is illustrated in the treatment of the field work by Maniscalco (2010) and conclusions of Holmes *et al*. (2003, 2007) found on page 87. Holmes developed a model and estimated relatively low reproductive rates for SSLs. Maniscalco conducted field work at Chiswell Islands to measure reproduction in SSLs. NMFS has been highly critical of Maniscalco’s work, and the critique is carried through to page 90/91 where various analyses of Maniscalco by NMFS are presented. These critiques were of the initial technical paper, and were addressed by Maniscalco in the final published work. Interestingly, Maniscalco’s findings regarding natality rates remained largely the same in the peer reviewed document. Nonetheless, the draft BiOp is largely based on a single modelling study as the basis for its conclusion of lower natality in SSLs, which NMFS in turn relies on to bolster the assumption that fisheries effects are the factor most likely to affect recovery.

We also note that NMFS staff critiques of the Maniscalco draft are posted on the NMFS website but there are no similar critiques or scientific peer reviews available for the public to review of the many NMFS documents and papers used in the BiOp analysis, including the work by Holmes cited above.

This bias continues through the analysis NMFS uses to reach its conclusion that fisheries are the most significant factor limiting recovery. NMFS has identified fourteen (14) indicators for nutritional stress in SSLs, and only one (1) out of fourteen was determined to have a positive correlation. And, even in this case, the data are highly uncertain and equivocal. This selective approach to interpretation of the available data and research, coupled with the apparent decision to not include other important work that conflicts with the BiOp conclusion, gives the appearance that the draft BiOp analysis was designed to fit a preconceived determination.

<u>3. The final Biological Opinion needs to reassess the role of predation as a factor affecting recovery.</u>

The draft BiOp inappropriately discounts killer whale (KW) predation as an important factor affecting recovery of SSLs. The SSLRP rated KW predation as a likely factor affecting recovery of the WDPS, and the majority of the recent scientific literature supports this conclusion. However, the draft BiOp largely dismisses the role of predation in recovery of the stock. The analysis needs to more fully describe the basis whereby it concludes that KWs are only a “possible” stressor. On the surface, it appears that the analysis is more focused on supporting the conclusion that KWs are not a significant factor than assessing the effects of KW predation on recovery.

In this regard, the draft BiOp is inconsistent in its characterization of KWs in the Aleutians. The draft BiOp seems to indicate that transient KWs are largely absent from the western Aleutian Islands. However, this appears to be in conflict with findings that KW predation was a dominant factor in the decline of sea otters in the Aleutians, including the western Aleutians. Does NMFS honestly believe there are no transient KWs present in this area? Or, is the lack of sightings in the western Aleutian Islands more a function of marine mammal observer effort?

The draft should also update the population estimates for transient KWs in the WDPS region. The draft 2010 stock assessment report (SAR), recently released by NMFS [Federal Register, Vol. 75, No 149, Aug. 4, 2010], notes a 75% increase in the number of transient KWs in the Gulf of Alaska/Bering Sea/Aleutian Islands over what was reported in 2009. The impact of this significant undercount of past and present SSL populations is not discussed in the draft BiOp. Estimates of SSL consumption by KWs cited from previous reports most likely represent an underestimate, perhaps a significant underestimate, of the rate of predation and its effects on recovery. Given this new information, the rate of predation by transient KWs on SSLs in the WDPS should be updated in the final Biological Opinion.

MCA also notes that Dahlheim *et al.* (2008) document movements of 2,260 kilometers by transient KWs from Southeast Alaska to Monterey and speeds of 42.4 kilometers/day. These speeds and distances suggest that it may be difficult to obtain a realistic count in the western Aleutians unless significant effort is put into research in this region to assess predation effects (see comments on research below).

KW predation of SSLs is generally acknowledged but poorly documented. As such, empirical data on the frequency of predation events are lacking. However, Maniscalco *et al*.(2008) paid special attention to mortality of pups caused by KWs and reported occurrences in two out of seven years which accounted for 25% of the observed mortalities over the seven years of observations. Horning and Mellish (2009) implanted satellite-linked post-mortem data transmitters in 21 juvenile SSLs. Data recovered from 5 transmitters indicated that 4 of the juvenile sea lions had died a traumatic death consistent with predation by KWs. The authors concluded that, although the sample size is small, their data are consistent with current understanding that the largest source of natural mortality for juvenile sea lions is KW predation.

The only real insights we have regarding the impacts of KWs on SSL recovery come from modeling exercises (Williams *et al*. 2004; Guenette *et al*. 2006; Guenette *et al*. 2007) and the life history tagging project conducted by Horning and Mellish (2009). Several of these studies conclude that KW predation is a significant source of natural mortality of SSLs, and that small numbers of mammal eating KWs could have a significant impact on SSL populations. These studies also point out that the impact of predation increases as SSL populations decline or are at low numbers because predators were consuming a disproportionately greater number of SSLs relative to the size of the SSL population. These results are consistent with Durban *et al*. (2010) that even a small number of KWs in the central Aleutians has the potential to limit recovery of depressed sea lion populations.

However, the inconsistency of taking information such as this and incorporating it into the findings regarding factors affecting recovery, and subsequent RPA, carries through the draft BiOp. The draft BiOp acknowledges that, in the western Aleutians, a combination of factors including KW predation has possibly affected SSLs. The draft BiOp also notes that, although current surveys have yet to observe a transient KW in Rookery Cluster Area (RCA) 1, high numbers have been surveyed in RCAs 2 and 3. The analysis concludes that "there is ample literature to suggest that in some areas, particularly areas of low Steller sea lion abundance (e.g., the central Aleutian Islands), killer whale predation can be an important factor in either causing continued declines or contributing to a lack of a robust recovery (*see* Williams *et al*. 2004, Williams 2006, Guinette *et al*. 2007, Heise *et al*. 2003, Durban *et al*. 2010)" [Page 167]. However, the draft BiOp ultimately rejects KW predation as a significant factor affecting recovery because "the data to evaluate this hypothesis are unavailable." [Page xxiii].

This indicates that the analysis in the draft BiOp was focused more on discounting KW predation as a significant factor affecting SSL recovery, rather than objectively assessing factors affecting SSL recovery.

<u>4. The basis for measures outside critical habitat is flawed.</u>

MCA is concerned that the draft BiOp, and the proposed RPA, represent a de-facto re-designation of CH for SSL. SSL habitat usage was evaluated and CH designated in the 1990s through a specific scientific and rulemaking process. A re-designation process has not been undertaken, yet the draft BiOp seems to have expanded CH without undergoing review of available evidence or the appropriate rulemaking. The RPA proposed with the draft BiOp would close areas outside SSL critical habitat (CH) through reliance upon telemetry data showing SSL foraging trips beyond CH boundaries. This justification is flawed, and the RPA should be reevaluated in this regard.

First, despite NMFS making much over the "outside CH" telemetry data, the fact is that in the Aleutian Islands, virtually 100% of the telemetry "hits" are inside 10 miles during "winter" when most of the fishing takes place (Table 3.11). The draft BiOp fails to breakdown the percent of 'hits' within the 10 mile zone, but review of data NMFS made available to the public a few years ago revealed that SSL use of CH was even closer to shore during this critical period, with the most foraging behavior inside 3 miles**.**

Second, in determining the need for measures outside CH, the draft BiOp relies on the results of a relatively small number of tagged male juvenile animals (3) that apparently originated outside of area 543 who ventured into 543 in waters off the shelf and in the basin in May/June. These animals were foraging over water depths that indicate that groundfish (Atka mackerel, cod) were not the prey species these animals were after. This was acknowledged during NMFS' presentation to the SSC, where it was stated that these animals were probably foraging on squid and lantern fish or other pelagic species.

Importantly, the Recovery Plan and this BiOp have acknowledge the high survivability of juveniles and adult SSL, and the focus of concern is on the reproductive capacity of adult females as the sole link to potential nutritional stress as a factor affecting recovery. It might be argued that the presence of three juvenile males foraging beyond CH is a surrogate for the potential for female foraging behavior in these areas. However, the data seem to indicate that female SSL that were tagged were not present in these offshore areas. This further calls into question the basis for the RPA measures outside CH.

Third, the draft BiOp cites work by Boor (2010) that attempts to use historical platform of opportunity (POP) data to evaluate habitat usage by SSLs. This analysis attempts to use data that were not collected for the purposes used by Boor. These data poorly record important parameters, such as the age and sex of the observed animals. Marine mammal observer effort data, which are necessary for evaluation of distribution and density estimates, are also lacking.

Even recognizing the limitations and caveats needed for the use of the POP data in Boor, the BiOp has failed to take a hard look at what the data say about SSL locations in the central and western Aleutian Islands. The vast majority of offshore sightings over the long time series of data are once again in deep ocean areas where upwelling layers create pelagic feeding areas for surface fishes. The POP database shows that substantial ship effort has occurred on the shelf areas in central and western Aleutian Islands but relatively few SSL sightings have occurred there.

For the most part, this report is at best a reflection of past research efforts and the distribution of observers on these platforms, rather than SSL foraging behavior.

Fourth, and finally, the draft BiOp attempts to connect the presence of SSLs outside CH with scat samples, the majority of which were taken from terrestrial sites far from the area where the telemetry data show the animals foraging. Scat data are useful to identify the prey species utilized by SSLs in the nearshore environment close to the haulout or rookery. They in no way are indicative of SSL use of prey such as Atka mackerel or cod in waters of a depth where these prey are not available and several days away from the terrestrial site where the scats are sampled.

In such circumstances, the draft BiOp should be redrafted to reflect the limitations of the data and present a more balanced assessment of habitat usage and a more realistic description of the facts, and the RPA should be adjusted accordingly. Limitations on fishing operations in areas not critical to the health and recovery of the SSL WDPS is precedent-setting and requires a much higher threshold of certainty which is lacking in this document.

<u>5. The draft Biological Opinion relies on non-standard methodologies to assess the effects of fishing on prey availability to SSLs.</u>

The draft BiOp inappropriately relies on non-standard methodologies to determine the rate of removals (and hence fishery impacts) of SSL prey. The draft provides no explanation why NMFS deviated from the use of the standard stock assessment method which has been extensively reviewed by NMFS and the broader scientific community. This again raises the question whether NMFS' analysis is based on the best scientific information available.

The analysis in the draft BiOp is largely based on the "footprint" white paper (AFSC 2010a) which uses a non-standard method for calculating fishery removals within the RCAs, mistakenly concluding that fisheries are taking a higher percentage of the available SSL forage in the central and western Aleutian Islands than in other areas (*see* page 338 and throughout Chapter 8's rationale for RPAs for the western and central Aleutian Islands, and the "synopsis of fishery and catch metrics" (Table 5.2a). The catch rate calculations in Chapter 5 depart from methodologies used in the normal stock assessments and SAFE reports, which set the standard whereby NMFS and the Council determine fish stock status and set harvest rates. The stock assessments use rolling averages of the last four (mackerel) or last three (cod) Aleutian Island trawl surveys to apportion the stock assessment biomass for each species between Aleutian Island sub-areas. There is a well recognized inherent variability associated with using trawl survey results for smaller sub-areas due to the patchiness of fish distributions in the Aleutian Islands. The methodology used in the stock assessments recognizes these limitations and accounts for them.

The catch rate analyses of the BiOp shown in Table 5.2a follow the methodology of the Fritz and Logerwell "white paper" (AFSC, 2010a). This work has not been peer reviewed or sanctioned by the stock assessment team at NMFS, the Plan Team or the SSC. In fact, Dr. DeMaster acknowledged (in his SSC and Advisory Panel presentations) that this analysis relies on some very questionable assumptions as the authors attempt to calculate catch rates at the extremely small scale of the RCAs. NMFS has provided little justification for using smaller spatial groupings (RCAs) rather than management areas set out in the fishery management plans (AI 543, 542, and 541) or in the SSLRP itself.

Here again is a clear example of the inconsistencies between the draft BiOp and other NMFS analyses, and of NMFS not utilizing the best scientific information (or, in this case, assessment methodologies) in its ESA analysis. It is unclear why NMFS did not rely on the standard practice that is widely recognized both within NMFS and by the wider scientific community. Having not been peer reviewed or otherwise properly vetted, the unpublished "footprint" white paper (AFSC 2010a) should not be included in this BiOp, or the analysis should be substantially modified as recommended by the SSC (see our comments paragraph 7 below).

<u>6. The Biological Opinion needs to clarify the basis of the Rookery Cluster Areas and describe fully how the RCAs mesh with existing stock assessments and affect management.</u>

MCA has significant reservations regarding the draft BiOp's use of the RCAs for analysis and development of the RPA. MCA believes that the basis for designating the RCAs needs to be explicitly described in the BiOp. The SSC raised similar concerns (see comments below). The draft cites Fritz *et al*. (2008) as being the source, but the paper does not describe having done a statistical analysis to make these divisions.

The BiOp also needs to provide a detailed description how current SSL survey data and fishery stock assessment data fit within these fine scale subdivisions. This analysis should present the levels of uncertainty and be explicit regarding the limitations of use of the RCAs as management subdivisions.

MCA would also note that the RCAs appear to be yet another attempt to further subdivide SSL populations over and above the units used in the SSL Recovery Plan. The establishment of those subdivisions, and their relationship to the recovery criteria, were quite controversial at the time. The overarching requirement of the ESA is to seek recovery of the WDPS at the DPS level. It is questionable whether or not measures can be developed, and their effect on recovery measured, at the scale of the RCAs given the data available. MCA is strongly opposed to the RCAs becoming the new de-facto management regime without these designations going through a robust scientific and public review process.

7. The draft Biological Opinion and supporting documents should be modified in accordance with comments by the Council's Scientific and Statistical Committee.

At the special August meeting of the Council, the SSC met and considered the draft BiOp. The SSC's review of the 800 page draft, like the Council's and the public's opportunity to comment, was conducted over a very brief period of time. As a result, the SSC review was relatively abbreviated, hitting some of the major issues but not as thorough and considered as would normally be the case with a document of this importance. However, the SSC's detailed comments on sections of the draft BiOp and associated white papers warrant serious consideration, and MCA believes the final product should address the SSC's concerns. Specifically, MCA wants to highlight the following:

Chapter 3. MCA concurs with the SSC comments that the final Biological Opinion needs to explicitly articulate the process whereby the RCA boundaries were determined. These new subdivisions have important implications for the analysis and development of the RPA. Are the RCAs based on a statistical analysis of the data, or are they based on personal observation or opinion? The draft cites Fritz *et al*. (2008) as being the source, but the paper does not describe having done a statistical analysis to make these divisions.

Chapter 5. MCA agrees with the SSC comments regarding the "footprint analysis" and recommends that the SSC's detailed comments on the white paper (SSC minutes, page 6-8) also be incorporated into NMFS' analysis. While it has been stated that the white paper did not factor into the conclusions of the draft BiOp, it is evident that the paper is widely referred to in the draft and strongly influences the analysis. In fact, it appears that the same methods used in the white paper were used to calculate catch rates (historical catch as a percentage of biomass apportioned to RCA), thereby driving the BiOp's conclusions that catches have been a relatively high percentage of RCA biomass for cod and mackerel in RCAs 1-4. Either the analysis should be modified to address the concerns raised by the SSC on the white paper itself, or the paper and the associated analysis should be dropped.

MCA also supports the SSC's comment regarding the parts of Section 5.1.2.2 that rely on the white paper to conclude that the management measures implemented in the 2000s have had a positive impact on reducing the impacts of the fishery exploitation strategy on SSLs. The SSC suggests the authors re-evaluate that conclusion. MCA concurs. Either of the other two explanations on page 276 could be equally valid and supported by the existing data. There is also an additional explanation that a large body of the scientific literature supports, namely, that ecosystem changes coupled with predation could be the most significant factors affecting recovery. Any of these would be consistent with Dr. Boyd's comments that the existing measures at least did no harm to SSLs but that there is little empirical evidence that they are the reason for the stock's recovery.

Chapter 8. MCA concurs with the comment by the SSC that the scale of the areas in the RPA that are fished compared to the scale of the area closed seems to be mismatched.

8. The draft Biological Opinion ignores controversial aspects of the SSL Recovery Plan.

The draft BiOp does not consider or account for the controversy surrounding certain aspects of the SSLRP, especially the recovery criteria that require that no two adjacent sub-regions be "declining significantly," as well as the treatment by NMFS of the determinations regarding factors other than fishing that may be affecting SSL recovery. In fact, several of the members of the SSL Recovery Team withdrew their support for the Plan when NMFS revised it after they had completed their work. (See enclosure 3).

We are not going to go into all of the issues surrounding the SSLRP in these comments, except to note that at the time NMFS maintained that the SSLRP was more a planning document and that the recovery criteria were to be considered "guidelines." The draft BiOp now treats the recovery criteria less like guidelines and more like hard and fast requirements. This after-the-fact change in policy, in our mind, undermines the intent and spirit under which the SSLRP was first developed.

Attached as enclosure (2) are our previous comments on the SSLRP and related documents. We request that these be treated as an integral part of our present comments.

9. Cumulative Impacts.

MCA would note that the BiOp and the EA should give some consideration in the cumulative impacts analysis to the amount of great circle shipping traffic in the near vicinity of Buldir and Shemya and its potential impact on SSL in the area (*Vessel Traffic in the Aleutians Subarea*, Nuka Research & Planning Group (Sept. 20, 2006)).

10. MCA Supports the Council Action and Proposed RPA.

MCA urges NMFS to carefully consider the revised RPA proposed by the Council. From our comments above, as well as a significant body of the scientific literature, it is not unreasonable to conclude that fisheries are not a significant factor affecting recovery of SSLs. In fact, there is a growing body of evidence that SSLs may be responding to carrying capacity and ecosystem changes coupled with predation. However, if NMFS ultimately concludes that action needs to be taken in the western Aleutian Islands, then we believe the Council proposal is a viable and reasonable alternative approach. We base our support on the following:

- The RPA should focus on CH. Adopting measures outside CH is a significant departure from the existing management regime, and the data do not support adopting such far reaching measures.
- All CH is not created equal. The majority of the data shows that SSL foraging occurs within the 10 mile zone around rookeries and haulouts. The Council proposal provides a more focused approach that addresses the differences.
- There are no overfished stocks in the Aleutian Islands. The conservative management regime for setting Acceptable Biological Catch (ABC) and Total Allowable Catch (TAC) already builds in buffers from overfishing. The issue is prey availability not overall quantity. If an additional buffer (reduction of TAC/ABC) is warranted, it should be reflective of historical catches and actual effects within CH.

The result of the proposed modified RPA is that mackerel measures would close CH in 543 completely (no retention of mackerel in any target fishery), close a substantial area outside CH in 543, extend the Buldir closure to 20 miles, and limit the mackerel fishery in the area outside CH to a level not to exceed 65% of the Western Aleutian Islands ABC. This is reflective of the average catch outside CH in the period 2000-2008, and should be considered conservative because mackerel ABCs and TACs are already divided into 3 sub-areas.

For area 542, CH would be closed for trawling for Atka mackerel around Amchitka, and a limited Atka mackerel fishery inside CH fishery would be allowed which would be spread out evenly over the year and be limited to an amount of mackerel comprising considerably less than 5% of the mackerel biomass in that area as estimated in Fishery Interaction Team studies.

The result of the proposed modified RPA on the Pacific Cod trawl fishery in the Aleutian Islands is as follows. For Area 541, a no fishing zone is extended out to 10 miles in RCA 5 (170 W to 174 W) from the current status quo of 3 miles. For Area 542, much of the area outside CH is closed to fishing by requiring no fishing in CH west of 178 W and limit trawl harvest just to the A season. For Area 543, under the modified proposed RPA, the fishery will be limited in time, area and amount (Feb. 15 – March 15, West of 174 degrees 30 minutes E outside of 10 nm, no more than 2.5% of the BSAI TAC).

MCA believes that the Council's proposed RPA would meet the requirements of the ESA with less economic and social costs to the fisheries and communities involved, and urges NMFS to adopt this revised RPA in its final BiOp.

MCA also supports the schedule and process outlined by the Council, including the need for careful consideration of a process for scientific review of this BiOp as well as the overall research program for SSLs.

11. Comments on the Environmental Assessment/Regulatory Impact Review.

The EA that was made available to the public, and commented on by the SSC and Council at the special August meeting, was obviously an incomplete document. The authors acknowledged as much, stating that they were in the process of revising the draft even as the meeting was going forward. As such, it is virtually impossible to provide complete and specific comments on the available draft. This is echoed in the SSC comments on EA, where the "…SSC finds that the draft analysis does not presently provide a fully sufficient basis for public review of the likely environmental, economic, or social impacts of the alternatives."

Having said that, MCA would note some concerns regarding the EA that should be addressed as the document is revised and completed.

MCA is concerned about the lack of information in the EA regarding the effects of this action on the Aleutian Islands fishing communities of Adak, Atka, and Dutch Harbor/Unalaska. Adak is highly dependent on fishing and fishing related support industries for its economic and social survival. We are all familiar with the struggles Adak has had to secure a stable fishing economy, and the RPA proposed in the draft BiOp will significantly constrain any resurgence of Adak as a viable fishing community and fishery support center. Adak's revenue stream as a fuel provider, shoreside support, and crew change port for at-sea processors fishing for Atka mackerel and cod fisheries in 543 and 542 will for all intents and purposes cease.

Similarly, many businesses in Unalaska are highly dependent on a varied set of fisheries throughout the year. This multi-fishery base allows these businesses to justify the costs of having a service office in Unalaska. The business from boats fishing in the Aleutians is critical to these support businesses because it is a sizable component of overall revenues, and provides revenues when pollock and other Bering Sea fisheries are inactive or operating at lower levels. Additionally, the impact of the RPA on fish landing taxes to Unalaska and other communities needs to be fully assessed, along with the impact that the NMFS RPA would have on Aleutian Islands communities' ability to maintain public and government services.

The EA also fails to accurately characterize the impacts of the proposed action on the Amendment 80 (A80) catch share program and the fleet involved in the A80 cooperative system.

For example, the EA fails to consider the realities of a cooperative made up of competing business entities. Many A80 vessels and companies tend to have spent most of their operating history in Atka mackerel, or some combination of Aleutian Islands mackerel and cod. These companies in some instances do not have large shares of flatfish rights. To switch from Aleutians fisheries to Bering Sea flatfish, they would have to purchase or lease additional rights to flatfish and associated Prohibited Species Catch (PSC) from other A80 participants. In a competitive business environment purchasing or leasing such quota will be difficult and expensive.

The EA further underestimates the domino effect of shifting effort from Aleutian Islands fisheries to fisheries in the Bering Sea and Gulf of Alaska, and the effects on fisheries and participants not directly affected by the RPA. The EA needs to fully describe the potential for impacts of increased effort in these fisheries, including exacerbation of the race for fish in fisheries that are not managed under catch share systems, the effects on bycatch and PSC management, the potential for grounds crowding and increased gear conflicts (especially in the cod fishery), the potential for premature or unnecessary fishery closures, the various scenarios where changing ABCs have differing effects due to the 2 million ton cap, and other downstream conservation and management issues.

Finally, we would note the comments of the SSC on the draft and the SSC's request to have additional opportunities to review the document once it is completed. We understand that the SSC comments will be addressed in a revised draft.

12. Concerns with the administrative process now underway.

It is apparent that the level of analysis and the timeline for review are not up to the usual rigorous standard for analyses and decision documents used to develop management measures in the North Pacific. The special August Council meeting highlighted the compressed timeframe for review and comment on the draft BiOp and EA. Clearly, there was not adequate time for a robust scientific review or thoughtful public comment.

The discussion at the meeting also underscored the confusion regarding the process for approval. The rationale for not following the normal MSA process was not well explained, and it appears that NMFS' proposed approach to implementation of the RPA has been adopted largely for reasons of administrative convenience and contrary to the President's Open Government Directive (Dec. 2009). NMFS should carefully assess whether its proposed approach is fully compliant with its procedural obligations under the law. If NMFS continues down its present course, MCA believes that it is incumbent upon the agency to explain with greater precision and clarity the basis for its conclusion that proceeding by way of direct final rule and EA is fully consistent with the requirements of the MSA, NEPA and the APA.

13. Scientific Review and future research.

MCA has been a strong proponent for an open, transparent and robust scientific review of the biological information used to make determinations such as those found in the draft BiOp. In fact, MCA was a strong supporter of the process that had been outlined by the Council and agreed to by NMFS early in this process for peer review of the draft BiOp. This process would have had NMFS make the draft available to the Council and the SSC with sufficient time for a thorough SSC and Council review, and meaningful review and comment by the public. The results of this process -- Council and SSC comments/critiques, comments or critiques by outside experts, and any public comments -- would have been forwarded along with the draft for peer review by an outside entity such as the Center for Independent Experts (CIE). This would have provided the CIE with a full suite of comments and critiques as well as NMFS' presentations, to set the context for peer review of the draft. The Council would also have been involved in helping shape the terms of reference for this review.

However, now the situation has changed. Even with the one week extension for public comment, the time allotted has not been sufficient for an in-depth review of the draft BiOp by the SSC, the Council, or the public. Any peer review will be after the draft is revised and completed as a final Biological Opinion. Given this set of events, MCA believes that a more comprehensive scientific review, of which review of the draft BiOp would be one part, is warranted. Such a review should be more far reaching, and investigate what we have learned over the intervening decade since the first SSL Biological Opinion was implemented. It should identify specific gaps in our understanding of factors affecting SSL recovery, including the role of predation and environmental change (changing carrying capacity) on recovery of SSLs. Particular attention should be devoted to the issue of natality and "chronic nutritional stress" which is the only factor the draft BiOp found to be a possible link to fishery related impacts to SSLs. The review should be conducted by an independent and highly regarded group of experts, and should lead to a specific research plan and budget.

We appreciate the opportunity to comment during the limited time available, and we hope you will take the time to carefully consider the comments submitted by outside scientists and the public.

Sincerely,

David Benton
Executive Director

Copy: Governor Sean Parnell, State of Alaska
Governor Christine Gregoire, State of Washington
Senator Lisa Murkowski
Senator Mark Begich
Senator Patty Murray
Senator Maria Cantwell
Congressman Don Young
Honorable Gary Locke, Secretary of Commerce,
Ms Jane Lubchenco, Undersecretary of Commerce for Oceans and Atmosphere
Mr. Eric Schwaab, NOAA Assistant Administrator for Fisheries
Chairman Eric Olson, North Pacific Fishery Management Council

Enclosures (1) Dr Ian Boyd Cover Letter of 6 May 2010, *Assessing the effectiveness of conservation measures: resolving the "wicked" problem of the Steller sea lion*
(2) MCA Letter of 18 July 2007, Draft Revised Steller Sea Lion Recovery Plan
(3) Some Former Members of the Recovery Team Letter of 20 August 2007, Revised Draft Steller Sea Lion Recovery Plan

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Mr. Eric Olson
Chair
North Pacific Fishery Management Council

13 April 2010

Dear Mr. Olson,

I am pleased to inform you that my analysis of the population viability for Steller sea lions that I presented to the Council's Steller Sea Lion Mitigation Committee and to National Marine Fisheries Service scientists in January has been accepted in the peer reviewed journal *Biological Conservation*. The paper, entitled *Assessing the effectiveness of conservation measures: resolving the "wicked" problem of the Steller sea lion*, is attached for your information. The format of the report has changed only slightly from the one presented and this was at the request of journal editors, but the results of the analysis remain the same. These include:

- The analysis looks at the SSL population as a whole, and at the Eastern Distinct Population Segment (EDPS) and Western Distinct Population Segment (WDPS) individually.
- All other things being equal, future scenarios based on information about historical dynamics suggest that the SSL meets conservation objectives for the population as a whole and also for the EDPS and WDPS segments.
- For the Western DPS, the population meets the conservation objective (less than 1% chance of extinction in 100 years) under all scenarios.
- The "pup ratio" suggests current population productivity is close to or above the long-term mean.
- Although there are differences, pup production rates based upon the pup ratio are similar between the EDPS and WDPS when all sites are considered. WDPS appears to be at long term mean, EDPS is somewhat higher.
- Current population levels may be close to long-term mean but depends on interpretation of pre-1980 counts;
- There is uncertainty about the relationships between trends in western and east parts of the range but there is a suggestion of a shift in population distribution from west to east;
- The evidence from changes in numbers and pup ratios suggest there may have been some emigration from west to east in the 1980s although this would need to be confirmed by targeted genetic studies.

University of St Andrews

- Past measures to prevent decline and promote population increase have been either neutral or successful at preventing further decline.
- Long-term stability suggests the SSL population may be close to carrying capacity.

I hope that this analysis is helpful to the Council and NMFS as you continue your work to conserve Steller sea lions and manage the fisheries in Alaska.

Yours sincerely,

Ian Boyd

Cc: Dr. Jim Balsiger

Biological Conservation xxx (2010) xxx–xxx

Contents lists available at ScienceDirect

Biological Conservation

journal homepage: www.elsevier.com/locate/biocon

Assessing the effectiveness of conservation measures: Resolving the "wicked" problem of the Steller sea lion

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ARTICLE INFO

Article history:
Received 5 December 2009
Received in revised form 22 March 2010
Accepted 7 April 2010
Available online xxxx

Keywords:
Wicked
Problem
Conservation
Policy
Marine
Mammal
Pacific
Alaska
Ecology
Population
Viability
Risk
Extinction

ABSTRACT

"Wicked" problems are those that are complex and that change when solutions are applied. Many conflicts in conservation fall into this category. The study approached the problem of how to constrain the apparent wickedness of a problem in the conservation management of a species by using simple empirical indicators to carry out iterative assessment of the risk to a population and to document how this risk evolves in relation to the addition of new data and the implementation of management actions. Effects of high levels of uncertainty within data and also concerning population structure were examined through stochastic simulation and by exploration of scenarios. Historical trends in the example used, the Steller sea lion, showed rapid declines in abundance in some regions during the 1980s. The current total population is 130,000–150,000 Steller sea lions through Alaska and British Columbia and this number has been stable since about 1990 in spite of regional differences in population dynamics. Regional differences in the sequence of changes in the number of pups and non-pups, suggested that an internal re-distribution of juveniles could have happened between 1980 and 1990. Current productivity also appears close to the long-term mean. Stochastic population projection using various scenarios showed that, based upon this history, the risk of extinction for the population has declined and is below reasonable thresholds for considering the population to be endangered. The trends in risk suggest that management actions taken since 1990 have probably been effective. Consequently, the conservation management objectives for the Steller sea lion are probably being met. The approach provides a mechanism, based upon experience and scenario analysis, for exploring future policy options and may help to constrain the debate amongst stakeholders about the cost-benefit trade-offs associated with different options.

1. Introduction

Conservation management can be costly both in terms of the direct costs of implementation and also because of opportunity costs to social and economic objectives that may be seen to conflict with conservation objectives. Some problems of conservation management can also be described as "wicked" (Jentoft and Chuenpagdee, 2009), in the sense that they are complex, difficult to define and delineate, they have a tendency to reappear and are therefore unconstrained and they tend to change in response to a solution. This is a common feature of problems that lie at the boundary between sociology and biology (Miller, 1993; Goldsmith, 1969). In order to deal with this and to potentially constrain the apparent wickedness of these types of problem, or to "tame" them, it is important to assess the extent to which conservation management is meeting objectives and to proceed as much as possible on the basis of the available information rather than on supposition built upon poorly tested assumptions, or partial information. A challenge for conservation biology is to define clear objectives and to synthesise and simplify the problem of assessing whether conservation objectives are being met. However, in the case of species conservation, the magnitude of this challenge is exacerbated by factors such as the presence of high levels of uncertainty in data, the presence of natural variability in background environmental conditions that could be a major contributor to population trajectories and slow population response as may be the case when considering long-lived species with low intrinsic rates of increase. These types of issues are emerging as challenges to the IUCN Red List criteria used to assess the conservation status of species (Freeman, 2008; Godfrey and Godley, 2008). The present study uses the Steller sea lion (*Eumetopias jubatus*) as an example of such a problem.

The Steller sea lion has been a focus of concern in the North Pacific for at least two decades. Widespread, long-term population declines over important parts of its range have triggered responses from managers in the United States of America under the terms of the Endangered Species Act (ESA). The history of the population decline, population biology and the management response are detailed in the most recent Recovery Plan for the Steller sea lion

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0006-3207/$ - see front matter © 2010 Elsevier Ltd. All rights reserved.
doi:10.1016/j.biocon.2010.04.006

Please cite this article in press as: Boyd, I.L. Assessing the effectiveness of conservation measures: Resolving the "wicked" problem of the Steller sea lion. Biol. Conserv. (2010), doi:10.1016/j.biocon.2010.04.006

(National Marine Fisheries Service, 2008). For the purposes of management, the population has been considered to exist as two distinct stocks known as the Western Distinct Population Segment (WDPS) and the Eastern Distinct Population Segment (EDPS) with a dividing line between these stocks at Cape Suckling, Alaska (144°W). The WDPS has been classified as "endangered" and the EDPS has been classified as "threatened" under the ESA. This has placed a duty upon the US National Marine Fisheries Service (NMFS), which is the responsible agency, to develop and implement a recovery plan for the Steller sea lion.

The presence of the largest commercial fishery in the United States, when measured by volume, mainly for walley pollock (*Theragra chalcogramma*, http://www.nmfs.noaa.gov/fishwatch/species/walleye_pollock.htm), that is broadly spatially coincident with some locations where declines have taken place has added complexity to the management of the Steller sea lion populations. Evidence for an indirect effect of fishing is difficult to find (e.g. National Research Council, 2003; Wolf and Mangel, 2008) and the consensus at present suggests that the population is likely to be tracking a change in the environmental carrying capacity that appears not to be driven by fishing (Trites et al., 2006).

Specific criteria that may need to be addressed when managing wildlife populations are to ensure that the population is maintained at a viable level; that there is an appropriate response to any significant decline; and that the historical range of the species should be maintained. Some criteria may also add the objective of managing populations to be functioning parts of ecosystems. Two problems presented by these criteria, and illustrated by the Steller sea lion, are the underlying assumption of a stationary carrying capacity and the interpretation of historical distribution and abundance as the target level for the species. This has led to the belief that "recovery plans" (e.g. NMFS (2008)) can be created to counteract range contraction or declines in abundance. Some situations, such as deforestation, lend themselves to providing clear evidence of changing carrying capacity but in others, such as the Steller sea lion, this is much less clear (Trites et al., 2006). Because of the way in which conservation legislation is often framed, managers are left with the unenviable task of operating without clear objectives. In the most difficult cases they also have few tools for management other than to speculatively reduce possible conflicts with anthropogenic factors, often setting up costly and socially divisive conflicts. As in the case of the Steller sea lion, where management measures have restricted fisheries activities, the cost-benefit trade-off of conservation measures can be highly uncertain.

An approach often adopted by managers to justify conservation measures is to assess the risk of extinction of a population using population viability analysis (PVA), or similar methods. For long-lived species, a probability of quasi extinction of less than 1% in 100 years has been suggested as the threshold for considering a species to be endangered (Angliss et al., 2002; DeMaster et al., 2004). For the Steller sea lion the quasi-extinction level has been equated to a total population size of 4743 Steller sea lions (NMFS, 2008). This number arises if one sets the cut-off using a "genetically effective population size" of 1000 animals (DeMaster et al., 2004).

PVAs use current knowledge of past population dynamics as well as general life-history information to predict future trajectories. Although PVAs may be broadly satisfactory for predicting extinction risks (Brook et al., 2000), these predictions may be sensitive to factors such as spatial and temporal correlation within the data structures (Melbourne and Hastings, 2008). To date, at least eight models have been developed for Steller sea lions to examine extinction risk (York et al., 1996, three models; Gerber and VanBlaricom, 2001, three models; Winship and Trites, 2006; NMFS, 2008). All have produced broadly similar predictions. However, the existence of so many PVAs suggests a need for ongoing tracking of extinction risks as a way of assessing the performance of management measures against a consistent standard.

The objectives of the present study were to develop a method for assessing the performance of management against a background of high uncertainty with the ultimate aim of helping focus decision-making about management upon robust principles. To achieve this, it was first necessary to collate, assess and analyse the historical population data, a task that had surprisingly not been done for Steller sea lions since the early 1990s (Trites and Larkin, 1996; Loughlin et al., 1992). The analysis focussed upon survey estimates of the number of Steller sea lions at rookeries and haulouts which is by far the richest source of data about the Steller sea lion and for which there are comparable range-wide estimates. This was followed by stochastic simulation of population futures based upon historical data (including its uncertainties) associated with different periods in the management history of the population to assess the risks to the population under different scenarios, to examine how sensitive the risks are to the addition of new data and how well management may have met its objectives.

2. Methods

2.1. Count data and definitions

All data were obtained from the Alaska Fisheries Science Center Steller sea lion database (http://www.afsc.noaa.gov/nmml/alaska/Stellersealionhome/databases/) as well as more recent sources (Fritz et al., 2008; Olesiuk, 2008). Although there are rich data sources for Steller sea lion biology, the data that has been collected most consistently and most reliably are counts of pups at "rookeries" across the species range and counts of non-pups (juveniles and adults) at "haulouts" during the breeding season in June and July. In the present study, no distinction was made between "haulouts" (sites where animals rest out of the water) or "rookeries" (where pupping takes place) during the breeding season. This is because the historical distinction appears to be somewhat arbitrary, e.g. Pitcher et al. (2007) decided that any site having <50 pups was classified as a "haulout" rather than a "rookery".

The principal source of data about Steller sea lions came from periodic surveys of these sites. These data have been collected at the resolution of individual sites but had previously been reported at various scales by grouping sites into sub-regions, trend sites, regions and stocks (e.g. York et al., 1996). An important underlying feature of the present analysis is that it was based upon the finest scale of data collection and included all count data, not just the six range-wide surveys (e.g. as used in the PVA in NMFS 2008). This meant that the analytical approach was designed to cope with partial coverage in some years and missing values for many sites. Consequently the need for aggregation of sites into "trend" sites (Fritz et al., 2008) or by region (Sease and Gudmundson, 2002; Pitcher et al., 2007; Fritz et al., 2008; Olesiuk, 2008) was avoided. Any aggregation of sites was based upon clustering derived from congruence between the population dynamics shown by sites (see Supplementary information for details about the methods used to cluster sites). However, the analysis aggregated sites across the EDPS and WDPS to allow analysis with respect to the historical approaches to management which have recognised these segments as distinct management unit.

Although past studies have fitted population models to current count data (e.g. York et al., 1996; Winship and Trites, 2006; Holmes et al., 2007), the largely unknown and non-stationary error structure within the count data means that the capacity of the data to support these types of analyses is quite limited. Consequently, the present study did not attempt to directly model the population dynamics of Steller sea lions but it did produce an index of productivity called the "pup ratio", which was the number of pups counted as a proportion of the number of non-pups counted.

2.2. Observation error

Various methods have been used to count Steller sea lions. These include, aerial photography using 35 mm film format normally using an oblique aspect with hand held cameras and no motion compensation; aerial photography using medium-format (5 in.) with motion compensation; aerial photography using digital format with motion compensation; aerial counts using visual estimates; beach/ground counts; viewpoint observations; counts from a skiff near to shore, and counts from a vessel offshore (Supplementary information, Table 1). Each method will have different levels of error and in most cases these have not been quantified.

Five general categories of processes were identified that will lead to error in estimates of Steller sea lion abundance. The main processes leading to error in observation are described in detail in the Supplementary information and these include: (1) incomplete coverage of the site; (2) variable capacity to observe animals even if they are present; (3) variance due to the counter; (4) dependency on the timing of the survey in relation to both the time of day when it is carried out and the date in relation to the peak of the season (e.g. Pitcher et al., 2001); (5) dependency upon the type of terrain. Many of these processes will interact with one-another and all will vary depending upon survey method. The supplementary information evaluates the range of error associated with each of the factors and these ranges were used to generate an estimate of the overall uncertainty around the counts. This was achieved by assuming that all sources of uncertainty were independent and by modifying each count using a uniform distribution across the range of possible values for each source in the Supplementary information (Table 1).

2.3. Modelling future trends

The objective was to predict the range of future population trends conditional on past population trends and current population state. A four-step approach was adopted. Step 1 defined the process by which observations, in this case actual counts of Steller sea lions at each site, represented as a set of state vectors $\mathbf{x}_k$, where k denotes a particular site, were translated into an equivalent set of state vectors $\mathbf{n}_k$ representing the real state of the populations at a particular site. As already described, this involved accounting for the errors within the data and building distributions of possible alternatives of $\mathbf{x}_k$. It follows from this that

$$N_t = \sum_{k=0}^{k=K} \mathbf{n}_k \tag{1}$$

where N_t is the total population size in year t.

Step 2 defined the potential distribution θ of λ which is the proportional change in the state of each successive element in the state vector $\mathbf{n}_k$. This defines the historical trends in the population. Assuming exponential growth or decay, the rate of population change (λ) was derived as follows:

$$n_{k,t} = n_{k,t-\tau} e^{r\tau} \tag{2}$$

where $n_{k,t}$ was the number of non-pups or pups in the population at time t, $n_{t-\tau}$ was the number at the previous time of survey, defined by a time interval of τ years and r was the growth rate where $\lambda = e^r$. Both $n_{k,t}$ and $n_{k,t-\tau}$ were measured variables. The exponential rate of increase was derived as:

$$r = \frac{\ln(n_{k,t}) - \ln(n_{k,t-\tau})}{\tau} + \xi \tag{3}$$

In this case ξ was a random deviate defined by the standard deviation around the principal eigenvalue of a Lefkovitch matrix representing pup, juvenile and adult stages in the population and where the principal eigenvalue was equal to λ. The Weiner process described by Dennis et al. (1991) was used to estimate the value of ξ. This log-linear relationship, together with uncertainty, was used to interpolate the number of pups and non-pups at each site for years when surveys did not take place.

Step 3 was the process by which the elements within the population state vector at a site, $n_{k,t}$, was updated to define scenarios for future population trajectories. This used the distribution θ_n, where θ was subscripted by n, because λ varied depending upon the size of the population at each site. Thus the population at each site was

$$n_{k,t} = f(n_{k,t-\tau}, \theta_{n_{k,t}}) \tag{4}$$

In this case, τ was equal to unity in the projections. The value of λ, drawn from the distribution θ (Eq. (4)) was selected to satisfy the condition of a first order Markov process in which $\lambda_{k,t} = \lambda_{k,t-\tau}\alpha_\tau$, where α was a random variate drawn from a distribution defined by the direct observation of the autocorrelation between values of λ at different values of τ.

Step 4 involved aggregating the results for sites across different scales of relevance to management and assessing the probability of recovery criteria being met.

2.4. Density limitation

There was no direct information available about the carrying capacity. However, it can be assumed that there are upper limits to the number of Steller sea lions at particular sites, because of limits on space or because of limits on the local environmental productivity. To allow for this type of limit within the model, it was assumed that each site had an upper limit of population size drawn from the distribution of maximum observed values across all sites. Since the populations at many sites have only been measured while they have been increasing, this approach will tend to produce a conservative estimate of overall carrying capacity.

Table 1
Definitions of the different population risk analyses undertaken. All of these scenarios were investigated with and without density limits.

	Model 1			Model 2		
	a	b	c	a	b	c
Spatial clustering included				✓	✓	✓
Temporal correlation included	✓	✓	✓	✓	✓	✓
Basic data at the scale of rookeries and haulouts (June/July)	✓	✓	✓	✓	✓	✓
Sampled λ from empirical distribution across the all years	✓			✓		
Sampled λ from empirical distribution across phases A and B only (post 1990)		✓			✓	
Sampled λ from empirical distribution across B only (post 2000)			✓			✓
Results modified to reflect the observation process, including error and bias	✓	✓	✓	✓	✓	✓

2.5. Model definitions

Several models were used to investigate possible future trends in the population and these are summarised in Table 1. Spatial correlation of sites was included in Model 2 but not in Model 1. Both models were run on data for the whole population and also separately for the Western and Eastern Distinct Population Segments (WDPS and EDPS respectively) (York et al., 1996; NMFS, 2008). Results from all these combinations of models were investigated using data from the complete time series: (a) from 1990 to the present (b) and from 2000 to the present (c). These data sets in (b) and (c) represent two different phases of conservation management. Before 1990 there were no conservation management measures; after 1990 measures to reduce human impacts were introduced and after 2000 measures to reduce possible indirect effects of fishing were expanded. Consequently, population projection using data from 1990 and 2000 respectively assumed similar types of management will be extended into the future.

3. Results

3.1. Data available

The complete data set contained a sample of 7175 counts of non-pups and 906 counts of pups at individual sites. After exclusion of data outside the June/July pupping season and taking averages for repeated counting at individual sites within years, the total sample of site counts for non-pups was 4530 and 828 for pups. The total sample of sites from the region included within the present analysis for non-pups was 431 and the total number of rookeries was 312.

3.2. Observation errors

Taking information from the whole data set where there were repeated counts of a site within a single year, the coefficient of variation (CV) followed a log-normal distribution with a mean of 0.28 (SD = 2.492). The distribution of CVs (Fig. 1) is unlikely to have completely captured the biases associated with the characteristics of the different sites because it tested for internal consistency in within-site counts and says little about consistency among sites. Unfortunately, there was very little that could be done to assess the differences in count bias among sites even though it is possible that this could be important. For example, Chumbley et al. (1997) showed clear differences in the seasonal pattern of abundance among neighbouring beaches on Marmot Island. There was also evidence of large inter-site variation in the apparent proportion of non-pups hauled out at sites during the breeding season. The local conditions that drive these differences are not understood but all may add significantly to the overall uncertainty around counts.

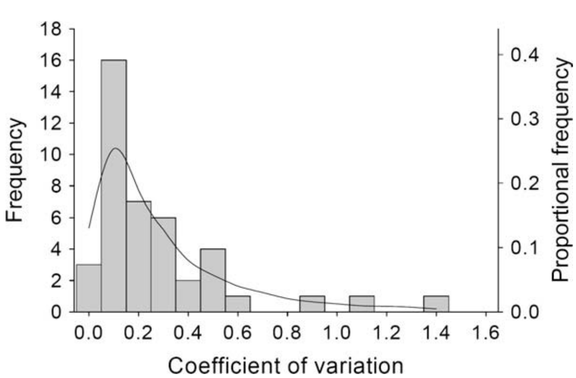

Fig. 1. The frequency distribution (histogram) of the coefficient of variation found for repeated counts of non-pups at the same site in the same breeding season. The skewed form of the distribution meant that counting error was modelled using a log-normal distribution (solid line plotted as a proportional frequency scaled to the observed frequency). The geometric mean was 0.280 (SD = 2.492).

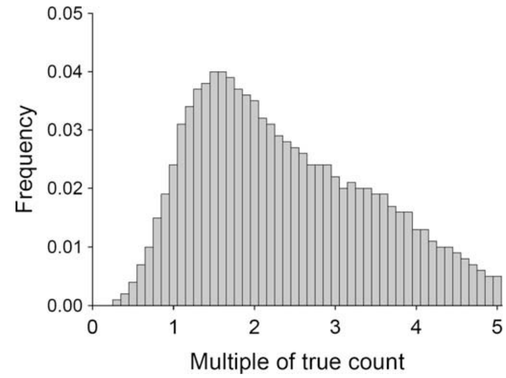

Fig. 2. The frequency distribution of N_{true} for non-pup counts expressed as a proportion of N_{obs} for non-pup counts.

The overall effect of the procedure used to account for error and bias is shown in Fig. 2 for non-pup counts. This shows how the true population is likely to be distributed as a multiple of observed population.

3.3. Historical abundance

The original interpolated counts across the whole population, together with counts corrected for error and bias are shown in Fig. 3a and d. Although the number of pup and non-pup Steller sea lions counted apparently increased between 1960 and 1980, relatively few sites were surveyed through this period so much of this increase could be caused by increased observation effort. The total number of pups and non-pups declined after 1980 even though the observation effort, in terms of number of sites included within the surveys, continued to increase.

The number of non-pups counted declined almost linearly through the 1980s so that, by 1990 < 50% of the numbers observed at the peak in 1980 were being counted (Fig. 3a). However, from 1990, the total number of non-pups has remained stable and the apparent observation effort based on the number of sites included in the counts has also changed relatively little through this time.

The number of pups observed did not begin a sustained decline until after 1985, and suggested a roughly 5-year delay between declines in non-pup abundance and declines in pup abundance (Fig. 3d). This decline continued until about 2000 and thereafter numbers counted have increased at about the same rate as the previous decline. The rapid increase in the number of sites surveyed for pups since 2000 partly reflects greater observation effort but it also reflects a reclassification of some sites into a larger number of smaller units.

The application of the procedure to account for error in the counting methods caused an increase in the estimated number of pups and non-pups. As expected from the distribution illustrated in Fig. 2, accounting for counting errors led to an approximate doubling of the number of non-pups (Fig. 3a). The confidence limits around the mean estimate narrowed through time reflecting the increasing accuracy of the counting methods used and the relative level of uncertainty was greater for pups than non-pups. Thus, based on this account, the total number of non-pup Steller sea lions within Alaska and British Columbia is between 130,000 and 150,000. The trends in these estimates follow those for the observed numbers.

The "pup ratio" before 1980 was excluded as unreliable (Fig. 3g). These data suggest that relative productivity increased

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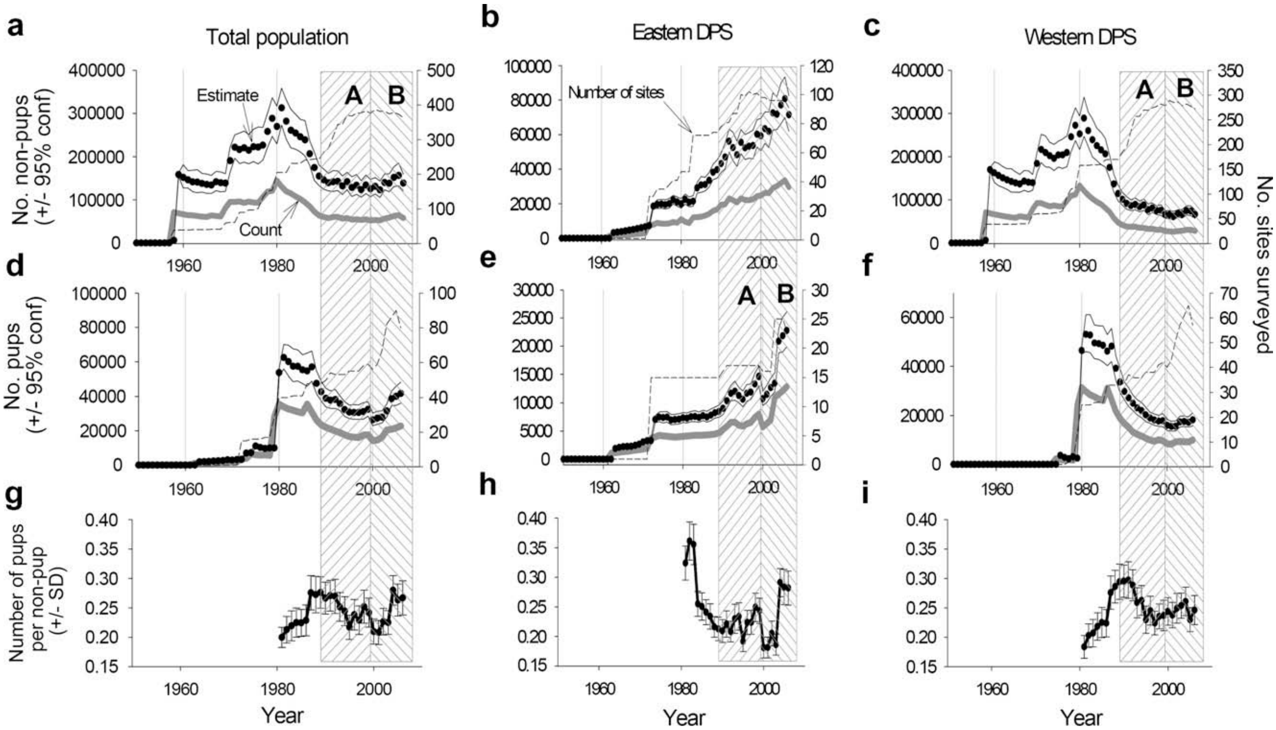

Fig. 3. Changes in the observed number of non-pup (a–c) and pup (d–f) Steller sea lions at rookeries and haulouts during the June–July pupping season (shown as a broad grey line to reflect that there is uncertainty about these numbers). Log-linear interpolation with Weiner diffusion was used to account for missing values. The dots show equivalent numbers adjusted for errors in the observation process. The solid lines above and below these dots show ±95% confidence intervals around these mean values. The dashed line shows the number of sites included in the estimation during each year. In panels g, h, and i the ratio of pups in the population to non-pups is shown ±1 SD. The shaded blocks show the two phases of conservation management of Steller sea lions.

through the 1980s, when the population was in decline. The pup ratio then declined through the 1990s when the population had stabilised and began to increase again after 2000.

The Eastern and Western Distinct Population Segments (Fig. 3b, c, e and f) followed roughly opposite trends (Fig. 4). The rapid decline in abundance through the 1980s in the WDPS meant that the population reached about 20% of its peak size and it has shown a much slower overall decline since then (Fig. 3c). The decline in pup numbers in the western segment of the population lagged behind the change in the population as a whole (Fig. 3f) and this is reflected in the increasing pup ratio through the 1980s. The pup ratio in the western population segment then declined rapidly between about 1990 and 1997 and has remained unchanged at an intermediate level, or has shown a slight overall increase (Fig. 3i).

In contrast to the western segment, the number of non-pups and pups in the eastern segment has shown a near-monotonic increase since before 1980 (Fig. 3b and e). The pup ratio in the eastern segment declined through the 1980s (Fig. 3h), at a time when the pup ratio in the west was high or increasing and when the non-pup portion of the population was in rapid decline. The pup ratio in the east has remained relatively low or increased slightly since 1990, but at a level equivalent to the pup ratio in the western population segment in recent years. The apparent low pup ratios in the eastern population segment during the early 2000s could have been caused by unaccounted bias within a single survey.

3.4. Rate of increase (λ) and the distribution of λ

The overall rate of increase at sites was heavily centred on unity (Fig. 5) which reflects the fact that most of the data came from a population that has been showing little directional change in recent years. The distribution was highly leptokurtic. Although this type of distribution could potentially be modelled using the Parieto or Generalised Extreme Value distributions, to avoid additional

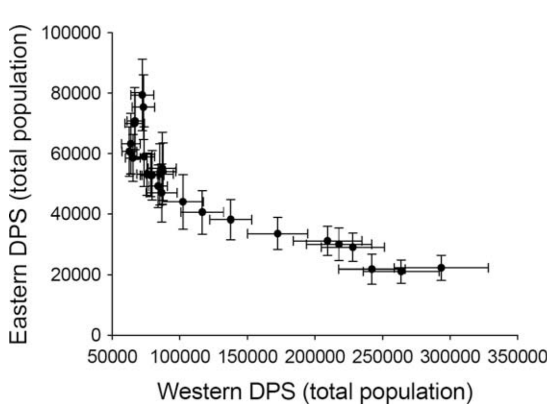

Fig. 4. The relationship between the total number of Steller sea lions estimated within in the western and eastern parts of their range. Data from 1980–2006.

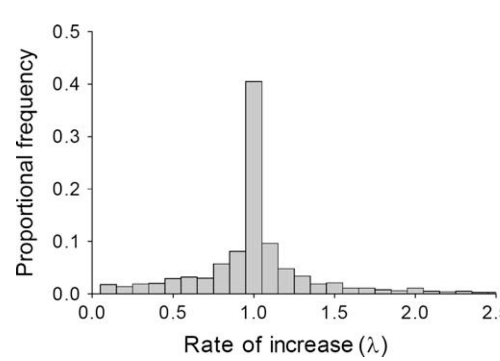

Fig. 5. The frequency distribution of λ for non-pups at rookery sites during June and July across all sites irrespective of population size ($n = 2874$).

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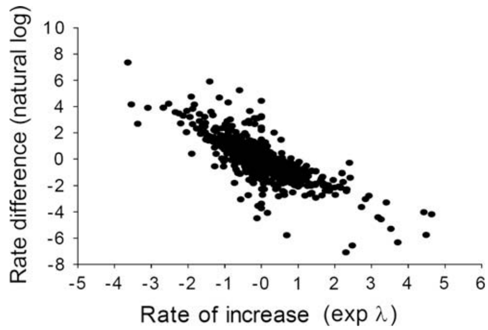

Fig. 6. The relationship between the rate of increase at each site and the change in the rate of increase the next time it was measured. Extreme values at each end of these distributions are attributable to fluctuations at small population size, which was a significant co-variate in this relationship. Ln rate difference = −0.0729 to 1.2138ln(λ); r^2 = 0.642.

uncertainty due to model fitting, bootstrap re-sampling from the empirical distribution was used to model future population growth at each site.

However, the rate of increase appeared to be affected by population size in that increasing population size resulted in lower rates of increase (Kruskal–Wallis, Chi-square = 46.07, df = 10, $p < 0.001$). Population size had a particularly strong effect upon the variance in λ. This was mainly because of high variability in λ at low population sizes presumably because of an increasing effect of immigration and emigration and stochasticity in the data. Consequently, population size was used as a co-variate in the choice of rates of increase to model future population growth (see Eq. (4)).

There was strong evidence that the rate of increase at individual sites was dependent upon the rate of increase at the previous time of measurement (Fig. 6). This autocorrelation was also included in population projections.

3.5. Future trends in abundance

3.5.1. Model 1

The results of population projections using Model 1, which included temporal correlation but no spatial correlation (Fig. 7), showed the risk of extinction depended upon which parts of the

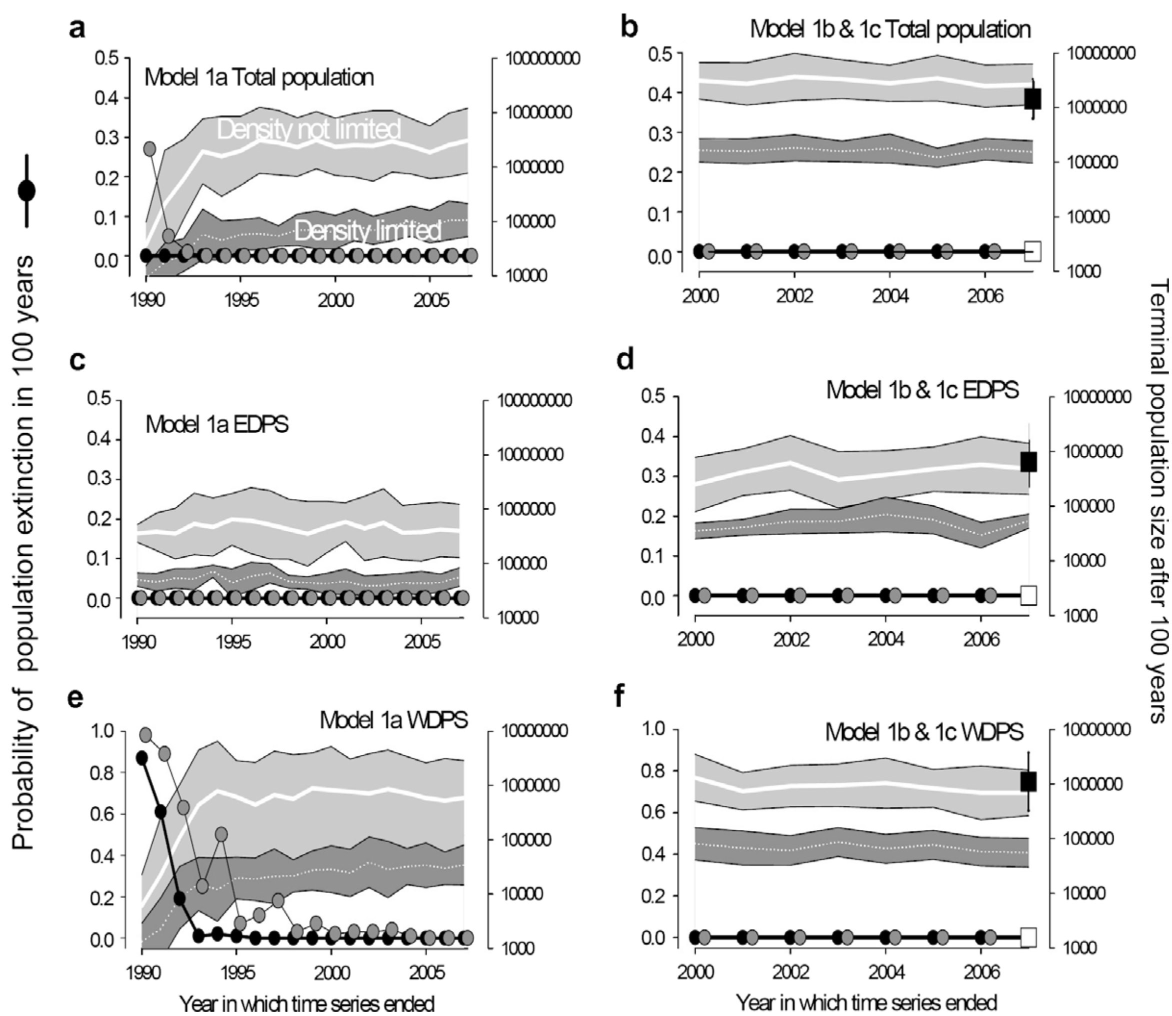

Fig. 7. The evolution of population projections for 100 years into the future using Model 1a (see Table 1 for the definition of the model and results are in panels a, c and e), and Models 1b and 1c (panels b, d and f). The projections used historical time series that included all the available historical data up to the year in which the time series was terminated (shown on the x-axis) to illustrate how the predictions evolved as new data were collected. The probability of extinction during the future 100 years is shown as dots and the terminal population size is shown as a mean (white lines) ±1 SD (shaded regions). The result of running the models without density limits at individual sites (light grey shading, grey dots) is contrasted with running the models using density limits (dark grey shading, black dots). The projections using Model 1b used historical time series that included data collected after 1990 when management measures were introduced to promote conservation of the Steller sea lion (phases A and B in Fig. 4). The projection using Model 1c used historical time series from 2000 to 2006 (phase B in Fig. 4) and the output is shown as a single shaded square (±1 standard deviation) for the predicted terminal population size and an unshaded square to show the probability of extinction. The output for Model 1c with density limits in place is not shown. EDPS signifies the Eastern stock of Steller sea lions and WDPS signifies the western stock.

time series of historical counts was used to model the rate of change in the population. The population as a whole (Fig. 7a) when modelled using all historical data had a probability of extinction that exceeded the management objective until 1993 and after that the probability of extinction was <0.1 in 100 years (referred to here as the "conservation objective"). The terminal population size after 100 years was considerably greater in the case where no density limits were used and this pattern was the same across the population segments and irrespective of which prior distribution of rates was used (contrasting Models a–c). The western population segment (WDPS, Fig. 7e) showed less of a tendency to meet the conservation objectives early in the time series but using the full range of historical data, it was compliant with the conservation objective after 2005. Using Model 1 and only the later parts of the time series of λ to project the population forward showed that in no circumstances did the stochasticity included here result in the extinction of the populations (Models 1 and 1c; Fig. 7b, d and f). The terminal size of the population as a whole (Fig. 7a) and of the western stock (Fig. 7c) continued to increase as additional data were added, reflecting the addition of information about the population as it has stabilised through time (Fig. 3a and c) and suggesting that this period of stability is increasing confidence that the population is robust to extinction. This analysis suggested that the predicted risk to the population will be relatively insensitive to the addition of new data.

3.5.2. Model 2

The results of population projections using Model 2, which included temporal correlation and spatial correlation, are shown in Fig. 8. Overall, the addition of correlation increased the probability that the population would not meet the conservation objective but this effect was only present in Model 2a (Fig. 8a, c and d) and not in Models 2b and 2c, and the present probability of not meeting the conservation objective was very low. Adding density limits produced an increased probability that the populations would not meet the conservation objective and this is illustrated by the pattern in Fig. 8c where even the eastern population segment failed to meet the conservation objective up to 1995. As with Model 1, the total terminal populations after 100 years were predicted to increase to a roughly constant level, showing that the prediction appears to be robust to the duration of the time series used to derive the sample distributions of λ.

Use of the time series from 1990 (Phase A in the management history, Fig. 8b, d and f) showed slightly elevated terminal populations compared with the scenarios using the complete time series. The terminal population levels derived using only the data from

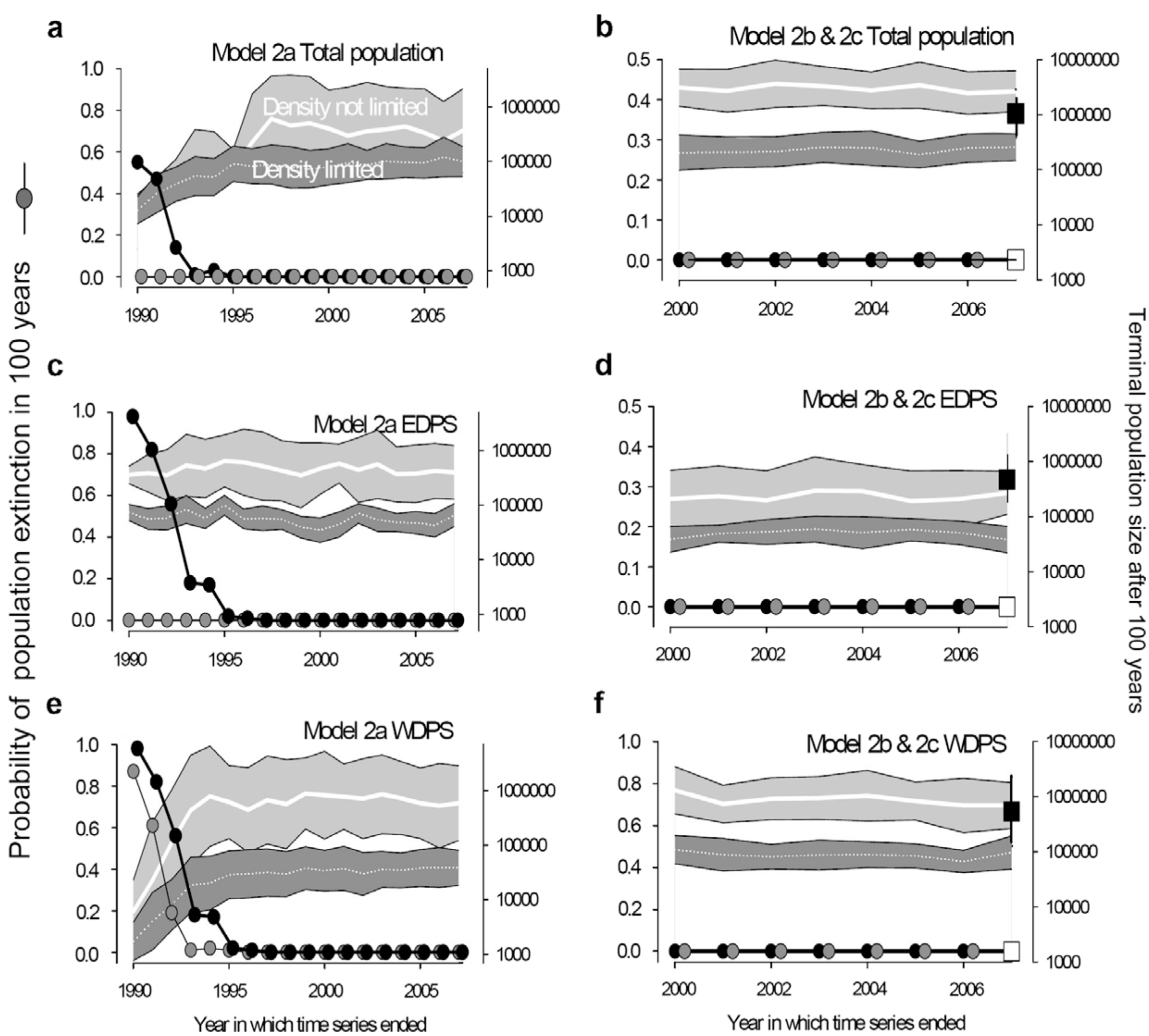

Fig. 8. The evolution of population projections for 100 years into the future using Model 2a (panels a, c and e), and Models 2b and 2c (panels b, d and f). Refer to the caption for Fig. 7 for further details about the meaning of each symbol.

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Phase B of the management history (post 2000) were similar to those derived from the distribution of λ from Phase A of the management history. When the population changes during Phases A and B were used, all scenarios suggested that the population met the conservation objective.

4. Discussion

The Steller sea lion is a particularly challenging example for testing methods to assess conservation status. However, the present analysis has demonstrated using a broad range of different scenarios and reflecting different combinations of assumptions, including reasonable estimates of uncertainty in data, that it is possible to derive a coherent assessment that may release managers from the yoke of over-precaution (Gillespie, 2007).

4.1. Observation process

Count data contained errors only some of which have been taken into account in past analyses of population trends (e.g. NMFS, 2008; Fritz et al., 2008; Olesiuk et al., 2008). Past analyses have either accepted count data without error or with generalised error terms used when fitting models (e.g. Winship and Trites, 2006; Wolf and Mangel, 2008). The error defined here was approximately log-normal (Fig. 2) but the mean and variance of this distribution will vary with the counting methods used and has changed through time. Overall, the present analysis suggests that past approaches to understanding the population dynamics of the Steller sea lion have generally underestimated the uncertainties within the raw population data.

There has been an accumulation in the number of sites surveyed through time. We cannot be sure what proportion of this accumulation reflects the dispersal process as sea lions move to occupy new rookeries and haulouts and what proportion represents increasing diligence on the part of those conducting surveys. However, it is unlikely that the relative number of sites surveyed between the start and end of the time series (Fig. 3) is directly proportional to the observation effort.

4.2. Data considerations

Time-series count data from marine mammals are frequently complex and difficult to interpret mainly because it is often difficult to carry out truly synoptic surveys of widely spread marine mammal populations, not all animals are available to be surveyed, the relationship between the observed number and the true number is often obscure and difficult to assess, and there are often missing values caused by stochastic factors such as weather, funding, equipment failure, platform availability and, in the case of the Steller sea lion, withdrawal of permits to conduct surveys. More often than not, those involved in surveys have to make a leap of faith that the numbers they count are at least internally consistent because quantitative assessment of all the errors that could occur is extremely difficult. However, the type of post hoc assessment of the scale of error applied here can provide additional information that is relevant to management.

The approach adopted in the present study using sites as the unit of sampling overcomes having to manage incomplete surveys or surveys that have a regional focus that shifted between years, often by excluding the data from the assessment of the performance of conservation measures (e.g. NMFS, 2008). However, even in the present example the data set did not include sites from the whole population: data from Washington, Oregon or those from Russia were not included within the NMFS data base. It would be simple to include these in future if data were made available at the level of individual sites. Lack of these data is unlikely to have seriously affected the outcome of this analysis because these regions make a comparatively small contribution to the overall population (Pitcher et al., 2007; NMFS, 2008). Future synoptic analyses of the Steller sea lion population would be simplified if all data holders could contribute to a common data base.

The analysis also allowed the data to dictate the cluster structure rather than geography, genetics or past perceptions of population sub-divisions. A number of analyses have sought to sub-divide the population demographically, geographical, politically and using genetics. O'Corry-Crowe et al., (2006) showed high levels of mitochondrial DNA diversity within Steller sea lions but, because of the recent trajectory of the population, it is not possible to know whether the distribution of haplotypes in the population is a cause or a consequence of these demographic changes. Consequently, the present study has not made any underlying assumptions about the structure of the population. However, because of the historical management approach taken to Steller sea lions, the effects of considering the eastern and western segments separately has been investigated, but there seems to be no *a priori* reason, other than historical precedence, to view these population segments as distinct.

4.3. Historical trends in abundance

The data presented in Fig. 3 need to be interpreted with care in terms of fine-scale changes shown. Even the estimates adjusted for error cannot account for the relatively low observer effort in the early part of the time series. It is likely that information from before 1980 contains an underestimate of the total population, although we do not know this for certain. The results of the present assessment of the Steller sea lion population trends before 1980 are in broad agreement with earlier estimates (Loughlin et al., 1984, 1992; Trites and Larkin, 1996). The present analysis indicates that Steller sea lion numbers may have increased through the 1960s and 1970s. It appears that some status reviews of Steller sea lions (e.g. NMFS, 2008) and some PVAs (e.g. York et al., 1996; Gerber and VanBlaricom, 2001; NMFS, 2008) reflect an assumption that the counts of the population before 1980 greatly underestimated the overall population and these PVAs appear to have used the counts after 1980 as a benchmark for the pre-1980 estimates of Steller sea lion numbers, even though there is little evidence for this within the available data. After 1980, the non-pup population declined consistently for 10 years from a high of ~312,000 in 1980 to a low of ~135,000 in 1990, an annual rate of decline of 8%. There is no evidence of a significant decline or increase in the overall non-pup population since this time. However, local or regional dynamics may differ (e.g. York, 1994; Trites and Larkin, 1996) meaning that it is difficult to draw conclusions about population status based upon regionally-based surveys or assessments focussed upon sub-regions.

There are potentially important implications of these historical data for setting management objectives. It appears that the population size in 1980 could have been a peak and could have been greater than the long-term carrying capacity. Consequently, the return of the population since 1990 to a level similar to that measured in the 1960s may represent a level that is closer to the long-term mean carrying capacity. This suggests that management objectives could be most usefully focused upon maintaining the current population level rather than attempting to "recover" the population towards an historic high (NMFS, 2008), unless it can be shown that the environment has returned to the conditions of the 1970s (see Trites et al., 2006).

Overall, the population declined in the west and increased in the eastern part of its range (Fig. 3). While data from genetics, mark-recapture, and satellite tagging (Raum-Suryan et al., 2002;

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O'Corry-Crowe et al., 2006; NMFS, 2008; Lander et al., 2009) do not suggest that there are regular movements of individuals from west to east, it remains likely that these opposing trends reflect a re-distribution of favoured habitat and a shift in the distribution of food. The pup ratios (Fig. 3) may indicate changing population productivity but they could also indicate movement of non-pups, most likely juveniles. It may be significant that the pup ratios in the eastern segment of the population declined at the same time as they increased in the western segment and at a time when the absolute size of the non-pup population in the eastern segment of the population began to increase (Fig. 3h compared with Fig. 3i and f). Rather than interpreting these as changes caused by internal dynamics within the eastern and western segments of the population, these could indicate recent emigration of juveniles from the western segment with subsequent recruitment into the eastern segment. This hypothesis could be tested by reassessing the genetics data against the hypothesis generated by the present study, although since few samples are likely to be available that pre-date the changes in the 1980s even this may be inconclusive. The outcome has implications for the approaches to management of the Steller sea lion as two separate stocks because of the effects that dispersal could have on meeting management objectives (Taylor, 1997).

Some features of the population data may reflect the potential causes of the decline in the Steller sea lion population during the 1980s. The lag of 5 years between the start of the decline in pups compared to adults and the increase in the pup ratio through the same period supports the hypothesis (York, 1994; Holmes and York, 2003) that the decline through the 1980s was largely the consequence of low juvenile survival, but it could also have been caused by the emigration of juveniles (see above). This trend appears to have been driven by changes in the west of the range. In other words, the time lag in the decline in pup production was probably a consequence of the loss of females recruiting to the adult population and the increasing pup ratio in the early 1980s was probably because adults remained relatively unaffected. However, the ratio of pups in the population then declined and this suggests that adult fecundity declined through the 1990s, possibly as a delayed effect of the processes going on in the 1980s. Nevertheless, the subsequent increase in the pup ratio, with the apparent increase in the non-pup population, suggests that fecundity is once again recovering. This conclusion contrasts with that of Holmes et al., (2007) who suggest that fecundity is lower than expected and probably warrants further investigation. Differences may be caused by the smaller spatial scales used by Holmes et al., (2007) and we know that the population dynamics can be volatile at these scales (York, 1994; Trites and Larkin, 1996).

4.4. Scenarios for future trends

Simulations of future trends in abundance showed that if one assumes that there is a stationary distribution of λ across the full range of the data available, the probability of the population as a whole meeting the conservation objectives (probability of extinction <0.01 in 100 years) was met under all circumstances (Figs. 7 and 8). The density-limited cases of both Models 1 and 2 constrained the growth of the population within reasonable boundaries and, even in these circumstances, the population met the conservation objectives. Only when distributions of λ that excluded observations for the past 10 years were used were the conservation objective not met. Model 2 included a more complex but potentially more realistic representation of the population processes. Inclusion of information about metapopulation structure, as opposed to considering all sites as independent populations, tended to increase the vulnerability of the population to extinction. This result is similar to that found by Melbourne and Hastings (2008). Apparent vulnerability to extinction will increase further if the Eastern and Western segments of the population are considered to be internally freely mixing but separate. This has often been assumed within past studies that have modelled the extinction probability of the Steller sea lion in terms of eastern and Western Distinct Population Segments (e.g. Gerber and VanBlaricom, 2001; NMFS, 2008), even though genetic studies have tended to indicate that these sub-sections of the population are not freely mixing (Bickham et al., 1998; O'Corry-Crowe et al., 2006; Hoffman et al., 2006) and, as indicated by the present study, they may not be separate. Consequently, making the assumption that the eastern and western segments are freely mixing sub-populations is likely to result in an unrealistically inflated estimate of the extinction probability calling into question the results of some past risk assessments of the Steller sea lion population.

Although there was some level of density dependence included in the model, because the mean value of λ declined as population size increased, the only explicit form of density regulation in these models was the upper limit set on the number of sea lions that could be present on each site. This was introduced as an additional stochastic variable so the predictions will have accounted for uncertainty in the effects of density. There is no information from Steller sea lions about how density-dependent regulation actually operates but the overall effect of limiting the total number of sea lions within a site is likely to be a reasonable surrogate.

There are reasons to believe that the distribution of λ was not stationary. Inspection of the population trends in Fig. 3 suggest that a different set of conditions applied to the population before and after 1980 and before and after 1990. The threshold at 1990 is further supported by the introduction of extensive conservation measures at that time, and further measures were introduced after 2000 (NMFS, 2008). Consequently, if it is assumed that management approaches similar to the current conservation measures are maintained, then repeating the population projections using the distribution of λ from after 1990 and after 2000 are likely to provide a more realistic prediction of future risks. This demonstrated the Steller sea lion population as a whole, or if considered in the two segments, met the conservation objectives (Figs. 7b, d, f, 8b, d and f).

4.5. Implications for conservation actions

The conclusions of previous assessments of the risks to the Steller sea lion population were based upon the data that were available at the time they were developed (e.g. York et al., 1996). Although none of the previous studies has examined the population as a whole (preferring instead to consider it as two distinct populations or even sub-sections of these populations), the results of the present study suggest that both the eastern and western segments of the population have probabilities of persistence that mean they do not meet the criteria for classification as endangered and it would be reasonable to de-list them. They also suggest that conservation actions undertaken to date have either been successful or neutral in their effect. Even if one takes the most precautionary approach by assuming that management actions have had no positive effect the best risk models to apply would be Models 1a and 2a that have density limits in place. Both of these demonstrate compliance with conservation objectives.

5. General conclusions

Two general messages for conservation can be derived from the specific example of the Steller sea lion. First, the present study has demonstrated that the success of management can be assessed by updating analyses of risk with new data within a stochastic framework, using scenarios built upon the empirical distributions of directly measured population indicators. This contrasts with approaches using complex process models of the population or by the making assessments based only upon the latest information, which has often been the case for the Steller sea lion. The integration of historical information into a framework that makes few unsupported assumptions and that examines any assumptions through the stochastic structure of the approach and by the exploration of scenarios is open to modification and update in a manner that can engage with stakeholders and address, and test, their concerns. Second, considering that there will be cost-benefit trade-offs associated with conservation measures, the conservation benefits accruing from additional management actions could be negligible. For example, in the case of the Steller sea lion continuation of current management would appear to be justified but the addition of new management bringing additional social, economic or implementation costs would appear not to be justified. Without ongoing assessment of the likelihood of management meeting conservation objectives – which has not happened with the Steller sea lion as new data have been obtained – it is difficult to make an appropriate assessment of the cost-benefit trade-offs of future management policy. Lack of this type of assessment means that uncertainty in decision-making is not reduced as much as it could be and this encourages unnecessary conflict amongst stakeholders about future policy and it does not "tame" the problem. Consequently, the approach examined in the present study has the potential to both reduce conflict amongst stakeholders and to provide a mechanism for exploring future policy options based upon experience.

Acknowledgements

I wish to thank staff at the US National Marine Mammal Laboratory, Seattle for their assistance and discussions concerning the dynamics of Steller sea lion populations. In particular, I thank Lowell Fritz, Tom Loughlin, Ann York for discussions over many years. I am also grateful to Andrew Trites from the University of British Columbia for similar discussions and to Peter Olesiuk of the Department of Fisheries and Oceans, Canada for sending data. This study has been funded by many sources over the years including assistance from the US National Marine Fisheries Service, the Marine Conservation Alliance and the UK Natural Environment Research Council.

Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at doi:10.1016/j.biocon.2010.04.006.

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Please cite this article in press as: Boyd, I.L. Assessing the effectiveness of conservation measures: Resolving the "wicked" problem of the Steller sea lion. Biol. Conserv. (2010), doi:10.1016/j.biocon.2010.04.006

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July 18, 2007

Ms. Pat Livingston
Chair
Scientific and Statistical Committee
North Pacific Fishery Management Council
605 West 4th Avenue, Suite 306
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<u>Draft Revised Steller Sea Lion Recovery Plan</u>

Dear Ms. Livingston:

The Marine Conservation Alliance ("MCA") submits the following comments on the Draft Revised Steller Sea Lion Recovery Plan (the "Draft Revised Plan" or the "DRSSLRP"), as made available for public comment on May 21, 2007 (72 *Fed. Reg.* 28473).

MCA is a broad-based coalition of Alaska coastal communities, fixed and mobile gear fishermen, vessel owners, processors, support industries, Western Alaska native villages and related Community Development Quota organizations, fishing organizations, consumers, and others who are directly or indirectly involved in various aspects of the fisheries off the coast of Alaska, including efforts to protect the Steller sea lion ("SSL"). Previously, on August 10, 2006, MCA submitted extensive comments to the North Pacific Fishery Management Council (the "Council") regarding the May 2006 draft of the SSLRP. MCA believes that its comments of last summer remain valid, and it is disappointed with the failure of the National Marine Fisheries Service ("NMFS") to adopt many of the changes MCA recommended. It does not intend, however, to repeat all its prior criticisms to the Scientific and Statistical Committee (the "SSC"). Nor does it intend to rehearse in this letter all the deficiencies it believes are found in the Draft Revised Plan. MCA intends to submit more comprehensive comments to NMFS by the August 20 deadline.

Overall, the Draft Revised Plan fails to analyze well the relative importance, going forward, of the historical causes of the SSL decline. That failure makes it extremely difficult to focus on the current and prospective conditions that are likely most relevant to the survival and recovery of the species. The primary objective of a recovery plan must be to provide a "basic road map to recovery," *Fund for Animals v. Babbitt*, 903 F. Supp. 96, 103 (D.D.C. 1995), based on an analysis of the reasons for the current plight of endangered or threatened species, with an analysis of the relevant importance of all possible threats, not just a focus on possible threats subject to management. It is within this context that MCA wishes to focus upon four matters which are of particular relevance and importance to the SSC: (1) the failure of the Draft Revised Plan to assess threats to SSL populations in an unbiased, scientifically sound fashion; (2) the elaboration in the Draft Revised Plan of recovery criteria that lack scientific justification and virtually guarantee that down-listing and de-listing will not be achievable; (3) the Draft Revised Plan's call for rigid maintenance of current fishery conservation and management measures as a required recovery action; and (4) the continued specification of an adaptive management program as a required recovery action, even though such a program is likely infeasible.[1]

1. The Revised Draft Plan Fails to Assess Threats to SSL Populations in an Unbiased, Scientifically Sound Fashion.

Under the Endangered Species Act, 16 U.S.C. § 1531, *et seq*. (the "ESA"), NMFS is required to develop and implement a "recovery plan" for each listed species under its jurisdiction, unless it "finds that such a plan will not promote the conservation of the species." ESA, sec. 4(f)(1). NMFS' Recovery Planning Guidelines, dated October, 1992, specify that a central element of any recovery plan must be a discussion of "factors affecting the species." It is elementary that the science that undergirds this discussion must be objective, sound and free of bias, basic criteria that NMFS fails to meet in the Draft Revised Plan.

NMFS' obligations with respect to scientific analysis in its resource management documents, such as the Draft Revised Plan, are spelled out in detail in the Data Quality Act, Treasury and General Government Appropriations Act for Fiscal Year 2001, Pub. L. No. 106-554, § 515, Appendix C, 114 Stat. 2763A-153 (2000) (the "DQA"), the implementing Guidelines of the Office of Management and Budget, 67 *Fed. Reg*. 8452 (Feb. 22, 2002) (the "OMB IQ Guidelines") and the Information Quality Guidelines of National Oceanic and Atmospheric Administration, dated November 6, 2006, available at

[1] In its comments last summer, MCA also stressed the need for the recovery plan to integrate metapopulation considerations into its analysis and questioned the inclusion of the Population Viability Analysis (the "PVA") as an appendix. Without wishing to belabor the point, MCA would note that metapopulation considerations continue to be relevant, especially since it is increasingly apparent that SSLs seem to be migrating eastward, with populations shifting from the Western to the Eastern region. MCA is disappointed that NMFS has not followed the SSC's recommendation last summer that there be a "more thorough evaluation" of SSL population dynamics, including "if it would be more realistic to describe the SSL as a metapopulation." Report of the SSC to the Council, August 15-16, 2006, p. 4 (the "SSC Report"). As far as the PVA is concerned, MCA notes that the SSC identified a number of weaknesses and desirable improvements that should be made to future iterations of the PVA. Even though nearly a year has passed, many of these have not been addressed, including comparing the results of this PVA with the results of other models. MCA appreciates that NMFS now takes the position that the PVA "does not guide management so much as it guided the [Plan Recovery] Team in their weight of evidence approach to deriving recovery criteria;" that the PVA is "only a tool not a deciding factor;" and that the PVA's "results were not used as recovery criteria." *See* NMFS, "Response to Comments on Draft Steller Sea Lion (SSL) Recovery Plan", pp. 18, 19, 20 (the "Response to Comments"). If this is the case, and given that there has been no further refinement of the PVA, one can question even more why this version of the PVA continues to be attached as an appendix to the DRSSLRP.

www.cio.noaa.gov/itmanagement/IQ_Guidelines_110606.htm (the "NOAA IQ Guidelines"). The DQA requires that Federal agencies have in place guidelines that ensure the "quality, utility, objectivity and integrity" of the information they disseminate. DQA, § 515b.2.A. The OMB IQ Guidelines stress, in particular, that "objectivity" relates to both presentation and substance. In terms of presentation, it "includes whether disseminated information is being presented in a clear, accurate, complete and unbiased manner" and, in terms of substance, it involves a "focus on ensuring accurate, reliable and unbiased information." OMB IQ Guidelines, Sec. V.3, 67 *Fed. Reg*. at 8459. The OMB IQ Guidelines apply strict standards to the dissemination of information that is considered "influential," that is, information which "will have or does have a clear and substantial impact on important public policies or important private sector decisions." OMB IQ Guidelines, Sec. V.9, 67 *Fed. Reg*. at 8460. Such information must be presented to ensure a high degree of transparency about the data and methods to facilitate its reproducibility by third parties. OMB IQ Guidelines, Sec. V.3.b.ii, 67 *Fed. Reg*. at 8460.

The NOAA IQ Guidelines, for their part, similarly define "objectivity" in terms of both presentation and substance, tracking the language of the OMB IQ Guidelines. They define "influential scientific information" as "scientific information the agency reasonably can determine will have or does have a clear and substantial impact on important public policies or private sector decisions." They expressly cover "natural resource plans," such as the Draft Revised Plan, within this category. In accordance with the OMB IQ Guidelines, for influential information that assesses risks to the environment, such as the DRSSLRP, the NOAA IQ Guidelines call for the use of "(a) the best available scientific and supporting studies (including peer-reviewed science and supporting studies when available) conducted in accordance with sound and objective scientific practices, and (b) data collected by accepted methods or best available methods." For risk assessments that are quantitative in nature, "to the extent practicable," agency documents must discuss:

- each ecosystem component, including population, addressed by any estimate of applicable risk effects;

- the expected or central estimate of risk for the specific ecosystem component, including population, affected;

- each appropriate upper-bound and/or lower-bound estimate of risk;

- data gaps and other significant uncertainties identified in the process of the risk assessment and the studies that would assist in reducing the uncertainties; and

- additional studies known to the agency and not used in the risk estimate that support or fail to support the findings of the assessment and the rationale of why they were not used.

With respect to natural resource plans, the NOAA IQ Guidelines stress that such plans "will be based on the best information available," and "will be presented in an accurate, clear, complete and unbiased manner." In particular, under the Guidelines, "Clear distinctions will be drawn between policy choices and the supporting science upon which they are based." Supporting materials must be properly referenced to ensure "transparency."

The DRSSLRP does not measure up to these standards. This is reflected particularly in its discussion of killer whale predation and nutritional stress as factors affecting SSL populations -- a discussion which still lacks the "consistency" which the SSC last summer urged NMFS to strive to attain. *See* SSC Report, p. 5.

One of the major changes in the Draft Revised Plan from the Plan released in May 2006 is the downgrading of the threat assessment for killer whale predation from "potentially high" to "medium." DRSSLRP, p. 114. The Plan states that "[m]ajor limitations in the available data result in substantial uncertainty," and, while the Recovery Team was unable to reach consensus, NMFS changed the ranking based upon "public review and comment and additional scientific data which was not available to the Team." *Id*. If the NMFS downgrading of the killer whale threat was based on the Maniscalco paper cited on p. 111 (but not listed in the literature list), the data in the paper was presumably available prior to the preparation of the May 2006 draft since one of its authors was on the Recovery Team. Yet NMFS ranked the killer whale threat as "high" in that earlier draft. The difficulties with the Draft Revised Plan's analysis of this factor are highlighted in the comments of Dr. Ian Boyd, dated July 14, 2007 (the "Boyd Review"). Dr. Boyd points out that the Draft Revised Plan, without substantial justification, relies upon an unreferenced paper by Maniscalco, *et al.*,[2] to dismiss estimates of killer whale predation in an earlier paper, Williams, *et al*., "Killer appetites: assessing the role of predators in ecological communities," *Ecology* 85(12): 3373-3384 (2004). Boyd Review, p. 15. Dr. Boyd also observes that the Draft Revised Plan (at p. 88) appears to understate the impacts of killer whales discussed in the Williams paper, by stating that a population of 170 transient killer whales could account for the decline of the western SSL distinct population segment ("DPS"), whereas, in fact, Williams suggests that fewer than 27 male transient killer whales or 40 female transient killer whales could have caused the decline. *Id*. If anything, the Williams findings would appear to be even more powerful today, since the latest transient killer whale population numbers show a population of about 314 animals. Draft Revised Plan, p. 84. This would appear to indicate that minimally only about 13% of the current transient killer whale population would be sufficient to explain the original decline -- almost double the size of the threat originally ranked as "potentially high."[3] In short, the Draft Revised Plan's discussion of killer whale predation does not appear to reflect an objective, unbiased discussion of this factor. Rather, as Dr. Boyd states, "NMFS appears to have gone out its way to counter the killer whale argument put up by Williams," Boyd Review, p. 16, and NMFS' treatment of this factor stands in stark contrast to its

[2] The paper is presumably "Assessing Killer Whale Predation on Steller Sea Lions from Field Observations in Kenai Fjords, Alaska," *Marine Mammal Science* 23(2): 306-321 (April 2007). Interestingly, this report strongly cautions against using its results to make comparisons to other times, areas or populations of transient killer whales and their effects on prey. The report specifically notes that it would not be appropriate to extrapolate the effect of this group of transient killer whales on Stellar sea lions to other regions of the state. Despite this caution, it appears that NMFS did indeed make such extrapolations.

[3] MCA understands that Dr. Williams has expressed similar concerns regarding the treatment of the predation data in the revised draft.

treatment of another factor, "nutritional stress," where, in Dr. Boyd's view, the evidence "is probably weaker than the balance of evidence supporting killer whale predation effects and yet the RRP comes to quite different conclusions about them as threats." *Id*. All this suggests a "worrying lack of objectivity," Boyd Review, p. 2, and "the possibility that NMFS is weighting the assessment to support preconceived notions of the underlying mechanisms [of decline]." *Id*., p. 2. Plainly, this is contrary to NMFS' obligations under the DQA and its implementing guidelines.

The deficiencies in the Draft Revised Plan become especially apparent in its discussion of nutritional stress. *See* Draft SSLRP, pp. 36-42, 100-106. The DRSSLRP links nutritional stress with "competition with fisheries." Even while acknowledging that the Recovery Team could not reach "consensus" on this factor and that it is subject to "high uncertainty," NMFS continues to rank competition with fisheries as posing a "potentially high" threat to SSLs. DRSSLRP, pp. 102-104, 112-114. Again, MCA believes that Dr. Boyd's critique is on point. Dr. Boyd underscores that, "without exception, no study has found support for this hypothesis." Boyd Review, p. 9. As Dr. Boyd states, "[T]he extended discussion [of nutritional stress] simply deepens the doubts that exist about the nutritional stress hypothesis." Boyd Review, p. 13. Dr. Boyd points out that, based upon very limited samples from the 1970s and 1980s, "[t]he new section of the RRP on nutritional stress spins a complex story around nutritional stress involving backdated growth through the lifetime of these animals to critical periods in life-histories of these animals. I simply cannot accept that this is justified. We have no life-history for these individuals and we have no data about the levels of food supply through these periods." Boyd Review, p. 14. Dr. Boyd goes on to observe, "[H]aving admitted that most retrospective analyses have been of little help, many of these analyses are then used in later parts of the RRP to justify a particular position especially about the effects of nutritional stress and also when assessing the levels of threat." *Id*. The conclusion is inescapable that the Draft Revised Plan "says more about current internal agendas in NMFS than about what we actually know about the influence of nutritional stress on Steller sea lions." *Id*., p. 15.

In its comments to the Council last summer, MCA underscored the difficulties with the nutritional stress hypothesis. MCA continues to question this hypothesis, particularly insofar as it may be deemed to implicate fisheries as a factor affecting recovery. Notwithstanding NMFS' strenuous efforts to assert the validity of the hypothesis, it remains dubious at best.[4] As Dr. Boyd pointed out in his June 7, 2006 testimony to the Council, "[T]here's really very little evidence to support the idea that there's been nutritional stress in this population as a causal factor in the population dynamics." Dr. Boyd went on to underscore that, even if nutritional stress occurred in the past -- a proposition that, even if dubious, cannot be entirely ruled out -- "the point is it's probably not happening now, and it's from now on that we need to manage the population." In short, nutritional stress, even if it could hypothetically be related to competition

[4] Dr. Boyd notes NMFS' reliance on the study by Hennen, "Associations between the Alaska Steller sea lion decline and commercial fisheries," *Ecological Applications* 16(2): 704-717 (2006), to establish the link between fishing activity and the "local population trajectory," but cautions, "[T]his study is flawed in the sense that it was an exploration of data to examine the possibility of a correlation between fishery activity and SSL population dynamics. In other words, it was not a fair test . . . [and] must carry relatively little weight in the assessment of evidence." Boyd Review, p. 10.

from the fisheries, is not a basis for targeting the fisheries as a factor significantly affecting the species from this point forward.

This leads to MCA's final point. The Draft Revised Plan indicates that the causes of recent declines in the Central Gulf of Alaska may be a low birth rate, relying on Holmes, *et al*., "Natality declines in Steller sea lions suggest new conservation and research priorities," *in review*. Draft Revised Plan, pp. 39, 106. NMFS suggests (though it does not definitively state) that the low birth rate is associated with nutritional stress. Draft Revised Plan, pp. 39, 42. In Dr. Boyd's words, however, "At best, [the new studies] can be viewed as fairly circumstantial evidence supporting low birth rate but they say absolutely nothing about the causes of the low birth rates. They provide no evidence for a nutritional cause of low birth rates." Boyd Review, p. 14. Low birth rates might, for example, be the result of infection or chemical toxicity, and the paper in fact discusses a range of different, potential causative factors. *Id*., pp. 4, 10, 18. Further, even if nutritional stress were a causative factor in low birth rates, the Draft Revised Plan suggests that such stress would have been chronic rather than acute, Draft SSLRP, p. 37, and the most likely explanation for any such chronic stress may be reduced carrying capacity in the North Pacific ecosystem rather than impacts from commercial fisheries.

Ultimately, what we know today is that the population of the western SSL DPS (WDPS) as a whole is increasing at the rate of about 3% per year since about 2000 and that the current population is about 44,800 animals, up 33% from the population low of 33,600 in 1994. Draft Revised Plan, pp. 1, 13-16.[5] As Dr. Boyd points out, this population size "lies well within the normal range of population sizes for pinnipeds on a global scale." Boyd Review, p. 7. At the same time, there is no current evidence of nutritional stress to adult male SSLs and juveniles. Indeed, we know that the health, survivability and longevity of juveniles and adults are unimpaired by nutritional stress or any other, identified factor. In such circumstances, the absence of evidence of nutritional stress to the western DPS as a whole suggests that one should be cautious in attributing the cause of a low birth rate to this factor. While it may be that the female reproductive rate is what is holding back the population, the causal factors simply remain unknown. In the face of such uncertainty, MCA fails to understand how NMFS can conclude that the fisheries threat is "potentially high," while it discounts such a factor as killer whale predation, which is subject to similar uncertainties. What is needed, as Dr. Boyd states, is for NMFS to "grasp and articulate, in an easily digestible form, the complexity of the knowledge base and to communicate this in a manner that is useful for policy implementation." Boyd Review, p. 3. Only in this way can NMFS meet its obligations to provide objective, unbiased information to the public in its natural resources plans, as required by the NOAA IQ Guidelines.

2. The Draft Revised Plan's Recovery Criteria Lack Scientific Justification.

MCA understands that NMFS is under an obligation to develop recovery plans which set out "objective" and "measurable" criteria for recovery. ESA, Sec. 4(f)(1)(B). The criteria outlined in the Draft Revised Plan, however, lack scientific justification, and they are written in such a way that they will make it difficult, if not impossible, for the Plan to achieve its objectives.

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[5] The number rises to 60,000, if Russian populations, which NMFS deems relevant to recovery, are included.

MCA is pleased that the Draft Revised Plan no longer requires that "vital rates" be consistent with the observed trend of population growth for down-listing or de-listing to be warranted. *See* Response to Comments, p. 28. As expressed in its comments last summer, because of the uncertainties associated with measuring vital rates, using vital rates as a criterion for down-listing and de-listing was simply infeasible. Still, in several other respects -- the requirements that "non-pup trends in at least 5 of the 7 sub-regions are consistent with the [overall U.S.] trend," Draft Revised Plan, pp. 3, 4, that "the population trend in any two adjacent sub-regions cannot be declining significantly" (which implicates management of SSLs in Russia outside of U.S. jurisdiction), Draft Revised Plan, pp. 3, 4, and the requirement for de-listing that "the population trend in any single sub-region cannot have declined by more than 50 percent," Draft Revised Plan, p. 4 -- the Draft Revised Plan remains problematic and does not meet the SSC's recommendations of last summer that the recovery criteria should be grounded in "sound science." *See* SSC Report, p. 4.

For both down-listing and de-listing, the Draft Revised Plan would not only specify that "non-pup trends in at least 5 of the 7 sub-regions are consistent with the [overall U.S.] trend" -- a trend that shows a "statistically significant" increase over fifteen years and an average growth rate of 3% per year over thirty years -- but also that "the population in any two adjacent sub-regions cannot be declining significantly." Draft Revised Plan, pp. 3, 4; 72 *Fed. Reg*. at 28474. In Dr. Boyd's view, the criteria are "overly precautionary," since, "if the population remained stable at current numbers for the next 15 years, the PVAs as applied in the RRP would almost certainly show an extremely low probability of extinction and would, in effect, take the population well above the ESA criteria." Boyd Review, p. 7.[6]

MCA previously pointed out that there are at least three problems with NMFS' approach. It ignores that the distribution of SSLs may be shifting across its range; it assumes congruence between the current definitions of sub-populations and actual, biological sub-populations; and it assumes that some factors, *e.g.*, fisheries competition, were more likely drivers of past population declines than others. It is insufficient in response merely to state, as NMFS does in its "Peer Review Comments on Steller Recovery Plan (the "Peer Review Comments") (at p. 2) that "if this situation occurs, it would indicate that a significant portion of its range . . . was still in decline and suggest that NMFS has not fully understood or mitigated the threats to the population." The point is that declines in a particular region may have nothing to do with the overall health of the population.

The requirement that two adjacent sub-regions can't both be declining significantly also implicates the problem of management of SSL populations found in Russia. The DRSSLRP specifically references "Russia/Asia" as one of the seven regions it covers. DRSSLRP, pp. 3, 4.

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[6] "Recovery" is defined in NMFS' regulations to mean "improvement in the status of listed species to the point at which listing is no longer appropriate under the criteria set out in Section 4(a)(1) of the [Endangered Species] Act." 50 C.F.R. § 402.02. In short, achieving the goal of recovery means just reaching the point where the species is no longer "in danger of extinction throughout all or a significant portion of its range" under current or reasonably foreseeable conditions. *See* ESA, secs. 3(6), (9) (defining the terms "endangered" and "threatened").

Although the ESA requires NMFS to consider a species' prospects for extinction "throughout its range," and while the status of a species' population outside U.S. jurisdiction, including in foreign nations, as well as foreign nations' conservation efforts, are appropriately taken into account in ESA listing and de-listing decisions, [7] nonetheless it is not sensible to peg recovery criteria so closely to the response of the species to a management regime over which the United States has no control. It might be that recovery efforts in Russia are insufficient, and so declines within Russian jurisdiction might continue. Yet, if they are offset by continued, positive growth in areas further to the east, then such declines may not be valid indicators that the population as a whole has not recovered to the point that down-listing is warranted. NMFS, *Interim Endangered and Threatened Species Recovery Planning Guidance* sec. 2.2.3 (October 2004), available at www.nmfs.noaa.gov/pr/recovery/guidance.htm, in fact suggests that NMFS has some discretion with respect to the inclusion of the recovery of foreign populations as part of the de-listing criteria in a recovery plan, depending upon the relationship between the status/protections of animals outside U.S. jurisdiction and the achievement of the goals of the recovery plan.

In addition to other factors being met, the Draft Revised Plan would require for de-listing that "the population trend in any single sub-region cannot have declined by more than 50 percent." Draft Revised Plan, p. 4; 72 *Fed. Reg*. at 48474. This criterion implicates some of the same concerns as the criterion requiring that trends be consistent in at least five of seven sub-regions, particularly that (a) there may be natural population shifts, not reflecting a species decline, that lead to a substantially decreased population in some regions, and (b) a major decline in an area outside U.S. control, that is, Russia, may not reflect on the success, for the species as a whole, of the recovery efforts within U.S. jurisdiction. Further, it is unclear what the starting date is for measuring the percentage decline.

In short, these criteria seem to be purely arbitrary with little to do with the health of the population as a whole and are essentially without biological basis. For example, the population in one or two sub-regions could grow such that the WDPS population increases to 60- 70 thousand animals in the U.S. but if one U.S. sub-region coupled with the Russian population decrease because of outmigration, then there is no possibility for downlisting to threatened. Moreover, the population could increase to more than 100,000 animals overall, but if the trend in two sub-regions is a "significant" decline then the population would not meet the delisting criteria.

Finally, the recovery criteria also appear overly stringent in comparison with criteria used in other recovery plans. In a review prepared for the Council, Dr. Thomas Loughlin compared

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[7] The ESA states that all listing decisions shall be made "after taking into account those efforts, if any, being made by any State or foreign nation, or any political subdivision of a State or foreign nation, to protect such species, whether by predator control, protection of habitat and food supply, or other conservation practices, within any area under its jurisdiction, or on the high seas." ESA, sec. 4(b)(1)(A), 16 U.S.C. § 1533(b)(1)(A). NMFS' regulations, for their part, recognize the role of foreign governments in the listing and delisting process. Thus, 50 C.F.R. § 424.11(e) provides that NMFS "shall give consideration to any species protected . . . by any State or foreign nation, to determine whether the species is endangered or threatened," while 50 C.F.R. § 424.11(f) provides that, in making listing and delisting determinations, NMFS "shall take into account . . . those efforts, if any, being made by any State or foreign nation, or any political subdivision of a State or foreign nation, to protect such species."

the Draft Revised Plan with recovery plans for eleven other species. Loughlin, "Review and Comparison of Recovery Criteria in the 2006 Draft Revised Steller Sea Lion Recovery Plan" (May 14, 2007). Although the Loughlin paper in general suggests that the Draft Revised Plan's specification of recovery criteria is consistent with what NMFS and the U.S. Fish and Wildlife Service ("FWS") have done in other plans, nonetheless SSLs have a larger population, which has been stable or growing, rather than declining, than the populations of many of the other endangered or threatened species in the reviewed recovery plans. In these circumstances, regardless of what has been done in other plans, requiring three generations to achieve full recovery, *i.e.*, de-listing, can be viewed requiring more than is justified by reference to the past practice of NMFS and FWS.

3. The Draft Revised Plan does not Sufficiently Recognize the Need for Flexibility to Modify Fishery Conservation and Management Measures.

Section 4(f) of the ESA was not intended to hamstring agencies with a suite of inflexible actions that would have to be taken before a species could be removed from the list. *See, e.g.,* Bean and Rowland, *The Evolution of National Wildlife Law* 211 (3d ed. 1997). As science improves regarding the causes of the SSL decline and the constraints on the species' recovery, management agencies, such as the Council and NMFS, should be able to modify and/or remove particular fishery management and conservation measures, to the extent that they may not be relevant to achieving the objectives of the Draft Revised Plan.

MCA recognizes that the Draft Revised Plan, by its terms, would not necessarily straightjacket the fisheries with the precise suite of management measures currently in place until the recovery criteria are met. Instead, it would allow for current management measures to be replaced with measures providing "equivalent" protection, and for the current measures to be modified if "substantive evidence demonstrates that these measures can be reduced without limiting recovery." 72 *Fed. Reg*. at 28474; DRSSLRP, p. 5. NMFS' Response to Comments also makes it clear that "[the] Council and NMFS have flexibility to modify existing management measures as new information on Steller sea lions and fishery interactions becomes available," as long as the changes are appropriately evaluated through the ESA Section 7 consultation process. Response to Comments, p. 35. Yet, if, as discussed in Section 1 above, the weight of current scientific evidence would suggest that the fisheries are likely not a significant factor limiting SSL recovery, then the rationale for strict maintenance of current measures as required for recovery of the WDPS is weak.

In any event, MCA wishes to underscore that management flexibility may be appropriate even in the short run. For example, recent science suggests that juvenile SSLs are weaned during the summer instead of during the winter. *See* Trites, *et al*., "Insights into the Timing of Weaning and the Attendance Patterns of Lactating Steller Sea Lions (*Eumetopias jubatus*) in Alaska During Winter, Spring and Summer," *Aquatic Mammals* 32(1):85-97 (2006). Winter is the most critical fishing time for the groundfish fleets because fish are aggregated and roe is an important product. Many of the mitigation measures now in place have reduced the winter fisheries in order, in theory, to protect weaning juveniles. With the new information in hand, the Council and NMFS may be able to modify those measures to enhance fishing opportunities without adverse effects on the SSL population. Similarly, current mitigation measures, which do not seem to discriminate among population segments, might be modified to give priority protection to segments of the population most important to increasing the population trend, such as breeding females, while mitigation measures that don't protect these segments might be reduced. MCA recognizes that any such changes will need to be supported by the appropriate ESA Section 7 analyses.

4. The Continued Specification of a Large-Scale Adaptive Management Program as a Needed Recovery Action is Inappropriate.

The Draft Revised Plan, rejecting previous comments from both MCA and the SSC continues to specify that designing and implementing an "adaptive management program" is one of three necessary recovery actions. Draft Revised Plan, p. 5; 72 *Fed. Reg*. at 28474. Even while acknowledging that "it will be a challenge to construct an adaptive management program that is statistically sound, meets the requirements of the ESA and can be implemented in a practical manner," *id*., NMFS remains committed to what MCA believes to be a chimerical goal. Indeed, in its Response to Comments (at pp. 15-16), ignoring specific criticisms, including those of the SSC, *see* SSC Report, p. 5,[8] NMFS does no more than assert that "development of an adaptive management program would provide another means by which the scientific and management communities can evaluate new information, determine the efficacy of current regulations, and recommend that new actions be taken or regulations be changed." *See also* Peer Review Comments, p. 5 (asserting only that "without a program of this nature, it will not be possible to distinguish the magnitude of the various threats to recovery"). The insufficiency of this response is manifest.

A "grand experiment" in adaptive management faces innumerable difficulties. Its practicality, costs and outcome are all in doubt. It may run afoul of the "jeopardy" and "no adverse modification" proscriptions of the ESA and so be infeasible as a matter of law. It may well not be able to produce, when the "experiment" is complete, any truly useful results. It is likely to be both complicated and expensive to design. It would raise internal equity questions between those in the fishery who would be able to fish under existing management measures in existing open areas and those who would be forced to move their operations, perhaps at substantial expense, or those who, for economic reasons, might be unable to transfer their operations at all. At the end of the day, there would be an upheaval in management, likely major costs imposed upon the industry, and far from certain benefits in terms of increased understanding of the potential impacts of the fishery on the recovery of SSLs. The bottom line is that, from a cost-benefit perspective, an adaptive management exercise is simply not likely to be worthwhile.

At the same time, the very need for any large-scale, adaptive management program, even if theoretically feasible and cost-effective, is open to question. As Dr. Boyd testified to the Council on June 7, 2006, "I don't think you need to do it. * * * [B]ecause of the highly variable trajectories that you have within the localized populations of the Steller sea lions, you already have enough statistical power there to come to reasonable conclusions about some of the drivers

[8] The SSC not only opposed inclusion of adaptive management as a required element of a recovery plan, but also indicated that any management experiments undertaken should be "at *small* but meaningful spatial levels." SSC Report, p. 5 (emphasis added).

for some of those changes overall." Dr. Boyd pointed out that statistical analyses have in fact already been done for SSLs, citing MRAG Americas, Inc., "Understanding the Decline of the Western Alaska Steller Sea Lion: Assessing the Evidence Concerning Multiple Hypotheses" (NOAA Fisheries, Alaska Fisheries Science Center #AB133F-02-CN-0085, 2005) (usually referred to as the "Wolf and Mangel" study). In short, whether or not the Wolf and Mangel study itself was sufficient, it appears that there may be enough data now available -- and certainly there are likely to be more data available in the future -- to allow the relative strengths of each hypothesis to be assessed through a table-top modeling exercise, essentially making any large-scale adaptive management program unnecessary.

As a final comment, MCA appreciates the time and effort the SSC has put into this issue over the past decade or so. When you reviewed the earlier draft of the recovery plan, you made a number of other recommendations (36 in total), some of which were addressed but many of which were not. MCA encourages the SSC to seek further clarification as to the issues raised in your earlier comments.

Thank you for your consideration of MCA's views. Please do not hesitate to contact me if you have any questions about this submission or any requests for further information.

Respectfully submitted,

David Benton
Executive Director

August 20, 2007

Dr. William Hogarth
Assistant Administrator for Fisheries
National Marine Fisheries Service
National Oceanic and Atmospheric Administration
U.S. Department of Commerce

Dear Dr. Hogarth:

RE: Comments from some former members of the Steller Sea Lion Recovery Team on the Revised Draft Steller Sea Lion Recovery Plan

As former members of the Steller Sea Lion Recovery Team and authors of the 2006 Draft Steller Sea Lion Recovery Plan, we recognized three leading hypotheses to explain the decline of Steller sea lions: 1) predation by killer whales, 2) a reduction in groundfish caused by fishing, and 3) an increase in groundfish and a decrease in oily fishes caused by natural changes in ocean climate (previously termed the junk-food hypothesis). As a Recovery Team we were unable to reject any of these hypotheses or discount them as *Potentially High Threats* to recovery. Rather we agreed that all three factors should be presented in order for each to be considered as a possible contributing factor to the population decline. We therefore developed a recovery plan that detailed the uncertainties associated with each of the hypotheses.

The Draft Revised Steller Sea Lion Recovery Plan is a significant departure from the original intent of the Recovery Team. The changes made by NMFS to our document leave the impression that only fishing could have caused the decline of Steller sea lions. The remaining hypotheses related to predation and changes in ocean climate (regime shifts) were summarily rejected or discredited.

Many of the alterations made by NMFS in support of their conclusions contain errors of fact and misrepresentations of scientific information and do not reflect the collective views of the Steller Sea Lion Recovery Team. Consequently, it is inappropriate to attribute this document to the Recovery Team as implied in the current plan. Instead the Revised Plan should have clearly stated the changes that NMFS made and why they were made.

The change in authorship of the Recovery Plan should have been made clear to the Independent Reviewers that were commissioned by NMFS and the North Pacific Fisheries Management Council. Many of the Reviewers concluded that the assessment of hypotheses was balanced but presumably never questioned the truthfulness of statements they thought had been prepared by well-known Steller sea lion experts that formed the Recovery Team.

Examples of the misinformation written into the Revised Plan are as follows:

Treatment of the junk-food hypothesis. NMFS added a new section to the Plan self titled *Energetic Demands: Rejection of the Junk Food Hypothesis.* In it peer-reviewed publications illustrating mechanisms by which juvenile sea lions can be negatively affected by low-energy prey are dismissed using erroneous information. NMFS reports unpublished data by Calkins and Trites which were not collected by Trites and provide insufficient information in the Revised Plan to evaluate their interpretation of the findings. NMFS further writes that "*instead of pollock being bad for sea lions (Alverson 1992), gadids are likely to have been an important component of a healthy sea lion diet for decades (Calkins et al 2005, Fritz and Hinkley 2005)*" [page 42]. This omits the fact that the junk-food hypothesis has been refined since Alverson (1992) noted that sea lions declined while consuming a greater proportion of pollock—a low energy fish. The junk-food hypothesis is now recognized to encompasses a range low energy prey species and only apply to young growing animals rather than adults. The Recovery Team recognized that nutritional stress encompasses much more than was encapsulated by the original junk-food hypothesis. The misuse and misinterpretation of nutritional research of adults versus juvenile pinnipeds and of otariids versus phocids leads NMFS to incorrect conclusions regarding this and other nutritionally-related hypotheses.

Treatment of the killer whale predation hypothesis. The revised presentation of research on predation by killer whales is biased, and contains miscalculations and false information. NMFS states that "*Williams et al. (2004) hypothesized that… 170 mammal-eating killer whales could have caused the decline*" when Williams et al. actually states that as few as 27 killer whale could have instigated the decline. NMFS [page 85] also presents new information from Maniscalco et al (in press) in an attempt to discredit the findings of Williams et al. but provides no alternative explanation for the discrepancy between the two studies. Interestingly, in providing new increased estimates for the number of transient killer whales NMFS has doubled the potential predation pressure originally calculated in Williams et al; this was not addressed in the revised Plan. NMFS further fails to consider the ecosystem modeling studies that show the vulnerability of small populations of sea lions to predation by killer whales. They also fail to consider regional specialization of killer whale diets. Overall, the decision by NMFS to down-list the threat of predation by killer whales from HIGH to MEDIUM appears to be based on erroneous facts and was not supported by the Recovery Team.

Treatment of fisheries effects on Steller sea lions. The revised Recovery Plan cites only 3 of the 8 studies that failed to find a consistent relationship between fishery catches and changes in sea lion numbers, but does not explore their findings or consider them relevant to understanding the effect of fisheries on sea lions. This contrasts sharply with the many citations to Hennen (2006) that found positive relationships between several metrics of fishing and the steep rates of population decline in the 1980s. "*This relationship vanished in the 1990s, leading to the conclusion by Hennen (2006) that measures taken in the early 1990s (e.g., trawl exclusion zones, spatial-temporal management, shooting ban, reduction in incidental catch) may have been effective in slowing the decline*." [page 29]. A balanced presentation of this information would have demonstrated that these conclusions are confounded by oceanic regime shifts concurrent with the implementation of the management actions.

The above highlights just a few of the problems with the Revised Draft Recovery Plan. It was presumptive to state that the revised plan was prepared by the Recovery Team and by implication that the Recovery Team endorsed the revised conclusions. Rather, the revisions have prompted one scientific Recovery Team member to formally request that their name be removed from the document as well as any future Steller Sea Lion Recovery Plans. Others may follow if the factual errors and misleading statements are not fixed.

The Revised Draft Recovery Plan requires significant editing and work to regain its scientific credibility. We therefore request that NMFS withdraw the current draft and that they revise it in conjunction with independent experts to accurately reflect the state of scientific knowledge and provide a clear blueprint to guide the recovery of the Steller sea lion in western Alaska.

Sincerely,

Andrew W. Trites, North Pacific Universities Marine Mammal Research Consortium

Terrie M. Williams, University of California, Santa Cruz

Kate Wynne, University of Alaska

Alan Springer, University of Alaska, Fairbanks

Lianna Jack, Alaska Sea Otter and Steller Sea Lion Commission

David Hanson, Pacific States Marine Fisheries Commission

Dave Fraser, F/V Muir Milach

Donna Parker, F/V Arctic Storm

Cc. Stephanie Madsen, Chair, North Pacific Fisheries Management Council