

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 than 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 where the 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.

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| 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 |

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| | | (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 |

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| 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 <i>per capita</i> food intake and may have no relationship at all with <i>per capita</i> 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 1 st paragraph of this section it is suggested that declining productivity is caused by chronic nutritional stress; in the 2 nd 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. |

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| 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 |

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| | | 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. |

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| 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 <i>hoc</i> . 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. |