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## Steller Sea Lion Recovery Plan

Review (version 2)

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### Basis of this review

This review was commissioned by the Marine Conservation Alliance. It has been conducted as a peer-review assessment and it is, therefore, the views developed by the author as a practicing scientist in this field and as the main individual responsible for delivering advice about the management of marine mammals to government in the United Kingdom. The author has a background in marine management issues within the Northeast Atlantic and the Southern Ocean. A brief CV of the author is given in Appendix IV to this report.

### Introduction

I propose that the approach taken in the production of the SSL RP is inappropriate and, because of this, the RP contains important logical inconsistencies and encourages polarised views of appropriate actions.

The RP should use a risk assessment framework (Appendix I) involving regular review and re-assessment of management actions in the light of assessments of their effectiveness. Many of the following comments will seem to be highly negative and some may ask what the author is suggesting as an alternative. The alternative involves the use of a risk assessment framework. Risk assessment involves a substantially greater level of managerial effort throughout implementation but it has the large advantage that it does not commit the Agency to a single course of action at the outset and it is based upon a *balance of evidence*. Perhaps

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the most frustrating aspect of the current RP is that the Agency is already using a *de facto* risk assessment approach but the RP fails to recognise this or to develop it into a formal mechanism. Much unnecessary tension could be avoided by doing so.

Readers of this document should also be aware that there is a strong philosophical divide between what is proposed in this document and what is within the RP. Implicit within the RP is the idea that a rational basis for management requires detailed knowledge and this can be achieved through high levels of investment, especially in research. My thesis here is that some of the vital information required by the RP may never be forthcoming – some of the scientific problems simply cannot be solved with our current technology and capability and they are very unlikely to be solved in the near future and certainly not within the lifetime of the current PR. The RP is knowledge-hungry to an extent that makes it unsustainable. Although admission of failure to solve some of the most pressing scientific problems may be a profoundly depressing scenario (especially for those toiling on SSL research), there are pragmatic ways of dealing with this issue.

The narrative in this document supplies the rationale for the following set of recommendations:

### **Recommendations**

1. Ideally, the RP should consider the whole of the SSL population as a single metapopulation. This would help to focus management at appropriate sub-population scales.
2. The inherent properties of PVA and the specific features of the current PVA used in the RP mean that the PVA should not be included in the RP. The PVA developed for the PR is likely to be highly misleading.
3. Revise the recovery criteria because the highly prescriptive nature of these criteria, the difficulties with obtaining adequate information to support some of these criteria and lack of biological realism in some of them mean that they are unlikely to be met.
4. The risk framework approach (Appendix I), which has been implicitly adopted by NMFS towards the management of the SSL in the past but which is not acknowledged explicitly within the RP, should be used to develop an adaptive approach to management.
5. Although current or equivalent appropriate mitigation should be maintained for the time being this should be subject to periodic review in the light of new information to ensure that it is appropriate.
6. The RP should provide a formal definition of “adaptive management” because, confusingly, several are provided throughout the RP and these are used interchangeably.
7. There should be a full and proper assessment of the effectiveness of past management actions before any further management actions should be considered. A suggestion is made about how this could be done (Appendix II)
8. Although current mitigation should be maintained for the time being, this should be subject to periodic review in the light of new information.
9. The research associated with each of the recovery actions needs to be prioritised based upon the immediacy of its relevance to the assessment of risk to the SSL, its

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likelihood of producing a successful outcome and the level of past investment and the rewards from this investment.

### **General introductory comments**

I am not completely familiar with the process that was used to generate the Plan but, while I applaud the attempt to engage the breadth of stakeholder opinion, I suggest this Plan is simply Step 1 in a two-step process. The second step should involve substantial re-writing in the light of public comment. Ideally, the Plan should have been drafted by a truly independent group – probably for outside the US system before going to a broader stakeholder group.

The detailed, comprehensive nature of the Plan is impressive. It reflects a very large amount of thought and effort on the part of the Recovery Team and it summarises the outcomes from a substantial research effort and much debate from a broad spectrum of the stakeholder community.

These features are both a strength and a weakness. The resulting document is long and fails to crystallize what has become a complex subject with a plethora of strands of evidence pointing, confusingly, sometimes in different directions. The weighting placed on each strand of evidence is also subjective to a great extent. While some strands may be misleading others may be entirely congruent with the true picture, but there, unfortunately, no way of testing which of these is correct. Although the Recovery Team chose, correctly in my view, to adopt a “weight of evidence” approach instead of relying only on a quantitative model, I got a strong feeling that the Recovery Team struggled to deal with this approach. The result is a rather rambling, occasionally inconsistent review of the evidence and an approach to assessment of both the evidence and future actions that lacks a transparent structure. Inconsistencies come from the repetitive approach to the review of evidence that is often revisited on several occasions throughout the document, sometime with a slightly different slant. A committee wrote the document and this is certainly the impression it conveys.

Interestingly, many of the following criticisms and suggestions are echoed in some form within the pages of the Plan itself. In fact, the Plan probably reflects more about the controversy surrounding this subject, and the wish to include all points of view, than the need to focus on balanced and practical management actions. There are numerous reviews of the SSL issue and I suggest that simply referring readers to these documents could condense much of the Plan and, in places where it is necessary to provide detail, this should be placed in an appendix or an on-line bibliography.

The Plan places a substantial emphasis upon its analysis of the possible causes of the recent decline in the Western Distinct Population Segment (W-DPS). While the biology involved is fascinating and enormous progress has been made during the past 10 years, the objective of the Plan may not be well served by this approach. Structurally, much of this analysis could be consigned to an appendix.

Although the Plan states in several places (especially in relation to the PVA) that knowing what caused the decline in the past is important information, this is only true if (1) there is a

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reasonable chance of parsing the variance in the sparse historical data between causes and (2) the outcome of this analysis shows that there is a factor that can be actively managed into the future. Whatever the current or historical factors that led to the current population status, only a small number of these can be actively addressed through management. Most, perhaps all, of these have been addressed through precautionary actions during the past 15 years with the introduction of regulations to control the killing of sea lions and for fisheries operating in the vicinity of sea lion rookeries and haulouts. Apart from additional curbs on fisheries, which could be viewed as punitive given the lack of evidence of effects from fisheries, it is quite difficult to see what more can be done.

## **Specific comments**

### A. Population definitions

Defining the biologically significant populations is central to all management approaches. In this case, it has implications for the criteria used to re-list the conservation status of the SSL under the ESA. The RP continues to consider the Steller sea lion in two Distinct Population Segments (DPS), one to the west of Cape Suckling and the other to the east. I appreciate that perhaps little can be done about this because of the ESA designations but I do not think the Plan took a sufficiently critical look at the evidence and theory supporting these divisions.

Much of the recent management strategy has been built upon the assumption that these population segments are, for practical purposes, demographically distinct. Although there may have been some pragmatic reasons for doing this at the time of the ESA designations, the recent evidence, based both on DNA and the movement of marked SSL, suggests that this may no longer be the most appropriate interpretation. The RP reviews these studies and appears to support this interpretation within the review (pages 8-11) but then it appears to ignore this information about other possible segments within the population (e.g. Western Aleutians/Russian compared with others regions) for the remainder of the document and retains an approach to management with the population divided along the original lines. At some point, this needs to change a structure of management that does not fractionate the population according to the latest, and almost certainly incomplete, knowledge of the genetic structure. Adopting a metapopulation approach would provide a consistent framework for management that would be less sensitive to chance of our knowledge of the population structure than the current approach.

Even when the decision was made to define two populations, the evidence supporting this came from a single study of mitochondrial DNA. A more parsimonious interpretation of these data was that there was a genetic gradient across the range of the SSL and that geographical distance was likely to be a predictor of the extent of genetic introgression between different regional sub-populations. However, at the time it was published, the data from the Bickham study tended to reinforce jurisdictional issues concerning the research effort and management. It was also reinforced by the observation that, overall, the population from the E-DPS was increasing whereas that from the W-DPS was declining. This was, and remains, a circular argument because the management units were being defined, in part, by the different trajectory of the populations in each region. Thus, the response variable itself (the population

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trajectory) was being used to help define which response variable should be used. Notwithstanding this, the logic of comparing the biology underlying the different trajectories in these regions, as a natural experiment, was an opportunity that was correctly seized upon but translating this into management units may not be wise.

The current Plan has simply reiterated and reinforced this problem throughout but especially through its renewed implementation of a Population Viability Analysis. A more progressive approach to modelling and managing these populations would be to consider them as a single metapopulation. We know that there have been contrasting dynamics within sub-divisions of both the eastern and western DSPs and the grossing-up across this simplistic east-west division needs to be justified against what may be more natural divisions focussed upon rookeries or other objectively-derived spatial sub-groupings. At very least, there is a need to accompany assessments involving the east-west split with alternative views.

#### B. Population viability analysis

A population viability analysis (PVA) has been used within the RP. Although it only appears as an appendix, and the RP states that it has not been used directly to define the recovery criteria, there is little doubt that this PVA has had an influence on the RP (see RP pp115-116).

In general, PVAs attempt to predict the future probability of extinction of a population based upon what is known about the dynamics and general biology of that population. To be useful, the general assumption that needs to be upheld is that the vital rates are drawn from a stationary distribution. In lay terms, this means that the mean and variance of the distribution should not change through time. However, it is rarely the case in nature that environmental conditions lead to these types of stationary distributions. Consequently, the results of PVAs are probably of more academic than practical interest. Used in the context of advising about management, they could be at least misleading if not dangerous because, to the uninitiated, they lend a level of authority and certainty to results that is unlikely to be merited. They should always be accompanied by a strong health warning.

The RP points out that several PVAs have been carried out on the Steller sea lion. Each has produced slightly different results but all have shown a degree of consistency in that most of the predictions have been somewhat gloomy for the SSL. This really is not surprising given that the only data these PVAs have had to work with have come from the periods when SSL have been in decline. Consequently, the results of the PVA presented in the RP (known as the “Goodman PVA”) are entirely predictable and perhaps even trivial. However, the Goodman PVA does make an effort to capture the periodic nature of different phases of decline.

A PVA is a model and it is easy to pick holes in any modelling exercise. Ultimately, the utility of a model can be judged from the extent to which its predictions are true. The Goodman PVA is not assessed against data and has some particular features that make it difficult to use in the context of the RP and that should lead one to question its value as a management tool. In summary, these are:

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1. The PVA is not formulated in a way that would allow it to be tested. Although this would be difficult given the short time-series of data from the SSL, the type of method that could be used would involve developing the PVA using the early part of the time series and assessing how well it predicts the later part of the time series. If this is considered to be impractical then it may be correct to question whether PVA is an appropriate method to use in these circumstances.
2. The PVA appears to assume that the distinct periods of decline used in the model are statistically independent. Since the same sea lions were alive in consecutive periods this is clearly not the case. The importance of this assumption is difficult to judge but it is unlikely that rates of population change within each period were unrelated to those in adjacent periods. This derives in part from the long time lags, involving at least one generation (~10 years), between changes in some vital rates (especially fecundity) and changes in the population trajectory. It could also be argued that the most recent data about population trajectory are the most relevant and that a weighing policy should have been used both to take this into account and to account for the relative number of years used to define each period in the historical decline.
3. Density-dependence was assumed not to be important. The issues concerning density-dependence break down into several distinct points:
  - a. An extended discussion was provided for discounting the influence of density-dependence based partly upon the lack of evidence supporting the presence of density-dependence of a type experienced by marine mammal populations in the Southern Ocean. This was an unfortunate example to use because there is substantial uncertainty surrounding the apparent density-dependence in this case and it is wrong to use it to draw parallels with the SSL.
  - b. The remainder of the discussion on density-dependence made the case that there are no data to support an effect in the SSL and that some general underlying theory suggests that density-dependence is not essential for a PVA. Again, this appears to be a stronger argument for PVA being an inappropriate tool than for excluding density-dependence.
  - c. Looked at broadly, it could be argued that the dynamics of the SSL population do show some features of classical density-dependence when long time-lags are present (see Figs 1 and 2 on pages 268-269).
4. The PVA assumed no metapopulation structure. The dynamics of the SSL population are certainly not as simple as depicted in the PVA. To assume that the historical population declines were evenly distributed is untrue and the balance of local versus range-wide effects could have important implications for the long-term viability of the population. It is essential that a PVA should capture this type of behaviour.
5. The PVA assumed a critical population size of 4743 as extinction. While this is a very low population which anybody would wish to avoid, it is well above the historical population sizes of several marine mammal species that have subsequently shown phenomenal recovery. The PVA should really have shown effects when this assumption was relaxed and when density-dependence was present.
6. The PVA acknowledged that the historical decline could have been modified or eliminated by management actions. However, because the purpose of the management action was to eliminate those more obvious causes of the decline that were amenable to management, it would seem important to use a weighting policy for values of  $r$  that come from the eras with and without such management action and to weight much

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more heavily towards values long after the introduction of management because of the time lags required for some management effects to influence  $r$ .

7. The PVA assumed that the composition of the population, in terms of potential  $r$ , is stationary. If part of the cause of the decline in the 1980s was shooting on rookeries, which appears to be the case, the fittest segment of the female population will have been removed. Recovery of the population structure from such a stress could take several generations (arguably it took about 100 years from the cessation of hunting on the breeding grounds until rapid population increase was resumed in several species of southern fur seals). Consequently, the PVA would require to capture this feature of the population, possibly through a similar approach to that suggested under item 5 above.

It is likely that Goodman would argue that the purpose of the PVA was not to capture this type of detail but rather to show general trends based upon recent experience. Nevertheless, all the features listed above have the capacity, either individually or in combination, to radically affect the output from the PVA and they simply serve to illustrate the fragility of PVA as a rational approach to informing management decisions.

My recommendation is that the inherent properties of PVA and the specific features of the current PVA used in the RP mean that the PVA should not be included in the RP.

### C. The concept of recovery (development of the rationale for de-listing and down-listing)

The RP provides its definition of recovery through the development of down-listing criteria. Given the legal structure within which the RP is constituted it is understandable why this has been done but there the RP does not give a clear, unambiguous definition of recovery because it seems to be split between a pragmatic approach and one based upon historical precedent.

The concept of recovery implies that the system in question (in this case the ecosystem occupied by the SSL) has some form of static, or equilibrium, state. The RP is inconsistent in its view of this concept. At the detailed level it makes frequent reference to how the ecosystems occupied by the SSL are likely to respond in complex ways to a wide variety of external drivers, including physical drivers from climate, anthropogenic drivers associated with fisheries, historical harvesting of marine mammals (including SSL themselves), and many other forms of disturbance. In fact, the RP probably only documents some of the most obvious forms of disturbance to the ecosystem occupied by the SSL. Moreover, these systems will have internal dynamics that will lead to changes in the relative influence and abundance of different components, even without external forcing.

Therefore, while the RP does consider appropriate levels of complexity, this is not reflected in the grand challenge presented by the RP – to introduce management that will cause the SSL to “recover”. In reality, the concept of “recovery” makes little sense in dynamic complex systems like ecosystems except if it is understood in terms of a human value-judgement about the most appropriate state for a system. This may be a particularly important consideration for marine ecosystems that probably display much less stable large-scale dynamics than many familiar terrestrial ecosystems.

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It is an established principle in ecology that the longer the time series of data that is available then the more variability will be encountered. The historical time-series for SSL are comparatively short so we can probably assume that we have seen only a small part of the type of variation in population size and distribution that has been present through pre-history. The question about whether the changes in the past 30 years are unprecedented cannot be answered and, consequently, the question about what is the desirable objective for a SSL population size cannot be derived using a meaningful biological argument. Even accountancy exercises showing how many SSL were lost to anthropogenic effects are unlikely to be very helpful because they fail to consider the trade-offs that may have existed between anthropogenic and natural effects on mortality, i.e. some animals removed through anthropogenic effects may have, in other circumstances, been preferentially exposed to natural mortality factors, such as predation.

I suggest that the Plan should have a strong warning up-front which should articulate the following type of concerns:

1. The objectives set for recovery may not be achievable. Even if anthropogenic effects have caused the current status of the SSL, the implication of the current plan is that the ecosystem that once supported larger numbers of this species can be restored and it is unlikely that this is an achievable objective.
2. A Recovery Plan of this type is a single-species management approach to a multi-species problem. Consequently, as our understanding of ecosystem dynamics improves, we will almost certainly find that there are trade-offs between species abundances. *Not all species can be abundant all of the time.* SSLs may be part of this trade-off.
3. “Recovery” is a term invented by managers that expresses a value-judgement they have made about the most appropriate state of a population. It does not necessarily represent the biologically optimal solution, or the solution that is in the best long-term interest of the SSL.

Where does this leave us with respect to a rationale for down-listing or de-listing? The PVA, which has many limitations (see above), is used implicitly within the RP to underpin the rationale for re-listing. The RP has explicitly stated that it has used a “balance of evidence” approach but has chosen to define recovery in a very prescribed way (see below). The rationale for re-listing is, I suggest, already present within the RP but it is simply not stated as such. This encapsulates the adaptive approach employed using a risk assessment framework. Re-listing the species under the terms of the ESA should occur as a result of reviews to qualitatively assess the probability that the factors driving the decline in the SSL have now been eliminated or dissipated. Although it is highly unlikely this will be achieved through knowledge of the actual causes of the decline, it can be achieved by building confidence that the dynamics of the SSL population no longer reflect the regime of the 1970s to 1990s. The risk assessment framework (Appendix 1) provides the structure within which this process could be implemented. It is worth noting that such a process is also sensitive to shifts in societal values and changing cost-benefit scenarios both of which could be substantial over the time periods involved.

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There is no scientific method to determine when re-listing should take place and in what circumstances. The PVA does not supply one (although some might maintain that it does) and the current RP gives just one of many possible scenarios for re-listing. Decisions about re-listing are entirely societal judgements, just as it was a societal judgement to list the SSL in the first place. It should not be the responsibility of science to provide justification for unwinding a past judgement and its consequences. I suggest that decisions about re-listing could be informed by judgements about the following set of questions, bearing in mind that it was the trajectory of the SSL population and not its absolute size that was the main reason for listing it in the first place.

Questions that could be used in a rationale for down- or de-listing of the Steller sea lion

1. Is the overall population size of the SSL sufficient to sustain it into the foreseeable future (around 25 years, or one human generation)?
2. Compared with what is known about other marine mammal species, is the existence of the Steller sea lion as a species apparently endangered?
3. Is there evidence that the dynamics of the Steller sea lion population that caused it to be listed no longer apply?
4. Bearing in mind that the causes of historical population dynamics may have been natural, is there evidence that management actions taken as a result of listing the species have been effective?
5. Is there reason to believe that down- or de-listing will cause past hazards that could have been implicated in the decline, to recur?
6. Bearing in mind that population distributions will change often on long time scales, are there core regions of sustained, strong population growth that counterbalance those that may be in decline?

In circumstances where society can, in general, agree to say “yes” to questions 1, 3, 4 and 6 (qualified in the case of question 4), and “no” to questions 2 and 5 then there should be a strong rationale for down- or de-listing. Whether this involves down-listing, as opposed to de-listing, will depend on the current level of listing (i.e. threatened or endangered). Down-listing is simply a staged progression towards de-listing and is part of the process involved in building confidence in the judgements being made.

#### D. Recovery Criteria

According to the RP, before reclassifying the SSL, NMFS is duty bound to determine that the species is not likely to become endangered in the foreseeable future throughout all or a significant proportion of its range. Moreover, the RP also states that “the recovery criteria *must* [my emphasis] include measures of demographic health (biological criteria) as well as measures that indicate the elimination of threats to the species (listing factor criteria)” and both “*must*” [my emphasis] be met before reclassification is possible (see p115).

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These are very strong statements, especially in the use of the word “must”. It is probably impossible, at least within the foreseeable future, to reclassify the SSL based upon these criteria, if strictly applied, and the suggested reclassification of the eastern DPS within the RP does not meet these criteria. A mistake made in the RP is to become fixated on a single definition of recovery, (e.g. P115, para 4, lines 1-2) when recovery mainly concerns ensuring that the causes of recent declines are no longer active. This reflects insufficient attention in the RP to the actual meaning of recovery (See Section B above).

The RP sets out 4 fundamental criteria for down- or de-listing. These concern statistically significant population growth for 15 years (down-listing); an average annual growth rate of 3% over 30 years (de-listing); no decline in two adjacent sub-populations; no decline in 5 of the 7 sub-populations and the observed dynamics should be supported by other indicators of vital rates. The following reasoning elaborates why the current definition is not a feasible way of dealing with the issue:

*General approach adopted by the RP*

1. Since it has proved impossible to distinguish clearly between potential causes of the decline in the western DPS it seems highly unlikely that we will be able to say with confidence that specific threats to the species have been eliminated. With the passage of time, we may be able to build confidence that the threats which led to the decline are no longer active but it is unlikely we will be able to provide “measures that indicate elimination”. Consequently, the statement at the bottom of page 117 “eliminating and controlling the threats to the western DPS ... is imperative prior to downlisting...” is impossible to achieve because it has proved impossible to define the active threats, or hazards, and some – perhaps most - of these are well beyond our control. Unfortunately, this kind of inconsistency in the logic applied within the RP is common. On the one hand, the RP acknowledges the lack of progress in this area and the very small chance of progress in future but, on the other hand, underlying the RP is the blind belief that a solution to this problem will be forthcoming.

*Use of vital rate and other indicator criteria*

2. The indicators of demographic health described in the RP are subject to considerable uncertainty. Although this is acknowledged within the RP, the RP has an over-inflated expectation of their value. The RP has to recognise that *the apparent certainties of today can turn into tomorrow’s uncertainties* very quickly indeed as new knowledge is produced. There is an assumption that new scientific knowledge can narrow uncertainty and this is certainly not always the case. Although NMFS and other researchers have made substantial efforts to improve knowledge of underlying demographic vital rates, there is still insufficient confidence in the data to allow their use to judge whether these rates are pointing in the right direction for down-listing or de-listing to occur.
  - a. Almost without exception, measures of vital rates are biased to an unknown degree. Where there are reliable data about one vital rate it is often possible to anchor the other(s) using this information but where there is substantial uncertainty around all the vital rates, as in the case of the SSL, such a task becomes impossible.
  - b. I present two examples of how certainties can be overturned

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- i. The RP repeats assertions made in the NRC report that were initially developed by Bowen *et al.* Both these sources suggested these assertions could be used to indicate “biological criteria” for the effects of food (see p112 of the RP). Building upon one of the few apparent certainties in the biological criteria, the RP states that “sea lion birth mass, pup growth rate, and body condition will decrease whereas foraging effort would decrease [presumably *increase* is meant in this case]” as a result of changes in food (prey) caused by fisheries and/or climate/regime shifts. Unfortunately, this is not necessarily true, although it was once thought to be certain. Much of this theory has been based upon my work with Antarctic fur seals. Even under extreme conditions of food deprivation not all these indicators necessarily point in the same direction. For example, under extreme food deprivation, it is possible for apparent pup growth rate actually to increase because of sampling problems in years of low pup survival (i.e. the covariance of measured growth rate with pup survival).
  - ii. The population time-series for grey seals breeding in the NE Atlantic is one of the longest and most detailed on record for any marine mammal. Up to the early 1990’s, the total population could be estimated from the counts but since then changes in the dynamics of the population have meant that we can now no longer estimate the population to any useful degree of accuracy. However, it took about 10 years for the scientists involved to convince themselves of this problem and then admit publicly that in spite of improved methods and substantial effort, their estimate of the uncertainty surrounding the population size was actually increasing year on year. In this case, more knowledge led to greater uncertainty as we understood more about the nature of the measurements being made.
- c. The underlying reason for this situation is that neither fecundity nor survival can be measured directly and fecundity is confounded by survival to some extent. The same is true for the SSL. We have no unbiased method for measuring these vital rates. Current fecundity measurements using aerial surveys will be subject to biases that are specific to individual haulouts and rookeries. Even a cursory consideration of where these biases can originate does not provide confidence that an assessment of biological criteria for reclassification, if strictly applied and based upon current measures of vital rates, will come up with the right answer.
- d. Biases in the measurement of vital rates emerge from difficulties with sampling evenly across the population. For example, in mark-recapture studies, including branding programmes, even if one can sample evenly when marks are applied, recaptures are unlikely to be unbiased. The simple choice of which rookeries to brand pups in will probably introduce an immediate bias, for example because of the reasons for historical declines (see Appendix III, section 1 and 3). It is possible that fecundity measured in branded females that can be re-sighted reliably at specific rookeries year on year may be relatively unbiased within the context of that rookery but, by choosing specific rookeries, the measure will probably be biased if applied at a whole population

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level. Similarly, if counts on non-pups are to be used, it is important to appreciate what may bias the proportion of time spent at sea in each age class and this is something that may covary with region and local population size and composition. Accounting and correcting for these types of biases should be an important and long-term priority, but it is hardly acknowledged as an issue within the RP.

*Overall population growth criteria*

3. The down-listing criteria involve a statistically significant average increase in the population for 15 years (1-2 generations) based upon counts on non-pups and the de-listing criteria involve average annual population growth of 3% for 3 generations. Both these assume that carrying capacity has not changed.
  - a. If carrying capacity has changed then this may never be achieved and it is, therefore, an unreasonable criterion. A more pragmatic approach would be to review the classification of the SSL regularly (perhaps at 5 year intervals) so that appropriate actions could be taken in the light of most recent knowledge and most recent opinion (see suggested rationale at the end of Section C). For example, if the SSL population remains stable for the next 30 years, but does not meet the 3% increase criterion, should we still consider that the population is endangered? A sensible risk assessment (see Appendix I) would most likely lead to de-listing under these circumstances because the actual risk to the population would be deemed to be low.
  - b. The choice of 3% seems not to have any biological basis other than its congruence with the observed rate of increase in the eastern DPS. This is a reasonable judgement but it would be considerably more honest to express this explicitly. Once again, this shows that the actual process being used by NMFS to develop reclassification criteria follows a pragmatic risk assessment approach rather than one set down by highly prescriptive measures of demographic health and measured indicators that threats have been eliminated.
  - c. An issue with these criteria involves the counts of non-pups. This assumes a lack of systematic bias associated with these counts. What determines the number of animals at a haulout is likely to be very complex and biases are also likely to co-vary to some extent with local population size. For example, if there is competition for haulout space one could imagine that the relationship between haulout population size and actual population size will be non-linear as the haulout carrying capacity is approached. Even if this is not the case, a number of other factors could lead to similar effects.

*Sub-population growth criteria*

4. No decline in adjacent sub-populations and no decline in at least 5 of the 7 recognised sub-populations. This is assumed to address the duty that the Agency has to ensure that the SSL is not likely to become endangered in the foreseeable future throughout all or a significant proportion of its range. However, setting such objectives does not appear to consider three important issues:
  - a. The current SSL population dynamics support a hypothesis that there is a shifting distribution of SSL across its range. Because of the philopatry shown by species like the SSL, shifts are likely to occur over generational time scales rather than through the movement of individuals. If this is happening, possibly

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because of completely natural changes, the requirements for sub-population increases are unlikely to be realised whatever the circumstances.

- b. There is an assumption within this criterion that the current sub-population definitions are congruent with biological sub-populations. We do not know this. If some of the current sub-populations are subsequently found to be more, or less, related in terms of their dynamics this will affect the assessment under such a criterion. It is unrealistic to suggest that the dynamics of adjacent sub-populations are not linked so, by defining that these declining sub-populations will be adjacent, probably places a standard of stringency on the reclassification that was not intended.
- c. Inherent within this recovery criterion is an assumption that some potential drivers of the past decline (e.g. fisheries) are more likely than others (e.g. climate change, regime shift or predation). This is because the criteria are focussed at the sub-population level, i.e. on local rather than global effects.

### *Summary*

5. There are several serious problems with the current reclassification criteria. These are such that it seems unlikely that the SSL eastern or western DPS could be down-listed under any circumstances. However, it appears that the RP is out of step with the real methodology used by the Agency. The prescriptive features of the criteria being used are naïve about the capacity for new knowledge, or changing circumstances, to alter perspectives on what defines recovery. The pragmatic solution is for NMFS to provide a more honest description of how it really does business with respect to the review process for classifying the SSL. Otherwise, it will almost certainly find itself being brought to book for breaking its own rules.

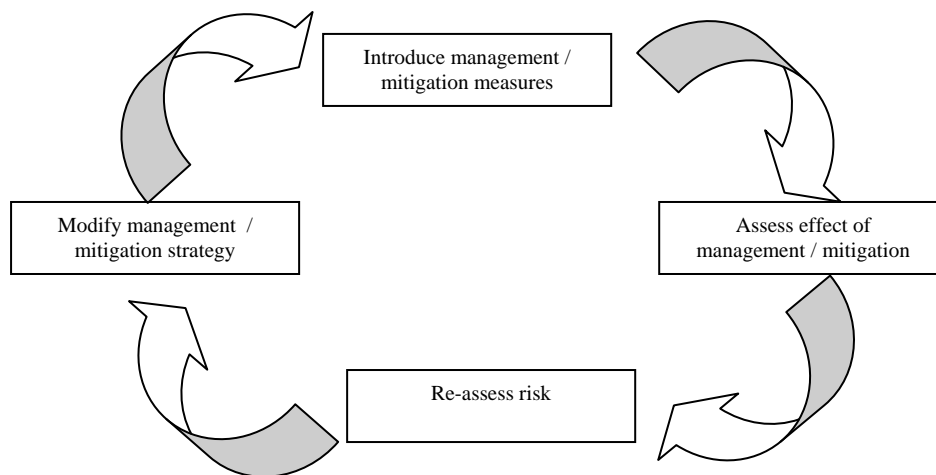
### *Recommendation*

My recommendation is to revise the recovery criteria because the highly prescriptive nature of the recovery criteria, the difficulties with obtaining adequate information to support some of these criteria and lack of biological realism in some of them mean that they are unlikely to be met.

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### E. Adaptive management

Adaptive management seems to have two definitions in the RP. One, which is aligned with the risk assessment approach (Appendix I), involves an iterative, or adaptive, approach to management described in the following diagram.



The other definition seems to involve undertaking some form of elaborate experiment in an attempt to determine what are the underlying drivers in the population dynamics. This has been suggested by a number of individuals and committees, perhaps most notably the NRC<sup>1</sup>. However, to date there has been no clear definition of what such an experiment would entail and there are strong doubts about its practicality, costs and the value of its outcome. This is not the place to provide a critique of such an experiment but there are likely to be few positives in such a plan.

Alternatively, adaptive management by the definition used here, and as summarised by the diagram above, has been an implicit activity in SSL recovery actions to date and is a central plank of the risk framework (Appendix I). The RP has not made a clear link between past management and the proposal for future management. Few significant efforts have been made to analyse the effectiveness of current management actions, re-assess risk in this context and then formulate new or modified management actions upon that basis. This is a serious failure with the RP.

I recommend that the risk framework approach which has been implicitly adopted by NMFS towards the management of the SSL in the past, but which is not acknowledged explicitly within the Plan, should be used to develop an adaptive approach to management.

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<sup>1</sup> National Research Council 2003. The decline of the Steller sea lion in Alaskan waters: untangling food webs and fishing nets. Washington, D.C.: National Research Council 216 pp.

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Furthermore, I recommend that a full and proper assessment be carried out of the effectiveness of past management actions before any further management actions should be considered. Appendix II considers how this may be done.

#### F. Current mitigation

The RP recommends maintaining current mitigation measures (P114). Although there is much to recommend not changing mitigation measures until an appropriate assessment of their effects has been carried out (see Section ), the logic for maintaining “current” measures beyond any review phase is unclear. Although current mitigation should be maintained for the time being, I recommend that this should be subject to periodic review in the light of new information to ensure that it is appropriate.

While the RP makes it clear that the recent apparent cessation of decline might have been the result of management intervention, it seems unlikely that mitigation measures introduced in the past 5 years will have been responsible for this. It seems most likely that changes in the population dynamics that are detectable with the precisions available using current survey methods will take at least 10 years to materialise as a measurable effect at the level of the population. Consequently, if measures were to be maintained then those introduced in the early 1990s, especially the strong controls on shooting, would appear to be the most effective. Measures introduced to control the indirect effects of fishing are founded upon a rather simplistic view of how these interactions between sea lions and fisheries might occur.

Concerning the Actions Needed (p5), there needs to be a careful cost-benefit analysis carried out on each management measure in order to assess their current relevance. The RP has not, in general, done these cost-benefit analyses. Cost-benefit analyses are particularly important given the high level of uncertainty in current data and the potential impact that some measures may have upon the economics of the fishery.

#### G. Recovery actions

Implicit within the recovery action is a long list of research (Section D, p124) Although some narrative has been provided to support these items there is no rationale for prioritising between the different items. Some items are extremely ambitious and probably completely impractical within the funding limits, some have already been heavily researched and probably should be viewed as of low priority and others that are vital for future management have been rather lost in the mass of actions of very variable relevance. Adoption of a risk framework (Appendix 1) would help to define those items that address areas where vital knowledge is absent. However, whatever is done, the greatest challenge will always be to obtain unbiased samples of the population. The more information that becomes available to suggest there is sub-structure within the population the more difficult and complex this will become. None of the current list of actions adequately addresses this problem.

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The section on Recovery Actions would benefit from some sharp editing. There is a lot of loose wording, e.g. tasks beginning with words like “assess the”, “collect and analyze” or “monitor and evaluate” do not give an impression that the authors have much idea how to carry out the specific task or what the objective of the task happens to be. In the UK, this would fail the first hurdle of peer review and, I suspect, it would also fail under the review system used by NSF. The research-related recovery actions are very sloppy and do not present a cohesive programme to meet specific objectives of management. They are a rather ad-hoc mix that amounts mainly to a continuation with the *status quo*, some of which should not be continued. The fact that the development of an implementation plan for recovery actions – presumably the PR itself - (action 1.5) appears under the section titled “Baseline population monitoring” suggests inadequate thought has been given to the structure of this very important section.

It is beyond the scope of this review to provide a detailed analysis of the research priorities. However, in general, those items of research that are directly associated with the measurement of population dynamics, including population trajectories and their underlying vital rates should be considered as essential. An important element of this, which is currently absent from the PR is to attempt to quantify the biases within the current measurements.

Of medium priority are the studies that define the habitat used by SSL, diet and the prey fields exploited by SSL. This may be of particular importance to the fishing industry (in which case it may be of high priority) because they need to be satisfied that the management actions that have been taken, and that affect their financial viability, are well founded. However, given the potential complexity of indirect interactions between SSL and the fishery, it seems unlikely that this type of research will do more than illustrate the uncertainties involved with attempting to manage these interactions.

Of lowest priority should be areas of research into the underlying biological processes that have received significant attention during the past 5 years unless they show a real prospect of producing methods that are sufficiently refined to be able to provide useful insights within the foreseeable future. There is a strong argument based upon both practicality and ethics (mainly because they are difficult animals to work with, not because they are especially rare in many parts of their range) not to conduct speculative research on SSL unless there is no alternative and more accessible model species.

A sample set of comments concerning tasks is given below, but this should not be seen as exhaustive:

*Monitor health, body condition, and reproductive status* (1.3). This is affected by ability to adequately sample from across the population. The indicators of “health” are also uncertain and open to complex interpretations. The chances of finding any effects using these methods is small<sup>2</sup>.

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<sup>2</sup> The reasoning here concerns how reduced condition, health or reproductive capacity is likely to be manifest within the population. There will be a spectrum of fitness amongst individuals within the population. As conditions decline for SSL those individuals at to low end of the spectrum will be affected before those at the higher end and some of these may be eliminated from the population (and consequently from the potential

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*Determine the environmental factors influencing sea lion foraging and survival (2.4).* The second part of this task is dependent upon being able to measure survival and this would appear not to be feasible at least with sample sizes and appropriate unbiased population sampling. The first part is feasible and should be of medium priority.

Items 2.4.2 and 2.4.3 would appear to be highly speculative and enormous in their scale that is not reflected in a realistic costing in the table in section E of the RP. If we, as a global science community, could resolve these problems we would be doing well. It is well beyond the scope of any SSL research programme to do this in a meaningful way. Much the same could be said for 2.6.1, 2.6.2, 2.6.5 and 2.6.7. In the case of 2.6.7 it would be fair enough to “explore” these models but quite a different matter to implement their outputs in any meaningful way to manage SSL-fisheries conflicts.

*Investigate sea lion bioenergetics (2.5).* Much of this research has been at the centre of the hypotheses concerning nutritional stress. For the reasons set out in Appendix III, there may be strong reasons for viewing this as a low priority. Most of these objectives have already been met during the past 5 years. With the possible exception of 2.5.4, it is doubtful if further refinement will help resolve the likelihood of the various hypotheses being true. Much work has been done in this area using captive individuals but if strong effects had been present they would probably have been observed already. The limitations of sample size and the difficulties of working with SSL in captivity suggest that in this area using alternative species would be a more profitable way forward. Also see the discussion in Appendix 3.

*Design and implement an adaptive management program for fisheries, climate change, and predation (2.6.8).* This is very difficult to understand because this would appear to be the whole of the RP contained within a single sub-task.

Overall, the section detailing the research required as part of the recovery actions needs to be re-written as it does not present a credible and cohesive research programme.

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sample range) quite rapidly. Thus, one would not necessarily expect to observe an overall lowering of reproductive condition, body condition or “health” as a result of changes in the environment.

## A Risk Assessment Framework for the SSL Recovery Plan

The RP attempts to identify and classify the major threats to the SSL. There is a semantic issue here that concerns the difference between “threats” and “hazards”. The Plan has actually listed “hazards” (see Table IV-1, p 100 and related text) but its analysis attempts to articulate the extent to which these hazards threaten the objectives of the Plan.

The Recovery Plan should probably contain a strong drive towards risk assessment and risk assessment frameworks usually deal with hazards in advance of threats. In this context, a threat is an integration of the severity and likelihood of a hazard having an effect and characterises the risk associated with a particular hazard. Although risk assessment is implicit within the processes set out in the Plan. The table below is a modified from of a table produced in “Science and Judgment in Risk Assessment” (1994, Commission on Life Sciences, National Academy Press) and it summarises an alternative approach that could provide greater clarity to the process of assessing and then mitigating threats.

Table 1. Risk Assessment Framework. Normally, this would include an adaptive approach to controlling risk. Having carried out Steps 1 and 2, Steps 3-5 are then carried out repeatedly in a cycle of a duration that is determined by the time scale over which the effects of mitigation can be assessed. Note that the risk assessment should, ideally be carried out sequentially but this is rarely the case with difficult problems like that of the SSL when there is incomplete information to fully respond to the challenge of each step in the risk assessment.

- Step 1:**     **Hazard Identification** entails identification of the sources of any hazard (in this case hazards that cause mortality or reduced reproductive rate) and the circumstances in which they are likely to occur, quantification of the concentrations at which they are present in the environment, a description of the specific effects of the hazard, and an evaluation of the conditions under which the effects of the hazards might be expressed in Steller sea lions. Information for this step may be derived from the direct correlation of effects with the presence of a hazard as well as other types of experimental work. This step is common to qualitative and quantitative risk assessment.
- Step 2:**     **Dose-Response Assessment** entails a further evaluation of the conditions under which the effects of the hazard might be manifest in exposed species, with particular emphasis on the quantitative relationship between the dose and the response. This step may include an assessment of variations in response, for example, differences in susceptibility in relation to age, sex, reproductive status and time of year.
- Step 3:**     **Exposure Assessment** involves specifying the population that might be exposed to the hazard, identifying the routes through which exposure can occur, and estimating the characteristics (magnitude, duration, and timing) of the doses that species might receive as a result of their exposure.
- Step 4:**     **Risk Characterization** involves integration of information from the first three steps to develop a qualitative or quantitative estimate of the likelihood that any of the hazards will be realized in exposed species. This is the step in which risk-assessment results are expressed. Risk characterization should also include a full discussion of the uncertainties associated with the estimates of risk.
- Step 5:**     **Mitigation** involves the introduction of measures to reduce or eliminate risk. Often this

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will involve a process of adaptive management of the risk. Based upon the results of Step 4, appropriate measures may be tested by repeating Steps 3 and 4 after an appropriate interval of time. Depending upon the extent to which mitigation has led to reduction of risk, it may then be appropriate to adjust the mitigation procedures. In circumstances where Step 4 suggests there is little understanding of a hazard and its possible effects it may be appropriate to apply precautionary mitigation.

SSL management has been following the rationale set out within Table 1 for the past 15 years, albeit informally. The Recovery Plan provides an opportunity to formalise this approach by (i) describing the qualitative and quantitative progress made towards filling in the blanks in the risk assessment procedure; (ii) attempting to conduct an assessment of what blanks remain and what prospect there is – especially given budget constraints – of making progress toward filling in those blanks; and (iii) attempting to show how effective current mitigation has been at reducing the apparent risk. It seems that the recovery plan is in need of this or some similar structure.

The need for this is particularly apparent in the section D on the Recovery Action Outline Narrative where there is no connection between the parts of the assessment of threats (sic) and the recommended actions. This means that there is no procedure for prioritising amongst the actions. In fact the Plan had no useful advice about how it should be implemented and it simply left this problem for a future process (see 1.5, p 132). Adopting a risk framework provides simultaneous guidance about prioritisation of mitigation, research and implementation.

## Assessment of the effectiveness of past management actions

The risk assessment process requires that an assessment be made of the effectiveness of measures taken to manage perceived risks. This is an essential pre-cursor to modifying the management actions.

In the case of the SSL, there is a problem associated with doing this because of the complex nature of the interacting factors leading to recent changes in the population. Ideally, we would want to know the extent to which recently observed dynamics have been the result of management actions first brought in the early 1990s. Theoretically, we might expect the effects management actions taken then to start being observed about one generation (~10 years) later. A simplistic view suggest that this may be the case because there are signs of population recovery but what is the balance of evidence to support this view?

Several studies have examined single potential drivers of SSL population dynamics (e.g. most recently Hennen 2006<sup>3</sup> for the effects of fisheries). There are problems with these single-effect approaches. In particular, (1) they start with a presumption either that there is, or is not, an effect of that driver and thus they may be inherently biased depending upon the viewpoint of the analyst; (2) even if they tend to show effects, there is no way of saying if those effects are acting directly or through another driver to which both SSL and the driver being examined are linked, i.e. correlation does not imply cause.

These single factor statistical designs are, therefore, of limited value. However, multifactorial statistical designs, possibly using Bayesian inference as a tool, have the potential to rank the different drivers by the relative extent to which they explain the data. The types of designs available would have the capacity to include the introduction of management actions and, therefore, would be able to help test the effectiveness of these actions.

Wolf & Mangel<sup>4</sup> carried out a preliminary form of such an analysis. Although there have been some criticisms of the exact formulation they used, especially with respect to the data sets, they set out to assess the relative evidence within the data to support the various hypotheses there are for the SSL decline<sup>5</sup>. Their main conclusion was that there was little statistical power to distinguish between most of the hypotheses but further work involving more complete and up-to-date data sets could improve this.

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<sup>3</sup> Hennen, D.R. (2006) Associations between the Alaska Steller sea lion decline and commercial fisheries. *Ecological Applications* 16, 704-717.

<sup>4</sup> Wolf, N. and Mangel, M. 2004. Understanding the decline of the Western Alaskan Steller sea lion: assessing the evidence concerning multiple hypotheses. NOAA Contract Report AB133F-02-CN-0085.

<sup>5</sup> The approach is only as good as the data that are used. Specifically, if two factors result in exactly the same effect, as could be the case involving the effects of regime shift or fisheries, then the approach will be unable to partition the variance in the SSL variable between these causes. However, with time-series data involving each rookery or haul-out, spatial and temporal variations in cause-effect relationships may differ and this could tend to help the model to partition variance more towards one factor than to another.

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As with the single factor approach<sup>2</sup> this multifactor approach will depend on how the model is formulated and the perspectives of the analyst – this is commonly called *model error* or *model bias*. Therefore, ideally the analyst should be remote from the debates about the relative strength of different hypotheses but sufficiently aware of the biology to ensure that this is correct, at least as we currently understand it.

To address these apparently opposing requirements I suggest the following analytical exercise be undertaken:

1. Three analysts should be identified who have the capabilities to undertake the analysis but who have been comparatively remote from the current debates;
2. Each should be provided with the same sets of data and the same biological brief, including information about management actions. (note that the development of the biological brief will be critical and should be carried out by those who have the greatest knowledge of the data).
3. Each should then be given 18 months to produce a multifactorial statistical model to assess the relative strengths of each hypothesis. It is essential that the work is done independently by each group.
4. At the end of the 18 month period, a workshop should be held to compare the model outputs. This should assess the different assumptions made by each model and the technical approaches taken to modelling.
5. A decision should then be made about how to proceed. This could include taking the best features of each model and combining them into a single model by converging the model designs; operating all 3 models in parallel; or opting for a single “best” model to use in future.

This approach should attempt to explain the trajectories of sub-populations, inclusive of both the western and eastern DPS, based upon available local data including environmental, fisheries, predator and other sources. The approach partitions the variance in the response (local population trajectory) among the explanatory variables using the inherent variability in these explanatory variables. This will extend and add rigour to the types of informal assessments begun by Alverson<sup>6</sup>. It will also form a basis for designing experimental approaches in future if these are required because it could be used to project the additional statistical power obtained by undertaking experiments.

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<sup>6</sup> Alverson, D.L. (1992) A review of the commercial fisheries and the Steller sea lion (*Eumetopias jubatus*): the conflict area. *Reviews in Aquatic Science* 6, 203-256.

The “is it food?” hypothesis.

Since the first symposium that met to ask the question “is it food?”<sup>7</sup>, there has been an unprecedented amount of research focussed upon this question. It may be time to ask why, more that 10 years on, we still cannot answer this question?

A range of different factors could cause reduced food availability but it is suggested that the measurable effects will include reduced body condition, reduced health indices and/or reproductive capacity. The approach adopted to date has involved a combination of broad surveys at the population level to measure condition and health as well as experiments focussed on a very small sample of captive individuals.

Although population samples from the 1980s and some results of experiments have indicated that there could have been affects from food, strong evidence has not been forthcoming. The population samples from the 1980s, compared with the 1970s, contain all the problems of potential bias referred to earlier in this document and there are significant difficulties with scaling up from laboratory-based studies of small samples of individual, which are presenting a rather complex set of results, to whole populations.

*What effects would we expect to observe if low levels of food availability affected the SSL population?* I suggest that the answer to this lies in an appreciation of the different responses at the level of individuals, because all sea lions are not equal, and also because there is not a simple relationship between cause and effect in these predators. What we would not necessarily expect is an observable reduction in the mean fitness (measured in terms of body condition, health indices or reproductive condition) of the population as a result of changes in food availability. In fact, under certain circumstances, we might even expect some of these indices to increase rather than decline.

There are three reasons for this:

1. Heterogeneity among individual responses

There will be a spectrum of fitness amongst individuals within the population determined by both genetic (see below) and environmental effects. Recent evidence from a range of mammals suggests that this is genetically determined to a large extent. As food availability declines for SSL, those individuals at to low end of the spectrum of fitness will be affected before those at the higher end probably mainly because of competitive exclusion. Since, by definition, the fittest individuals will be those contributing most to future generations, it would likely take an acute decline if food before any overall effect of food was evident in the population trajectory and the capacity for the population to sustain itself.

Thus, one would not necessarily expect to observe an overall lowering of reproductive condition, body condition or “health” as a result of changes in the environment. Depending

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<sup>7</sup> DeMaster, D. and S. Atkinson (eds.). 2002. Steller sea lion decline: Is it food II. Alaska Sea Grant College Program Report No. AK-SG-02-02. Fairbanks: University of Alaska.

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upon the functional relationship between food and survival, the population sample may actually show an increase in some of these indices because, the elimination of animals in poor condition from the population, will cause an increase in the mean of the indices and a decline in the observed variance.

Therefore, observing the effects of reduced food availability is extremely difficult and may be impossible in the circumstances presented by the SSL.

## 2. Non-linear functional relationships

A functional relationship in this case describes the relationship between food availability and the vital rates. Further detail is provided in Box 1 below. Similar functional relationships will exist for indices such as body condition, health and reproductive condition. Mori & Boyd (2004) have provided an illustration of the kind of behavioural strategies that exist to sustain food intake rate over a broad range of food availabilities. Similar physiological strategies are also known.

An obvious effect of these types of relationships, which appear to be quite general for marine predators, is that food availability can change substantially without any measurable effect upon the index of interest. Consequently, for food to have an effect, availability needs to decline to a very low level. Examples showing what happens in these circumstances are available both from the Antarctic and from the North Sea. Interestingly, in both these cases, the commercial fishery appeared to be governing by the same economics that drove the natural availability of food for the predators partly because of the presence of management control rules but also for commercial reasons.

This means that the effects of small, chronic or subtle changes in food availability are very difficult to observe through predator indices.

*Box 1***Sensitivity to changes in food supply.**

Most seabirds and marine mammals have demographics characterised by high longevity and slow reproduction. Using a state-based matrix projection of populations, it is possible to assess the consequences of non-linear relationships between vital rates and food density. Based upon the empirical observations (Phillips et al. 1996; Boyd and Murray 2001), the type of functional relationships we can expect are illustrated in Figure 1. In this case, the model predator is assumed to breed annually and has 3 life history stages; *year 1*, *juvenile* and *adult*. The functional relationships assigned by Curves 1 and 2 are the probabilities that individuals transit from *year 1* to *juvenile* and from *juvenile* to *adult* respectively. A similar function to that of Curve 2 was used to describe the annual survival of adults. Each of these transition probabilities was calculated using Equation 2 with  $P = \text{food density}$ . Similarly the fecundity rate was calculated using Equation 1 and this is shown as Curve 3.

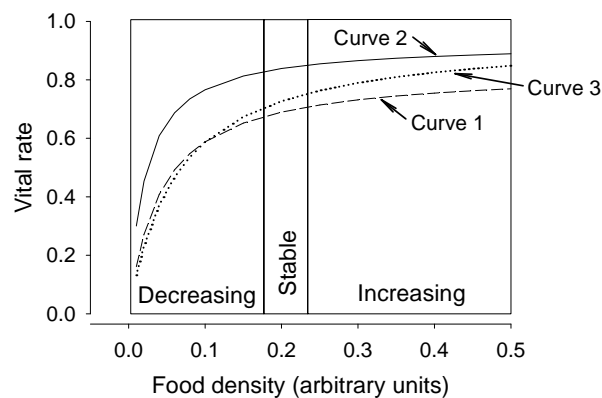


Figure 1. Modelled variation in vital rates in relation to food density. Curve 1 (dashed line) is the transition probability for *year 1* to *juvenile* stages; Curve 2 (solid line) is the transition probability for *juvenile* to *adult* stages; Curve 3 (dotted line) is the fecundity rate. Food densities at which the population is decreasing, increasing and stable are shown.

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### 3. The “silver spoon effect”

This effect is similar to the more generic effect of heterogeneity of individual responses described above but it assumes that inherited characteristics are highly influential in determining the success of individuals.

Throughout the RP, including the PVA, there is an assumption of homogeneity of quality amongst individuals. This affects many aspects of the current data and the analyses of vulnerabilities. In reality, most of the future viability of the population is likely to be down to a rather small proportion of the population including particularly successful individuals, especially females. There is increasing evidence from a variety of long-lived predators that the population is carried on the shoulders of these few individuals and that their success is partly inherited. There is evidence that this may also be true for pinnipeds since early survival appears to be strongly related to some indices of genetic heterozygosity.

Consequently, when undertaking the historical analyses of animals lost from the population it is important to consider that animals removed by direct killing did not distinguish between these high quality individuals. Conversely, animals removed through reduced nutrition or predation will have been segregated to some extent into those born with the silver spoon and the losers, which may always have been killer whale food. The silver spoon effect will lead to heterogeneity in individual vulnerabilities that will have consequences for the population dynamics with some parallels to those of a metapopulation structure in that, it would tend to lend stability to the population. Thus, in these circumstances nutritional condition would have to decline a very long way before a population-level effect, at least in terms of its fundamental capacity for increase, would have been observed. This is because there would be a strongly non-linear response to food availability.

The RP appears to be suggesting that deliberate killing of sea lions was (and may still be) much greater in the past than is shown for current records. This seems a very sensible suggestion and is a conceptual leap forward from the official position being taken by NMFS during the mid-1990s. Indeed, the old Recovery Plan contained almost no reference to this as a source of mortality. One of the greatest problems with deliberate killing, especially when it happens at the breeding colony, is that it is likely to have had a significant impact on the silver spoon females within the population. The long-term impact of this for the intrinsic capacity for population increase are possibly substantial and long-lasting.

The Plan still places a lot of emphasis upon samples collected in the 1970s and 1980s to suggest that there was evidence of nutritional stress. I remain doubtful about the extent to which emphasis has been placed upon this evidence both in this Plan and in previous documents. The samples in question were not collected for the purpose to which they have been put in recent years and the interpretation of their results rarely accounts for the likely biases that exist within these samples. They can be no more than indicative. The reproductive data are particularly susceptible to bias caused by changes in sampling conditions. Although the results may be consistent with the concept of nutritional stress, they are also consistent with a number of other explanations and, in anycase, it is unlikely that the population as a

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whole would have experienced acute nutritional stress – intra-specific competition would probably have quickly separated the winners from the losers and the most likely outcome would have been a greater variance in condition and reproductive success.

## Summary

Thus, there are serious doubts about whether the nutritional stress hypothesis is testable. The common assumptions surrounding the kind of effects that one might observe (also see the section on Vital Rates, section 2.b.i) are unlikely to hold. Nutritional stress will not be evident across all individuals of a cohort and co-variance between stressed individuals and low survival mean that, by definition, the evidence for nutritional stress is being removed. Moreover, it is likely that those individuals that are most likely to suffer under chronic nutritional stress will be those that are least likely to be good competitors and, consequently, their removal may have comparatively little influence upon the population dynamics.

Alternative approaches to testing the nutritional stress hypothesis have focussed upon examining animals in captivity when fed known diets under controlled conditions. This takes a “first-principles” approach by attempting to build predictive capacity from understanding different components of the biology of the SSL in isolation. There are clearly problems extrapolating from these captive situations into wild individuals. Although these experiments have been useful, in general, none has seriously shed light upon the relative merits of the nutritional stress hypothesis. Their utility has come mainly in terms of their capacity to show that there is no single, simple solution and that, the more we delve into the biology, the more we understand how little capacity we have to provide management solutions built from a first-principles approach.

Boyd, I.L. and Murray, A.W.A. (2001). Monitoring a marine ecosystem using responses of upper trophic level predators. *Journal of Animal Ecology* 70, 747-760.

Mori, Y and Boyd, I.L. (2004) The behavioural basis for non-linear functional responses: the case of the Antarctic fur seal. *Ecology* 85, 398-410.

Phillips, R.A., Caldow, R.W.G. and Furness, R.W. (1996) The influence of food availability on the breeding effort and reproductive success of Arctic skuas *Stercorarius parasiticus*. *Ibis* 138, 410-419.

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## Appendix IV

### CURRICULUM VITAE (brief)

**Ian L. Boyd**

#### QUALIFICATIONS:

1979 B.Sc., (First Class Honours) in Zoology, University of Aberdeen.

1982 Ph.D., Cambridge University (St John's College).

1995 D.Sc., University of Aberdeen.

**PRINCIPAL DISCIPLINE:** Vertebrate ecology

#### CAREER SINCE GRADUATING:

2006	Marsh Award for Freshwater and Marine Conservation, Zoological Society of London
2006 – present	Chief Executive Officer, SMRU Ltd
2006 – present	Editor-in Chief, Journal of Zoology
2002	Fellow of the Royal Society of Edinburgh
2001 - present	Professor in Biology, University of St Andrews
2001 - present	Director, Natural Environment Research Council, Sea Mammal Research Unit
1999 - 2006	Editor, Journal of Zoology
1998 - 2001	Science Programme Director , British Antarctic Survey
1998	Scientific Medal, Zoological Society of London
1997	Honorary Professorship, University of Birmingham
1995	Bruce Medal, Royal Society of Edinburgh.
1995	DSc, University of Aberdeen.
1995	Antarctic Service Medal of the United States.

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1995 - 1997	Honorary Senior Lectureship, University of Birmingham.
1993 - 1995	Honorary Lectureship, University of Birmingham.
1987 - 2001	Senior Principal Scientific Officer, British Antarctic Survey. Promotion UG7 1991. Merit Promotion UG6 1996.
1982 - 1987	Physiological ecologist, Institute of Terrestrial Ecology, Monks Wood, Huntingdon. Promotion HSO to SSO 1986.
1982	PhD, Cambridge University
1980	Churchill Fellowship
1979 - 1982	NERC Research Studentship at Department of Anatomy, Cambridge University and Sea Mammal Research Unit (NERC)
1979	B.Sc. (First Class Honours) in Zoology, University of Aberdeen

### **MEMBERSHIP OF NATIONAL/INTERNATIONAL COMMITTEES**

BOARD OF DIRECTORS, NERC MARINE SCIENCE, Member 2005 - present  
MEMBER OF MoD/NERC CO-OPERATIVE ARRANGEMENTS FOR RESEARCH IN  
OCEAN SCIENCE – CAROS 2003 –present  
STEERING COMMITTEE, NATIONAL CENTRE FOR STATISTICAL ECOLOGY 2005-  
present  
CHAIR, SCOTTISH MARINE SCIENCE POOLING INITIATIVE, 2005-present  
CHAIR, INTERNATIONAL WORKING GROUP ON MARINE MAMMALS AND NOISE,  
2005-present  
**ROYAL SOCIETY OF EDINBURGH ENQUIRY INTO THE FUTURE OF THE  
SCOTTISH FISHING INDUSTRY, 2003**  
SCOTTISH SEALS WORKING GROUP, 2002-present  
NORTH ATLANTIC FISHERIES ORGANIZATION / ICES HARP SEAL WORKING  
GROUP  
Member 2000-present  
Natural Environment Research Council MARINE SCIENCES PEER REVIEW PANEL  
Member 1999-2001  
Natural Environment Research Council ANTARCTIC SCIENCE PEER REVIEW PANEL  
Member 2002-present  
SCIENTIFIC COMMITTEE FOR ANTARCTIC RESEARCH (SCAR):  
Group of Specialists on Seals, 1990 - 2002  
Secretary, Steering Committee for the Antarctic Pack Ice Seals Research  
Programme, 1994 - 1999.  
INTERNATIONAL UNION FOR THE CONSERVATION OF NATURE (IUCN):  
Seals Specialist Group, 1990 - present.  
UK SPECIAL COMMITTEE ON SEALS (SCOS):  
1993 - present  
SOCIETY FOR MARINE MAMMALOGY:  
Member of the board of management, 1992-1994.  
COMMISSION FOR THE CONSERVATION OF ANTARCTIC MARINE LIVING  
RESOURCES:  
Member of the Working Group on Ecosystem Monitoring and Management  
1993 - 2001.  
ICES WORKING GROUP ON BALTIC SEALS  
Member 1982-1988.

### **DIRECTORSHIPS/CHARITY WORK**

SCOTTISH ASSOCIATION FOR MARINE SCIENCE:  
Member of Council, 2003-present.

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HEBRIDEAN TRUST:

Member of Council of Management, 1992 - present.

SEAMARK TRUST:

Member of Council of Management, 1993 - present.

REPRODUCTION RESEARCH INFORMATION SERVICES LIMITED

Member of Board of Management, 1984 - 1989.

## **PUBLICATIONS**

Author of: 8 books, over 130 peer-reviewed publications in the field of vertebrate ecology.