

## **Response to Comments by Dr Amélie Augé on the draft report.**

Generally, the comments suggest that more attention should have been given to Augé's PhD thesis which examined a number of aspects of NZ sea lion biology at the recolonized Otago breeding colony. I was made aware of this thesis only after the first draft of my report had been reviewed by DeepWater and DOC. New material has been coming in on a rather frequent basis making it difficult to decide at what point in time I could not consider new studies. Although a hard cutoff was not adopted, generally material that has become available after June 2011 has not be considered in detail.

I have attempted to address the minor errors of fact or issues that required clarification that were identified in her review of the draft, unless otherwise stated below. Her comments were not numbered; therefore, I have addressed them sequentially point by point.

P20: growth rates – I have incorporated the reference to growth rates from the Otago colony, and also made reference to those on Campbell Island which are similar to Otago. Growth rates do vary, apparently not simply related to whether they are taken from remnant vs recolonized colonies.

P20 productivity – I have included several new paragraphs noting the variability in the structure and functioning of pelagic ecosystem used by NZ sea lions.

P22: remnant colonies – 13 females where studied, but only 8 of these were adults as indicated in my report. Attention was given to the adults for comparison with the Auckland Islands females.

P22: core range – I agree that Otago is located in what we know as the historical range of the species. Change made.

P22: behavioural differences – I have included reference to the 2010 data. However, my interpretation differs suggesting that 4 of the females showed similar foraging patterns and 2 did not. Nevertheless, I do not believe this captured the point I was making – namely that environmental variability might well have a strong influence on the foraging behaviour of females. Although all or most females have been studied at this colony, the number of females is too small to have much confidence that we understanding how they might respond to environmental variability. Also, there has been no attempt to characterize interannual variability in prey so the question remains relevant but unanswered.

P22: more on sample size – I would have to disagree here. Yes, the entire colony has been studied, but not all in one year. Thus inference is still needed and small sample size is still relevant to our understanding. More importantly, however, is the issue of how to interpret what we see today at Otago relative to females in

larger and denser colonies in remnant habitat. Are females at Otago doing well simply because there are only 8 or is the habitat really that much better? I'm not convinced we know.

P22: prey abundance – I'm afraid I have to disagree again. Part of the disagreement may concern how one identifies marginal habitat. In the case of sea lions I do not think it is straight forward. That is, we are not identifying something that limits their use of the habitat, such as a temperature or some chemical property that simply make the area "marginally suitable" for the species. We seem to imagine that it is characteristics of the available prey field – perhaps diversity, abundance, distribution and the variance in these properties - but these are poorly known.

Other points -

Table 1 is correct as it stands.

The Augé et al. (2011) paper arrived too late to be considered.

The point about Table 3 is that expected direction of change unfortunately tells us little about causation.

The report referred to on winter foraging behaviour of females was consulted but there was very little detail on how the analysis was done or even where the females had been tagged. On this basis, it was not used.

P24: In my report, I stated that "*Apart from the time series of pup production estimates at the Auckland Islands, there are few time series with which to test competing hypotheses about causes of the decline.*" Augé points to Figure 5 in Meynier et al. (2010) as a long time series (actually just 6 years) of fishery catches versus sea lion diet and contends that this figure clearly shows "that they were related, especially that the collapse in hoki catches around the Auckland Islands (likely collapse of stocks) matched the decrease of hoki in the diet of sea lions. This should be a real clue that resource competition exists and, clearly, the author has not described this."

As I noted in my report, Meynier et al. (2010) undertook a rather careful and comprehensive analysis of a small number of blubber samples of NZ sea lions taken as bycatch from the arrow squid fishery. Nevertheless, I believe that caution is needed in using these estimates of the diet in a quantitative analysis. First, they are taken as bycatch and therefore they are unlikely representative of the foraging population. Second, annual estimates are based on just 8-12 individuals, and third, only a small sample of prey was used to characterize the fatty acid composition of each prey species. The effect of this can be seen in the large differences in diet composition when estimates are based on prey means vs. individual prey. I do not have much confidence that these are reliable

estimates of the interannual composition of the diet and therefore any quantitative analysis using those data must be regarded as tentative. Finally, interannual variation in diet, even if correlated with catch data, is not a reasonable basis for testing a resource competition hypothesis.

A marked Final Report of the Meynier et al. (dated 2010) manuscript on the population energetics of NZ sea lions was seen, but was not used as there were a number of comments and queries that suggested the paper could be not be considered complete.

Don Bowen