



Meeting: Conservation Services Programme Technical Working Group Meeting
Date: 20 May 2014
Time: 9:30 am– 2:30 pm
Place: Level 4 Conference Room, Conservation House, Wellington.
Chair: Ian Angus (ph: 04-471-3081; email: iangus@doc.govt.nz)

Attendance: Bruce Robertson, Stefan Meyer (Otago University - Skype), Darryl MacKenzie (Proteus), Jim Roberts, Kyle Morrison, Ian Doonan, (NIWA), Simon Childerhouse (Blue Planet Marine), Biz Bell (WMIL), Karen Baird (Forest & Bird and Bird Life International), Chris Gaskin (Forest & Bird), Janice Molloy, Kirsten Crawford (Southern Seabirds), Martin Cawthorn (Cawthorn & Associates), Rob Mattlin (MWR), David Middleton (Seafood NZ), Richard Wells (FINZ and DWG), George Clement (DWG), Milena Palka (WWF), Edward Abraham (Dragonfly Science), Michelle Beritzhoff-Law, Nathan Walker (MPI), Ian Angus, Brent Beaven, Sarah Wilson, Laura Boren, Katie Clemens, Kris Ramm, Igor Debski (DOC)

Presentations:

1. POP2012-02 New Zealand sea lion – demographic assessment of the cause of decline at the Auckland Islands Jim Roberts (NIWA)

There was discussion about why predation was not looked at, noting that immature animals are more likely to be affected as they are often the target of predators. JR mentioned that different age classes would be vulnerable to different effects, and that it would be likely to only affect survival so would act in the same way as direct fishery impacts.

RW – Was nursing of pups 2-3 years old seen?

JR – This is a phenomenon seen in declining populations and seen in a wide variety of species.

MBL – Have you seen any evidence that when a female's condition decreases, she prioritises her own well being over the nursing of her pup?

JR – I'm not sure. When we go through the correlative assessment, many females only reproduce over five to six year period, many stop breeding by the time they reach 15 years of age, and then their condition gets better. They do exhibit quite strong site fidelity to feeding grounds, they find a place that works for them and they tend to stick with it.

RW – But at what point does the animal determine when it's going to save itself?

JR – There is a very large increase in food intake when they're lactating, especially in mid-late lactation, as well as chance feeding. In controlled studies they are given more food, but in the wild they would have to potentially alter their feeding habits.

I Debski – Do your hypothetical models allow for drivers at different levels, for example, if the pup's in poor condition, it may be more susceptible to disease?

JR – I haven't looked at it, but it would be good for looking at pup mortality. From an initial look at the data it seems that larger animals are the ones getting the diagnosis of disease. A lit review should be done as part of the process.

BR – Did you look at the relationship between ocean climate and fisheries abundance?

JR – I did look at climate against fishery catch rates, but not catches themselves. Relative abundance of species that were caught and quite low catch rates is always an argument of how catch per unit effort relates to abundance. A potential future assessment should look at historic catches and fish abundance.

There was discussion on the foraging strategies of the New Zealand sea lions and whether they were generalist feeders, exhibited individual-specific foraging strategies or targeted specific prey. JR replied that we have limited information on this subject and all that we have comes from scat and regurgitates analysis (potentially a biased measure) and we can't be sure which individual left them and that he suspects different individuals have different foraging strategies, as Louise Chilvers has mentioned in the past (mesopelagic vs. benthic feeders).

D Middleton pointed out that there should be two measures of condition in the models (maternal body condition and pup condition), and pup growth should just be used as a measure of condition.

D MacKenzie – does resighted pups mean those that were resighted that year?

JR – Yes.

I Debski – Was there any difference in the tag loss between the early 1990s and later years due to use of different tags?

JR – I haven't looked at that; different tags were used in different years.

There was discussion surrounding the timing of birth dates and the assumed use of ages vs. dates (e.g. three weeks old vs. 10 Jan).

BR – Before 2010 data was collected on birth dates, after 2010, there wasn't a lot of work done in during the month of December.

SC – There is remarkable consistency of mean pupping date from year to year, there isn't a lot of variation.

KM enquired into the rationale behind not accounting for tag loss. JR explained that with the exception of pup survival, tag loss wouldn't have had much effect, so he instead used an index of survival that would give the same information.

There was discussion around the success of PIT tags and whether they were always able to be read and how many failed. SC mentioned that the success rate is well over 90%, and that the veterinarians feel that once the tags have been inserted they will stay in the body, however they can migrate within the body. There was also discussion around the issues that Louise Chilvers experienced one year with scanners that weren't as functional and that the process itself can be difficult as the animals move around considerably while being scanned.

There was discussion around the average age of resighted individuals every year, the age at first pupping and mean age of puppers, and the fact that an individual has a higher probability of pupping if they pupped in the previous year, and that there are some individuals that aren't pupping every year.

There was discussion around the fact that something occurred in the 2004-2005 breeding season that seems to have massively affected pupping rate.

There was discussion around the definition of stillborn and disease related mortality, including the fact that starvation was a prominent cause of mortality for two years with no diagnosis of bacterial infection, and then all of a sudden the incidence of bacterial infection increased sharply the following year.

EA – When looking at time series, if you treat as them individual points you can overestimate degrees of freedom and should be considered and interpreted in results.

D MacKenzie – You make a lot of comparisons, multiple comparison adjustments, p values, etc. Could use Bonferonni corrections for example.

JR – Multivariate analyses might be more appropriate here.

RW – I'm concerned that limited information on diet is resulting in assumptions around fish stocks presence/absence and should be treated with caution.

There was discussion about whether milk lipid concentration is a good indicator of foraging distance, and research on that subject currently underway.

NW – Disease related mortality is bacterial infection?

JR – Yes, bacterial infection, not hookworm.

EA – Do you know why the 8-10 weeks data on the timing of pup disease mortality shows a different pattern to that seen in 11-13 weeks?

JR – We don't have much data for the period of 11-13 weeks. As to why it would be different post 8 weeks, the change in milk quality is related to foraging change, so it looks they're feeding further offshore. Ecologically, things are quite different there, maybe it's because they don't go far initially because pups too weak, and then venture further out when pups get stronger, but that part is a work in progress.

KM – Have you looked at the role for increasing pup mobility with age potentially increasing disease transmission?

JR – I'm not the right person to ask, Wendi Roe is looking at that kind of information. However, they are social breeding animals where pups are often all sitting in piles. Disease transfer rates could be high as a result.

MC – The onset of pups grouping together in pods would be a good marker for that (5-6 days).

JR – Disease mortality is low then but it doesn't mean that they aren't getting infected

MBL – do you think it would be worth adding to the list of important data to try and collect the possibility of seeing if we can do a bit more aging of the animals that don't have tags, to clear up tag loss issues?

JR – I think it would be extremely useful, but the problem is that it's quite invasive. There is a 100% survival rate, but you need to anaesthetise the female and pull the teeth out.

SC – We wouldn't have to catch the ones that are already tagged as we'd know how old they were, so we'd only have to do the ones that were never tagged, or had lost their tag.

JR – What proportion of the colony would you need to do?

SC – A reasonably high proportion, about a 1/3, although some of them might have a chip.

JR – You'd probably only need to do another 30 or so individuals per site in order to gain a lot of useful data from it.

KM – Increased research of the growing population at Campbell Island could help us further understand the population at the Auckland Islands.

JR – SC looked at the Campbell Island population in 2003, there's also Louise's research, and data on the population on the Otago peninsula.

There was discussion on uncertainty in estimates, autocorrelation, external correlation assessments, and building in sampling covariance.

RW – Research needs should be called research options, as it's all about collecting more data. To what extent do you feel, beyond what you've summarised today, that you could get more useful information out of the existing data sets with more money, time, etc, if those are lacking?

JR – The changes in pup mortality and diet in Jan/Feb data which we haven't really looked at. There's definitely still work you can do there, but only until the end of Feb, and I believe to really understand what is happening, you'd need to look into April, etc. We'd need to look at other sea lion species and see if we need to collect more data.

I Debski – There will be a period of two weeks for submission of further comments on this project. A draft final report, taking into consideration feedback received and further analysis will be presented to the TWG at a later date. Following that a final report will be produced and posted online by October 2014.

Note: further review comments on this project by DI MacKenzie are appended to these minutes

2. POP2013-04 Black petrel population project

Biz Bell (WMIL)

There was discussion around whether or not new burrows were being added to the study and the lifetime of a burrow. As BB explained, 100 burrows have been studied during the entire time period, and the occasional burrow has been added to the study, in particular all new burrows found within the census grid are added to the study. And burrows found containing eggs that are located outside the census grid will also be added to the study.

D Middleton – Do you know the exact state (i.e. active vs. inactive) of all burrows in all census grids?

BB – We check, when possible, by seabird detector dog to make sure that inactive burrows truly are inactive.

MBL – Are you worried about adults losing bands?

BB – The only time a bird loses a band is when it gets its leg damaged. A few birds in the colony only have one leg, but we've never lost a band any other way.

MP – Is there anyone else outside of New Zealand that could recognise banded petrels and could get in touch with someone here?

BB – Birds have caught and reported from South America, one where a bird had been released alive and band number recorded and two from dead birds have been returned.

I Debski – All bands have a return address on them as well.

MP – But they wouldn't be landing anywhere else?

BB – No, they only breed here.

I Debski – How did the GPS devices fair? Why do you suppose you got partial tracks?

BB – They performed well. I suspect that the partial tracks were due to the fact that the birds had either left the colony, or the devices were set on a timer that turned while they were on out of sea and then came back, we likely just misjudged when we thought they'd be flying away from the colony.

I Debski – Were there any issues with communication with the devices?

BB – No, they worked really well.

I Debski – Did you construct your own housings?

BB – Yes, while we were up at the colony.

There was discussion on the limits of the Hauraki Gulf area and how long the black petrels were within the marine park area shown by the data loggers.

I Debski – did the TDR devices work well?

BB – Generally they performed well. One didn't work, and two devices had batteries that died, so we sent them off to the manufacturer to recover the data.

There was discussion around the record that indicated that a male black petrel that 1000+ times and whether or not to remove the recording. BB will check the TDR device to check for possible malfunction and will amend the data to reflect it.

NW – What are the chances that the birds aren't going somewhere else to breed?

BB – The majority of black petrels are located within 35 hectares of the summit on Great Barrier Island. There are some scattered on the other peaks of Great Barrier Island and roughly 100 pairs on Little Barrier Island, but there are none found on other islands in that area. The

problem is if the birds are just off the tracks, because particularly during night-time surveying we're limited to the tracks. Our juvenile recruitment and survival estimates are uncertain as we're not getting the recaptures. We need more people searching, etc, to make it possible.

JM – So are you not getting the birds back or not able to identify that they've come back?

BB – We would have expected to have seen more birds returned.

JM – You can tell age from autopsy, are they juveniles?

BB – We haven't had very many, two previously, and two this season, one male, and one very young adult, never bred, (pre-breeder) but at adult age – but interestingly the captures appear biased to males.

RW – What sort of time period do you have when the bird get sighted, disappears, and then gets resighted?

BB – The longest we've had is 18 years between initial sighting and resighting, more often though it's just a few seasons.

There was discussion about how new birds are assigned an "age", for example if it is certain that they are breeding, then they're listed as being 5 years of age.

RW – Does capture mean recapture or resight?

EA – Yes, capture means resight, I'll standardise the language to only use resight.

RW – It looks like the resight probability for an adult is good, what's the percentage?

EA – Roughly 70%, it's high for adults but low for chicks.

D Middleton – Do you assume that all the tags are read accurately all the time?

BB – Yes, data sheets are checked every night, and if there's a number discrepancy then we'll go out and check the band that night.

There was some discussion about the models presented and why annual survival fixed does better than annual resight fixed.

NW – Do you record un-banded birds in burrows?

BB – Yes, the bird gets banded and then that gets recorded.

NW – It would be interesting to look at that.

BB – Roughly 150+ get banded each year.

NW – Roughly how old?

BB – They're automatically given age five as they're of breeding age and have not been banded previously.

EA – What do you know about their breeding status?

BB – If there's an egg then they're classified as a breeder; if there's no egg then they're a non-breeder; if they're caught early on then they are perhaps an interloper.

RW – Anything that gets caught in study area gets banded?

BB – We band everything because a bird that gets banded might just get caught in random survey tracks we do.

RW – You're missing quite a lot of juvenile returns. In random transects, do you tend to see most birds around your study area or further away? I'd like to see a plot of burrows, etc as these birds might be further away.

BB – There are different areas within the colony that birds prefer, but the transects provide good representation of the area.

3. INT-2013-02. Identification of seabirds captures in New Zealand fisheries

Biz Bell (WMIL)

KR – Have you noticed patterns over trips (for incorrectly identified birds)?

BB – I don't know off the top of my head, but I can look it up

KB – I've been thinking about the possibility that the black petrel's diet could have changed over time, and so it would be good to access the birds that were killed to undertake stable isotope analysis, but then I realised that we don't keep black petrels, so I was wondering about whether or not we should keep the highly endangered species that are returned for autopsy? Also are most are adult birds?

BB – Yes, one was three years old, technically a pre-breeder, but all others were adults.

KB – We also might need to have a look at what's happening in South America and overseas for the pre-breeders and juveniles. My final comment relates to the release of live birds. I was looking at those pictures of wet and bedraggled birds; is there a protocol for putting them somewhere warm to dry off before release that the observers follow.

KR – Yes, observers are informed on the proper procedure to follow in those circumstances.

KB – If we want to increase the probability of those birds surviving, then that might require some more work.

KR – Yes, we can look into that.

I Debski – Relating to KB's suggestion about keeping the highly endangered species, we could do that, but would the current permits that you hold allow for that, and would you have the storage space?

BB – Yes, we do have storage room for that.

There was discussion about the fact that three birds were autopsied when they potentially weren't supposed to (i.e. they were supposed to be photographed only), and one of these (a short-tailed shearwater) was only correctly identified through autopsy (required measurements).

There was discussion on the process for feeding the autopsy data into the COD database and the status of the autopsy database.

Other CSP Business

Further written comments on any of the material presented, or the draft minutes, will be accepted by email to csp@doc.govt.nz until 10 June 2014.

Review of Sea Lion Demographic Modelling

DI MacKenzie

Proteus Wildlife Research Consultants

General Comments

There has clearly been a substantial amount of work conducted here, and JR et al have come up with some interesting results. There is a lack of detail on some aspects of the work in the reports that I have reviewed (last 3 presented to CSP TWG), although some of that detail may reside in other reports I haven't seen in which case I presume it will be covered in the final report. I found that a lot of important detail was provided in the presentations, but not covered in the reports.

Overall I think this a good start but think there are some important areas that still need work. In particular I would have expected a more comprehensive analysis of potential correlates with the demographic parameters; ideally done within the demographic analysis itself, or at least a regression-type analysis that allows for uncertainty in the demographic estimates so that effect sizes can be estimated. This is particularly important given that one of the purposes of this contract was to identify potential causes for the observed decline. There can be a statistically significant effect that is too small to explain a decline, or an insignificant effect that could potentially explain the decline. Simple correlations of the point estimates is pretty rudimentary, and conducting a large number of significance tests on the correlations raises the possibility of finding spurious results if the number of comparisons being made are not accounted for.

Stronger justification needs to be given for looking for changes in the nature of correlations before and after 2005. The logic for it is not clear, particularly given that the decline is thought to have begun in the late 1990's/early 2000's.

As an editorial point, I noted that often the captions for figures and tables could have been more descriptive as often they did not fully describe what was being presented. It's typically expected that figures and tables should be standalone and require minimal reference back to the text.

Below I've made a number of specific comments about the work. I think that most of them are important and need careful consideration. As is the nature of a review, many of them are critical but as I noted above, I certainly think they are on the right track.

Demographic Modelling Points

- Does the sequence in which parameters are considered have an effect during the optimisation process?
- Were 'phantom' tags added for all years? I've only been able to find references to the number added from 1998 onwards. In some years the number of added phantom tags is quite large relative to the total number banded. Were any added for the early 1990's? If so, the number added needs to be documented. If not, but they were for latter years, the survival (and possibly other parameters) are not directly comparable.
- Why was MCMC used to assess the degree of uncertainty? Should be able to obtain variance-covariance matrix using standard numerical techniques and maximum likelihood.
- What priors been used for MCMC approach?
- Using MCMC may mask unidentifiable or confounded parameters. For example, it's well known that in regular CJS mark-recapture models the final survival and capture probabilities are confounded and are not separately estimable when both are allowed to vary in time. This can usually be identified in difficulties obtaining a valid variance-covariance matrix if some parameters are unidentifiable or confounded. Has any checking been done for correlation between posterior distributions for pairs of parameters? This may indicate some potential confounding (but could also just be sampling variation).

- In statistical jargon, MacKenzie (2011) and Chilvers and MacKenzie (2010) survival estimates used a model with *additive* age and year effects, so year effects are the same for all age groups. You're using an *interaction* model where the year effects are different for all age groups.
- Can Seabird allow common parameters in different partitions? May speed up tag-loss models if not trying to estimate separate survival rates.
- Cohort effects could be included as a covariate in the analysis rather than treating as separate analysis or partition, if Seabird can incorporate covariates (most similar mark-recapture packages can, or shouldn't be too hard to incorporate).
- Survival to age 2 looks suspiciously high for early 1990's
- Maturation 1 parameter also looks high for late 1990's. Your saying about 40% (on average for this period) of aged 4 females pupped. Simon's age distribution data suggests about 1.25% of breeders are aged 4 for the same period (on average). For this to hold it would suggest that the number of breeders is 32 times greater than the total number of age-4 females (i.e., from $0.4 * \# \text{ aged 4 females} = 0.0125 * \# \text{ breeding females}$). Whereas if that age-4 maturation parameter is more like 10-15%, that would imply the number of breeders is 8-12 times greater than the total number of age-4 females. The lower range of values seems more conceivable.
- In general there were some issues with the data collected in the earlier 1990's (based on previous discussions with Louise Chilvers) and I've typically excluded them from analyses I've conducted in the past. I'd be pretty cautious about any conclusions that are heavily influenced by estimates from the early 1990's. There may also be an issue with the use of phantom tags, as noted above.
- Limits in Appendix A for pupping probabilities appear to be reversed?
- Mean age of puppers is an output of a model rather than direct observation, but it is not clear how that has been calculated. More detail is needed. If some initial age distribution is assumed for breeders the resulting time series could be sensitive to those initial assumptions. That is, is that a real pattern or just the consequence of a model and the practice of only tagging pups (see below point).
- There could also be the potential for the age of the tagged population having an influence on estimated mean age of puppers as this is changing over time. For example, in 1998 the only tagged breeders would be aged 5-8 from the early 1990's. By 2002 this group would now be between 9-12 (and there's likely fewer of them), and the first of the females tagged as pups in 1998 would be starting to breed. The introduction of the more recently tagged females to the breeding population is going to cause the mean age of *tagged* breeders to shift. Over the next few years as more younger, tagged females start breeding, and the older tagged females die off, is going to cause that mean age of *tagged* breeders to continue to shift more towards the mean age for the newer groups, although it is going to gradually increase each year. It is probably only by about 2012 that there will be a good cross-section of tagged individuals across the age distribution for breeders, prior to that there's going to be some significant age classes which do not contain any tagged females. Plotting the mean age of tagged known-breeders might provide some insight.

Correlative Analysis Points

- Little justification is given for using a break point at 2004/2005 for some correlative analyses. JR made the comment at the presentation that it was based on observed patterns for a number of variables. Without a sound a priori reason, it may be a circular argument, i.e. if a number of things looked to be different before/after 2005, when the time series is separated on that basis, we find them to be different. I'm not saying there isn't a real change that has occurred between 2004/2005, but without an a priori reason, there certainly needs to be a sound, logical argument put forward to suggest why things may have changed so markedly.
- However, given the decline in the population is thought to have begun in the late 1990's/early 2000's, it would be reasonable to expect that any change in the correlation patterns would have happened prior to the decline, not after it's been happening for some time. Maybe I've missed something, but I don't follow the logic of looking for a change in correlation patterns after the decline has already started. The logical time to look for a change would be pre/post the start of the decline (for which there is little data).
- Results of correlation assessments might be better presented in a table so readers can see exactly how many, and what, correlations were assessed (regardless of whether they are 'significant'). Currently it's unclear from the report exactly what correlations were looked at.
- There are a large number of correlations being assessed. Even if every was uncorrelated, we'd expect a p-value to be <0.05 for 1 comparison out of 20. Therefore, when conducting a large number of tests we'd expect some of them to be 'significant' just by random chance. This is a well known issue of conducting multiple comparisons and there have been various suggestions over the years to account for it (e.g., Bonferroni adjustments, Fisher's combined p-value, etc). This needs to be accounted for somehow.
- In general I'd argue that statistical significance isn't as informative as estimating the size of the effect, and assessing whether that effect is big enough to contribute to the decline. EA made this point during the presentation. Regression-based methods may be preferable for this rather than simple correlation statistics.
- Performing correlation assessments on point estimates ignores the uncertainty in those estimates, and sometimes the error in both parameters. As the error is quite large in some cases this could have a substantial impact. It's basically a problem in measurement error (potentially in both the y and x variable) so looking at this literature should be able to provide some guidance. It is well known that ignoring it can cause bias, and lead to overly precise results (SE's or p-values too small).
- Some of the correlation analyses with demographic parameters could be investigated by including the potential correlate as a covariate in the demographic modelling. Potential correlations between demographic parameters could also be built into a model.
- Demographic parameters are estimates from a specific model. A different model could result in different estimates and different types of correlations. Model estimates may also not accurately reflect the actual degree of variation in the data. Would be much better if such assessments are built into the model directly (e.g. as a covariate above, or include a correlation term between demographic parameters in the demographic model and see if it's estimated as close to 0), although I recognise there could be problems there when there's a mismatch in the time series for the demographic data versus the potential correlate.

- There might be couple of short-cuts you could take rather than completely reanalysing the demographic data using the output of the MCMC analysis, depending on what software you have and how familiar you are with it. The basic idea would be to use the joint posteriors from the current analysis as an input (not sure whether I'd define them as observations or priors), setup the demographic model with the additional structure of interest (including priors on the additional terms), then let it run. Essentially the idea is to analyse the joint posterior distributions. Key is that you want to be using the fully joint posteriors of all parameters to make sure all the correlations and covariances of the original analysis are maintained. Of course, by the time this as all been figured out it may be faster to just reanalyse the original data.
- There are also non-MCMC solutions; performing secondary analyses on estimated parameters while properly accounting for uncertainties.