Comments on the Weller *et al.* Robben Island penguin model simulations, in particular as regards the impact on penguins of fishing restrictions around the island

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Summary

Some initial comments are provided on the Weller *et al.* model of the penguin population at Robben Island. Some general concerns are raised as regards the approach as a whole, and the absence of a good fit to the available data. More specific reservations are raised about the quantification of the relationship developed relating penguin egg and chick survival rates to fish abundance, and in particular the associated assumption that a time series of catches provides a reliable index of that abundance. Further comments await more information promised on the basis underlying of the "Expert opinion" advised to have informed the selection of the values of key parameters of the model.

Introduction

The Weller *et al.* (2014) model (also known as the "*penguin pressure model*") of the penguin population at Robben Island reaches conclusions that are at variance with key results reflected in other documents in the MARAM/IWS/DEC14/Peng/B series.

For example, Weller *et al.* (2014) conclude that "although restricting fishing around the island was on average beneficial to the penguin population, variability in population growth introduced by fluctuations in prey biomass tended to mask the outcome."[1] Their results[2] nevertheless indicate a clear pattern: the greater the restrictions on fishing around Robben Island, the more likely a benefit to the penguins, and the greater this benefit is likely to be. This contrasts sharply with the results in Robinson (2013), as extended in MARAM/IWS/DEC14/Peng/B4, where the values of the parameters of relationships between the extent of fishing and measurements related to penguin reproductive

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[1] Weller *et al*. (2014) make this choice of scenarios to relate to the current closure programme for Robben Island, which they state is "to examine the effect of closing the area around a penguin colony to fishing". But their comment about "masking" suggests that they have misunderstood the intent of this programme. The intent is not to estimate this effect directly (though some results obtained already do provide information on that), but instead to provide estimates of process variance in order to determine the power of experimental closure programmes to detect the effect of closure on penguin reproductive success..

[2] Three scenarios were explored in that paper, each starting with 3500 adult penguins.

i) The first scenario considers a complete cessation of fishing around Robben Island for 20 years. That paper reports that this results, on average, in a population increase "of ~8% of starting numbers" (corresponding to an increase of 289 ± 713 adults).
ii) The second scenario considers the effect of a single three-year fishing closure with climate and flooding effects enabled, as well as oil spills in 1998 and 2000. A set of 20-year simulations were run, with the closure starting in each run in years incrementing from 1988 to 2006. The mean result was an increase of 0.6% of starting numbers (corresponding to 22 ± 142 adults).
iii) A variant where three-year closures alternated with three-year fishing periods over 20 years was also tested (leading to an increase of 52 ± 327 adults).

success are estimated, with the majority showing positive correlations (i.e. fishing enhances rather than diminishes penguin reproductive success).

Given this marked difference, it is important to examine the basis for Weller *et al.*'s quantitative results, to check what inputs are driving them, and to ascertain how reliable these are. Similar concerns relate to Weller *et al.*'s conclusion that the Robben Island penguin population is (most) "strongly driven by food availability" (which in turn is impacted by fishery catches).

However initial examination of Weller *et al.* (2014) immediately shows that these results follow from values selected for key parameters, for which the justification that is put forward is "Expert opinion" alone. We have for some time sought details of the process by which this expert opinion led to the values in question. Appendix A lists some of the related correspondence. While we had hoped that a response would be forthcoming to our request therein for these details in time to respond by the 27 October deadline for this round of submissions to the Panel, this has not proved possible as that correspondence indicates. We will therefore reserve our overall position until those comments are provided in the first round of submissions, responding if and as appropriate in the second round of "response" comments.

In the meantime though, we take this opportunity to elaborate on the one issue on which some response has been received from Weller and Sherley (2014) - the basis for the selection of parameter values for the relationship of some penguin mortality parameters to fish abundance ("availability") – as well as to some other issues including further comments by Weller and Sherley (2014) in regard to the Weller *et al.* (2014) model.

Queries raised in relation to the Weller *et al.* modelling of penguin egg and chick survival

Methodology

In the Weller *et al.* (2014) model (see Table 1 for details of the symbols used which are pertinent to the discussion here), availability of food to penguins has an effect on the survival of eggs and of chicks. (The equations are provided in the supplementary material for the paper.) Here we consider their equations for egg survival (hatching success). (The equations for chick survival are similar.) Monthly egg survival S_e is modelled as a logistic function:

$$S_\mathrm{e} = \frac{1}{1+\exp(-A)} \tag{1}$$

where A is a function of predator and climate pressures P, a food availability effect F, and an index B of the prey abundance available to penguins in Zone 1, which extends 15 nm (28 km) from the island:

$$A = e_0 + P - FB \tag{2}$$

Here, e_0 is related to the mean egg survival $\bar{S}_\mathrm{e} = 0.649$ (a value which was found by Weller *et al.* (2014) to result in a stable population alongside "reasonable" choices for chick, immature, and adult survival):

$$e_0 = -\ln\left(\frac{1}{\bar{S}_\mathrm{e}} - 1\right) \tag{3}$$

The pressures P and food effect F are independent of fishery catches. The food effect is defined as:

$$F = -e_0 - \ln\left[\frac{1+\exp(-e_0)}{E} - 1\right] \tag{4}$$

where the source of the value of the effect on eggs $E = 0.5$ is advised to be “expert opinion”. The prey abundance indices B are calculated from the biomass of anchovy and sardine between Cape Columbine and Cape Point (survey stratum D) estimated in the annual May acoustic surveys and catches made by the purse-seine fleet in pelagic fishing blocks roughly covering the area within 30 nautical miles of Robben Island. These calculations assume that the recruit biomass is evenly distributed throughout Stratum D.

Discussion

Figure 1 provides example plots which show the relationship between egg survival S_e and the prey abundance index B for different values of E. All these plots are conditioned to give the value of $S_\mathrm{e} = 0.649$ for an equilibrium at prey abundance index $B = 0$. The role of the E parameter is clear from this plot—essentially it determines the extent to which the S_e value changes with prey abundance. Thus, for example, the further the value of E is set below 1, the more S_e will drop below the value which results in a stable penguin population, the poorer penguin reproductive success, and the faster penguin abundance will drop.

Thus the effect of food availability on egg survival is directly dependent on the magnitude of the value input for the E parameter. Similarly, the effect of food availability on chick survival (fledging success) is directly dependent on the value input for the corresponding effect parameter (denoted *%feC* in Weller *et al.*, 2014), which was also set at 0.5 based on “expert opinion”. Choices of values for these two food effect parameters directly determine the magnitude of the rate at which penguin populations are projected to change under different closure scenarios.

Evaluating the relative plausibilities of the contradictory results of Weller *et al.* (2014) and Robinson (2013) thus boils down to an evaluation of the justification underlying the values selected for the food effect parameters in the former using “expert opinion”. To be able to evaluate the credibility of those selections, there needs to be an explanation of how these experts were able to estimate the magnitudes of these effects, and with results which must differ appreciably from most of the corresponding effects estimated directly from data by Robinson (2013) because of the diametrically opposed conclusions of the two analyses.

Extracts from response by Weller and Sherley (2014)

[These are shown in *red italics*, followed by the authors’ responses]

Management recommendations for African penguin colonies based on results of the Robben Island penguin pressure model presented in Weller et al. (2014) were tabled at the Island Closure Task Team on March 12th, 2014. As these results contrast with some conclusions of Robinson (2013), the model has been subject to comments (Robinson and Butterworth, 2014; de Moor, 2014) that question the validity of several of the model’s conclusions. The following addresses these comments.

Penguin age classes in the model (eggs, chicks, immatures, and adults) have survival rates that are subject to modification when food availability changes. Class-specific parameters (referred to as "food parameters" in the following) are used to connect an index of available prey biomass to the survival rates; in effect, these parameters determine the strength of influence that changes in food availability have on penguin population development, as Robinson and Butterworth (2014) correctly summarize.

Robinson and Butterworth (2014) take issue with two points: a) there is a direct correlation of deviation from mean age class survival with deviation from median food availability; b) the magnitudes of parameters have not been directly estimated in the field, but are based on the expert opinion of the scientists involved in the model's development.

There may be a semantic confusion here, arising from the loose use of the word "availability" without clear definition. Hopefully Appendix A of MARA/IWS/DEC14/Peng/B3, which carefully describes the relationships between food availability, catchability, and local *vs* overall density/biomass**,** helps clarify the situation. The issue here is the conversion of biomass present to "available biomass" – "catchability" in standard fisheries terminology. If catchability is constant, the correlations mentioned above are not at issue. But as that Appendix A explains, catchability is not necessarily constant in the situation under consideration here. A particular concern in the context of the matter under consideration here is the failure to understand the predation shoaling model of Clark (1976) which is evidenced by the authors of Crawford *et al*. (2014) (see Item 10 of MARAM/IWS/DEC14/Peng/B10), who include the authors of the quotation above. Whether or not we are taking issue under a) depends on what the authors of that quotation intend to mean by "availability" (i.e. whether or not they intend it to include catchability as well as biomass *per se*). Our primary concern though remains b).

b) The provenance of the food parameters is stated as "expert opinion" in Weller et al. (2014). This denotes values that, in the absence of direct estimates, were chosen based on the best judgement of the involved scientists while taking into account all available related data. As the food parameters could not be estimated from available field data, they were chosen such that they mapped the range of observed survival rates for each age class to the range of observed variation in food biomass. Egg and chick survival data used to determine the range were taken from Sherley (2010) and have more recently also been presented on a per nest basis as derived from parametric survival models (i.e. survival during incubation and during chick-rearing) in Sherley et al. (2013) and as per egg and chick survival in e.g. Sherley (2012) and Sherley (2014). Juvenile survival data were taken from Whittington (2002), and adult survival data from Whittington (2002), later updated by Crawford et al. (2011) and now also confirmed independently by Robinson (2013) and Sherley et al. (in review).

Previous concerns expressed about the reliance of the results of Weller *et al.* (2014) on selection of parameter values through "expert opinion" have been enhanced by the explanation offered above for the manner in which these survival rate parameters were chosen. The particular problem is the manner in which the "observed variation in food biomass" has been inferred: through linkage to catches made in the year concerned. Why this is a totally flawed approach is explained in detail in MARAM/IWS/DEC14/Peng/B4, B9 and B10. This problem emphasizes the need for a much fuller justification of the other parameters selected by "expert opinion" in the Weller *et al*. (2014) document before the reliability of its conclusions might be properly assessed. Thus, for example, Weller *et al.* (2014) state that they use a mean adult survival rate for penguins of 0.88 (their Table B.1), and the authors of the quotation above therein that this is consistent with the results of Robinson (2013). Yet Fig. 4.9(e) of Robinson (2013) (see also Figure 6 of MARAM/IWS/DEC14/Peng/3a), based directly on fits to penguin tag-recapture data which contain information on survival rates, shows a quite different picture, with the dynamics of the penguin population dominated by the need for a marked reduction in this survival rate in the early 2000's for compatibility with those tag recapture data. This example is indicative of some wider and more fundamental problems with the Weller *et al.* (2014) modelling approach, particularly:

- Lifting parameter estimates provided by one model to substitute in a second when there are data available to estimate those parameters when fitting to that second model. The latter approach avoids transplanting the different biases associated with different methods, thus ensuring self-consistency, and in particular maintains variance-covariance structure thus facilitating defensible statistical inference. This is why the latter is now almost universally the standard approach in fisheries modelling, with the former approach as implemented in Weller *et al*. (2014) being avoided given also that the computing power now available is readily able to effect such fits in most cases.
- Failure to show in a statistically defensible manner that the model developed **IS** consistent with the available data. This is a *sine qua non* for use of models for tactical fisheries management advice, which is the issue in question here – models which do not exhibit satisfactory residual diagnostics in their fits to the data are simply not acceptable for such use (unless some compelling explanation for the mis-fit can be offered). (This would also seem to be the root of the concerns expressed by de Moor in FISHERIES/2014/APR/SWG-PEL/ICTT/15.)

Robinson and Butterworth (2014) furthermore assert that choice of food parameters also influences model-based conclusions regarding the benefit of different fishing restriction regimes around Robben Island to the penguin population, and that these conclusions may thus be suspect. Weller et al. (2014) found that restrictions were on average weakly beneficial to colony growth, but that the outcome was easily masked by variability in prey biomass. Benefits were found to increase and masking to decrease with longer restriction duration.

It is crucial to note that these conclusions, and resulting management recommendations, remain practically unaffected by the choice of food parameters, regardless of uncertainty about exact parameterization. Given the scientific consensus that penguin populations respond positively to improved food availability (Annex 1), the finding that reducing competition for small pelagic prey promotes penguin colony growth is uncontroversial. However, the main conclusion concerns differences in relative benefit of different closure scenarios rather than the absolute gain derived from each. It is readily shown that this is not subject to change depending on food parameter choice.

The three restriction scenarios discussed in Weller et al. (2014) were rerun using a range of food parameter permutations employed in that study to carry out sensitivity tests on model processes (Table 1). The four parameters concerned (affecting egg, chick, immature, and adult survival rates, respectively) were modified to 50% ('very high' effect strength), 90% ('high'), 110% ('low') and 150% ('very low'; capped at 0.99), in addition to standard values ('base', no change).

Parameter	*Very low*	*Low*	*Base*	*High*	*Very high*
Adult survival	*0.990*	*0.880*	*0.800*	*0.720*	*0.400*
Immature survival	*0.990*	*0.770*	*0.700*	*0.630*	*0.350*
Chick survival	*0.750*	*0.550*	*0.500*	*0.450*	*0.250*
Egg survival	*0.750*	*0.550*	*0.500*	*0.450*	*0.250*

Table 1. *Food parameter values used in the modified fishing restriction scenarios. Categories denote the strength of the effective changes in survival parameters based on underlying pressure drivers. For details see Weller et al. (2014), section 3.2.1.*

Figures 1 – 3 show the result of running the three scenarios using the modified parameter sets. Fig. 1 (corresponding to Fig. 16a in Weller et al. (2014)) shows a scenario where fishing around Robben Island is restricted for a full 20 years. Fig. 2 (corresponding to Fig. 16b in Weller et al. (2014)) shows a scenario where fishing is restricted for a single period of three years within 20 years, with closures starting at a progressively later date. Fig. 3 (not pictured in Weller et al. (2014)) shows a scenario were fishing is restricted and unrestricted for alternating three year periods over 20 years (for further scenario details see Weller et al. (2014)).

It is evident that changes in food parameters primarily affect the variance of the outcome (i.e., symmetrical distribution spread) while only minimally influencing mean population gain. The relative benefit of the three scenarios in comparison remains equally clear at each level of modification, even though parameters at the two extreme levels are well beyond those that could be expected to hold stable over a 20 year period. It can therefore be concluded that the discussed findings initially reported in Weller et al. (2014) hold, regardless of the exact parameterization of prey availability influence in the model, and that the associated criticism expressed by Robinson and Butterworth (2014) is unfounded.

The investigation of sensitivity presented here is useful, though the structure of the Weller *et al*. (2014) model in this regard still remains such as can reflect an effect of fishing ranging from minimal to very substantial, simply by extending the ranges of the alternative values considered here, where furthermore the basis used to infer those values and associated ranges is severely problematic for reasons referenced above. But this touches also on only one of the problematic issues associated with the model structure used: as long as E is set/remains below 1, the structure of the model does not admit any result other than greater catches having an increased negative impact on penguins. Yet inferences from the empirical data (Robinson 2013 and MARAM/IWS/DEC14/Peng/4, Table 2) are in many cases not compatible with that relationship. If this model was formally fit to those data then, it would presumably manage that by estimating values for the E parameter which are greater than 1 - but that hardly makes for realistic biological interpretation within the model structure assumed.

Thus in the context of the issue at hand - the impact of fishing in the vicinity of islands on penguin reproductive success - this restricted structure of the Weller *et al.* (2014) model renders it a “self-fulfilling prophecy”. It can produce results in one direction only. Yet there are plausible mechanisms that can lead to results in the other direction, as explained in Appendix A of MARAM/IWS/DEC14/Peng/B4 and further under Item 10 of MARAM/IWS/DEC14/Peng/B10. The Weller *et al.* (2014) model fails the key test of being unable to reproduce behaviour which inferences from the empirical data indicate to have occurred, because of a structure which lacks adequate flexibility.

In these circumstances then, the actual situation is quite the reverse of the statement made in the quotation above that: *“Given the scientific consensus that penguin populations respond positively to improved food availability (Annex 1), the finding that reducing competition for small pelagic prey promotes penguin colony growth is uncontroversial.”*.

Other issues arising from the Weller *et al.* (2014) model

[Again comments by Weller and Sherley (2014) are shown in *red italics*, followed by the authors' responses]

A further comment (de Moor, 2014) concerns a scenario in Weller et al. (2014) that attempts to recreate observed population developments on Robben Island between 1988 and 2008 using the model environment. This scenario was intended to test the capability of the model to simulate recorded data, given specifically chosen but plausible assumptions about parameter values. These included a reduction in climate pressures during this period, changes in marine predation pressure in 2003, and immigration by immatures (the latter is not explicitly included in current model versions due to lack of data).

de Moor (2014) presents the following criticisms:

a) The scenario fails to correctly reproduce recorded numbers. – Peak numbers of simulated adults are indeed lower than recorded peak numbers, as more weight was put on replicating the shape than the magnitude of the trace. This should arguably have been remedied, and in fact can be addressed without leaving plausible parameter space by slightly lowering maximum marine predation rates after 2003.

This response is problematic as a reflection of reality– as mentioned above the penguin tag-recapture data indicate a **drop** in the adult penguin survival rate at that time, which implies an **increase** in some component contributing to natural mortality.

b) Model fit was achieved by targeted modification of parameter values, creating an unreasonable combination of parameter states, including lack of known oil spills. – *This criticism misses the point of the scenario. The observed population trace can be thought of as a single replicate, based on a specific combination of parameter values. The scenario demonstrates the capability of the model to encompass this unique output in its output space, using specific parameter values from the implemented (plausible) parameter space. Naturally this requires picking these values; the important restriction is that they must be part of the plausible range. The plausibility of the chosen values is discussed in Weller et al. (2014). Note that the assumption that the catastrophic oil spills known to have occurred during the modeled period were excluded is mistaken; as stated, these spills were included, and the less quantifiable chronic oiling was disabled.*

c) Immigration should have been included as a model process as an 'expert guess'. – *As discussed above, parameters based on expert opinion are not 'guesses', but inferences from the best available data and the experts' understanding of processes. In the case of immigration, the data were judged to still be too scarce and/or the analysis of available data to be in too early a stage to allow reasonable parameterization. Migration between colonies is instead assumed to constitute part of the adaptations made to survival values to achieve a base equilibrium population; this is the factor that was adjusted to temporarily simulate increased immigration in this scenario. Migration is clearly a very important process in colony development, and will be included in the model as soon as sufficient results are available.*

Two immediate questions that arise from the above are:

i) exactly how is that "expert opinion" able to achieve supposedly more reliable values than the now standard practice of estimating those values when fitting the model to the available data; and

ii) why were the available data "judged to still be too scarce" when Robinson (2013) – see also MARAM/IWS/DEC14/Peng/3a and 3b - has already demonstrated how the tag-recapture data can be used to estimate the parameters in question with reasonable precision?

To demonstrate the capability of the model to reproduce observed dynamics using a plausible parameter space, without having to work around this unimplemented mechanism, the development of the Robben population from 2004 through 2012 was replicated (Figure 4). No immigration is thought to have occurred during this period (Robinson 2013), which features a steady decline in population size. Fig. 4a shows recorded numbers of breeding pairs, Fig. 4b shows simulated numbers of breeding pairs. This scenario uses recorded biomass and fisheries catch data, and all pressures except catastrophic oil spills (known to not have occurred during this period) are enabled. Mean survival rates for all age classes were set to 90% of base survival rates, simulating a period of years with slightly higher than average pressures. The result is a fair approximation of observed dynamics using reasonable assumptions about parameter values.

From the additional investigations and analyses detailed above, it appears that the model is fit for the purpose for which it was intended, and that the conclusions previously drawn from the model are well founded.

This "purpose" needs to be elaborated further. If a model does not exhibit a statistically acceptable fit to available data, how can any conclusions pertinent to tactical management recommendations be drawn from it? The position taken above is advocating a complete reversal of standard practice in fisheries internationally for making scientific recommendations of this nature (in other than extreme "data-poor" situations, which this is not).

Related to this concern is an underlying fundamental flaw in rationale offered for the approach adopted by Weller *et al.* (2014): that assessing ecosystem effects, as in this instance, requires an approach differing from traditional fish stock assessment models as these are "restricted by the necessity for the factors and relationships involved to be well parameterized", and that "difficulties associated with parameter estimation due to scarcity of data have meant that explicit consideration of multiple pressures driving penguin population dynamics necessitates a move to a different paradigm". **These are completely incorrect assertions**. Analyses of the type and level of complexity in Weller *et al.* (2014) are now routinely conducted under such "traditional" approaches, with "data scarcity" aspects handled, for example, through specification of priors within a Bayesian estimation approach. This flawed rationale offered by the authors of Weller *et al.* (2014) provides no acceptable reason for the model in question not to have been fit to the available data in a "conventional" manner. The model has potential to inform tactical management recommendations, but such proper fitting of the model to those data is an essential pre-requisite.

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Table

Table 1: Symbols used in this document and in Weller *et al.* (2014).

Parameter	Symbol used here	Symbol in article	Value
Monthly egg survival	S_e	S_t^e	
Mean egg survival	$\bar{S}_\mathrm{e}$	mean egg survival	0.649
Egg survival logit	A	egg survival logit	
	e_0	e0	
Predator and climate pressure	P	catE∗cat abundance + sharkE∗shark abundance + heatE∗climate Heat + coldE∗climate Cold	
Food availability effect	F	food effect E	
Zone 1 prey abundance	B	z1	
Food availability effect on eggs	E	*%feE*	0.5
Food availability effect on chicks		*%feC*	0.5

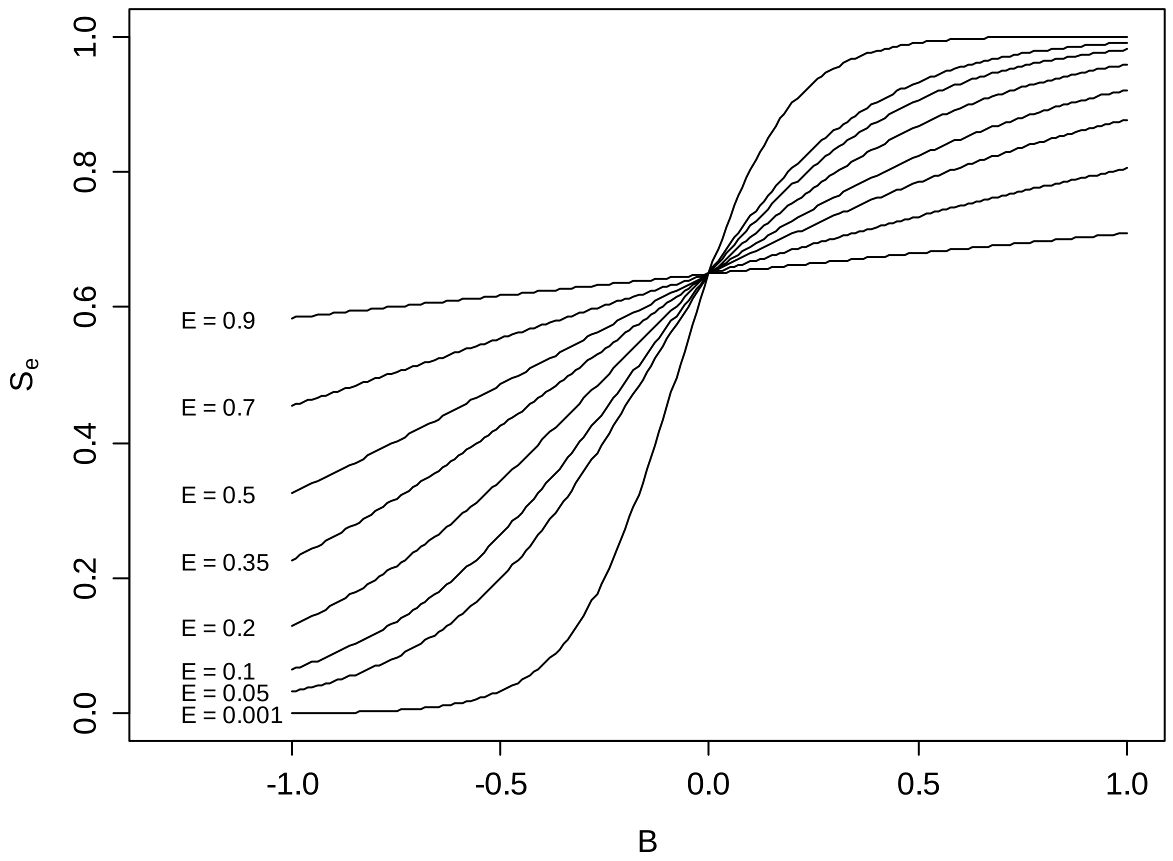

Figure 1: Egg survival as a function of the abundance index B for a range of values of the food effect size E, assuming average predator and climate pressures ($P = 0$). $B = 0$ corresponds to the median sardine and anchovy abundance (biomass) from Cape Columbine to Cape Point estimated in the annual May survey. $B = -1$ corresponds to no fish, and $B = 1$ corresponds to an abundance three standard deviations above the median. B varies piecewise linearly between these three points.

APPENDIX A

Correspondence in regard to Explanations Requested of the Basis to Parameter Value Choices in the Weller et al. Robben Island Penguin Model that are Reported in that Paper as "Expert Opinion"

Email dated 8 October 2014

Thanks Astrid for this acknowledgement, and I'm pleased to hear that Florian is working on this documentation.

Note that my request was following up on an offer made to provide this information at a PWG meeting during the first half of the year (my original to Lynne refers), so predates the 30 July procedures. I had thought it might have been helpful, had it been possible to provide this material earlier to allow our comments thereon by 27 October, to allow you and you colleagues to then perhaps respond to those by the later "responses deadline" in November. In the circumstances you indicate, we will comment on what has already been advised in our 27 October submission, and pick up as appropriate by that November response deadline on anything further in the submission which you and your colleagues make on 27 October.

Cheers

Doug

Email dated 7 October 2014

From: Astrid Jarre [mailto:ajarre@gmail.com]
Sent: 07 October 2014 04:16 PM
To: Doug Butterworth; richard.sherley@gmail.com; Celebrate Our Seas; Herman Oosthuizen
Cc: Lynne Shannon; florian.g.weller@gmail.com; janetC@daff.gov.za
Subject: Re: Aspects of penguin pressure model paper

Hi Doug,

In acknowledging receipt of your email below, I can confirm that Florian is finalising documentation of the basis of the parameters referenced as "expert opinion" in the article published in Ecological Modelling. This will be discussed at the BirdLife Workshop next week, and possibly at a PPMWG meeting shortly thereafter. We expect that the outcome will be documented in the report of the BirdLife Workshop, due on 27 October. Florian will then be away for some time on international duty travel.

The document referenced "Fisheries/2014/Jul/SWG-PEL/Aide Memoire 30th July" refers with regard to deadlines (bullets d) and e)) and appropriate channels for communication (bullet f)).

Best,

Astrid

Email dated 5 October 2014

On Sun, Oct 5, 2014 at 5:03 PM, Doug Butterworth <doug.butterworth@uct.ac.za> wrote:

Hi Florian

Lynne's helpful response below refers.

We are likely to be addressing some aspects of your "pressure model" paper in our submission (due by 27 October) to the international panel meeting in early December. To that end I hope that you can provide responses to the questions below, which we would include together with our comments in our submission.

These questions are asked in the context of the conclusion in your paper that the Robben Island penguin population is (most) "strongly driven by food availability" (which in turn is impacted by fishery catches). Clearly the magnitude of such effects as indicated by your model depends on the relationships between such "availability" and penguin reproduction and survival factors, and hence on the forms of these relationships and the values assigned to their parameters. Our present difficulty in evaluating your analysis and results stems from the fact that many of these parameters are put forward on the basis of "Expert opinion" alone. Customary practice in science is that an assertion needs to be falsifiable (at least in principle) for acceptability. You do indicate that there is a basis in data/information in these opinions, and that is what I hope you can spell out for us so that we have some basis to evaluate the appropriateness of these value selections (and argue their falsification if we see this to be appropriate).

The specific parameters for which we seek this information are (your paper's Table B.1):

%sharksA, %sharksI, %sharksC, %sharksE, %catC, %catE

%feA, %feI, %feC, %feE, febp

and in association which those:

i) the basis for the assumption of a 50% drop in survival of chicks born to oiled parents, and clarity whether this applies only to the year of oiling of to all further years in the adult's life,
ii) the basis for the choice of 1500 adults as the level below which the breeding proportion increases, and for the choice of 20% for that increase level, and
iii) whether you have any evidence, other than inference drawn from the dramatic peak in abundance of small pelagics at the turn of the century, for the assumption that marine predators were below average before and above average after 2003, and for the extent assumed for this effect.

Your document FISHERIES/2014/APR/SWG-PEL/ICTT/23 to which Lynne refers seems to provide only an incomplete response on one of these points in stating that food parameters "were chosen such that they mapped the range of observed survival rates for each age class to the range of observed variation in food biomass". What needs to be provided are the tables and plots of these values and associated biomasses, their sources, and further a plot as minimally some relationship needs to be evidenced rather than range mapping alone.

Thankyou for the offer to carry out some further stochastic sensitivity simulations, though I doubt we will be requesting those (at this stage at least). Our primary interest is first in the values themselves and their deterministic implications, as clearly the effect of fishing inferred from your model can range from negligible to enormous dependent entirely on what these values are.

Your response within 10 days would be helpful, to allow us sufficient time to incorporate that together with our associated comments in our submission to the panel by the 27 October deadline.

Cheers

Doug

Email dated 1 August 2014

From: Lynne Shannon
Sent: 01 August 2014 02:51 PM
To: Doug Butterworth
Cc: richard.sherley@gmail.com; Astrid Jarre; janetC@daff.gov.za; Florian Weller
Subject: Replying to your email: Aspects of penguin pressure model paper

Hi Doug

I did not agree to personally gather the data source information you mentioned, given my own severe time constraints, but I have checked with the first author of the paper in question and he has indicated his willingness to compile a table with a little more detail on the "expert opinion" sources for the parameters you are concerned about. As mentioned verbally at a meeting earlier this year, in response to your concerns regarding "expert opinion", these parameter estimates were the consensus of a dedicated panel of highly respected penguin biologists (experts) working on the dynamics of the African penguin, with parameter estimates being drawn directly from their various data sets, studies and knowledge. Please also refer to the response document by Florian and Richard (re-attached as a reminder), as this furnishes us with details on sources of some of the key parameters in question. It was unfortunate that this document, although tabled at the April meeting, was not afforded time for discussion at the time.

In addition to a more detailed parameter table from our side, I could suggest a second way forward that could hopefully ease the road ahead for us all. We would be amenable to running some additional (reasonable in number) model simulations using alternative parameter values which we invite you to provide (with appropriate reasoning for your requests). Of course, this would very much depend on the time available on our side, given we are not on contract to DAFF and thus this work needs to be fitted in where we can manage it in the wings of our other funded commitments. Would you let us know whether this may be a workable way forward in the interests of collaboration both across UCT departments and between UCT and DAFF? I am copying this to Florian and Richard who authored the initial response document, to Janet as Chair of the Pelagic SWG, and to Astrid as Florian's supervisor and employer. Please note, however, that Florian is away and out of the country from next week until early September, and I am largely out of the office until late August, thus understandably replies may be slow but we shall do our best.

I hope this is of some help in smoothing the process.

Regards

Lynne