Responses to MARAM/IWS/DEC14/Peng/B10

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Comments are made in response to document MARAM/IWS/DEC14/Peng/B10. For ease of reference, the original text of Butterworth (2014) is reproduced in full and additional comments are made in **blue bold text**.

Responses to Reservations Raised Concerning the GLM Analyses of and Inferences Drawn from the Results from the Island Closure Feasibility Study

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Abstract

This document provides responses to a number of reservations which have been raised locally over the past year to the GLM approach for analysis of the island closure feasibility study, and conclusions inferred from the results. The issues covered range from whether catch provides an index of local fish abundance, the Clark model of a relation between shoal size and predation, comparisons with what occurred in Namibia, the appropriate period for which islands need to be closed, an argued need to apply model selection methods when developing the basis for a power analysis, whether a step-function relationship is appropriate for describing the different results from closure *vs* non-closure of an area around an island to fishing, and what the default conclusion about the impact of fishing near to penguin colonies should be.

Background

During local discussions over recent months, a number of reservations have been raised concerning the GLM analyses, together with their results and inferences drawn therefrom, which have been applied to the results for penguin response variables forthcoming from measurements taken during the Island Closure Feasibility Study. (These GLM analyses were earlier versions of those presented in MARAM/IWS/DEC14/Peng/B4.)

This document details those reservations, with associated responses, on matters which have not been addressed in other of the documents to the Panel in this MARAM/IWS/DEC14/Peng/B series. Where the reservations are available in written form in earlier DAFF Working Group documents, and a relevant extract is being quoted, that quote is reproduced in *red italics* below. Details of references in those quotations are listed only if necessary.

Arguments that catch (near to an island) is an index of (local) abundance

[The source of the quotations following, until otherwise indicated, is Crawford *et al*. (2014).]

Item 1

Robinson and Butterworth (2014a, 2014b) were of the opinion that fishing around penguin colonies was of benefit to penguins. Their conclusion was based on outputs from GLM analyses of the form:

$$Ln(F_{y,i})=\alpha_y+\beta_i+\lambda_i(c_{y,i,p})/(average\_c_{i,p})\ \varepsilon_{y,i} \qquad (1)$$

where F is a penguin response variable (e.g. breeding success), y = year, i = island, α_y *is a year effect reflecting prevailing environmental conditions,* β_i *is an island effect,* λ_i *is a fishing effect,* $c_{y,i,p}$ *is the catch taken in year y in the neighbourhood of island i of pelagic species p,* $average\_c_{i,p}$ *is the average catch at island i of species p taken over the years considered and* $\varepsilon_{y,i}$ *is an error term. In a majority of instances they found that* λ_i *was positive, thence inferring a beneficial influence of fishing for penguins (Robinson and Butterworth 2014a). This inference though is based on the assumption that* $c_{y,i,p}$ *is not an indicator of fish availability near island i (local availability), although a ready availability of fish in the vicinity of an island might result both in improved catches in the island's proximity and benefit for penguins.*

The assumption promoted by the authors of this quotation that $c_{y,i,p}$ is an indicator of fish availability (abundance) near island *i* is confounded by other effects. The fundamental flaw in making this assumption, which is widely rejected in fisheries, is explained in detail in MARAM/IWS/DEC14/Peng/B4 and also B10. The assumption is also refuted by the analysis of South African anchovy catch-related measures in MARAM/IWC/DEC14/Peng/B9 which indicates no useful relationship between these measures and anchovy abundance.

See responses below under item 6.

Item 2

Robinson and Butterworth (2013) used a variant of the above GLM, in which α_y *is replaced by* $\mu B_{y,p}$*, where B is the annual (recruit or spawner) biomass in year y of species p. However,* $c_{y,i,p}$ *may be strongly correlated with* $B_{y,p}$*, as was demonstrated for anchovy (spawner) at Dassen Island (Durant et al. 2010) (and occurs at Robben Island) and sardine (recruit and spawner) at both Robben Island and Dassen Island (Table 5 in Sherley 2014), despite Robinson (2013) and Butterworth (2014b) reporting that the average correlation is relatively small (r ~ 0.3).*

Indeed there may be instances where the correlation is higher than 0.3. But that does not negate the implications of the statement by Robinson (2013) that "A review of the correlation coefficients r between the biomass and catch time-series used in each model considered revealed that the average correlation is $r \approx 0.3$, which is reasonably small. (Compare the plots of survey biomass versus catches for the full time-series in Figures 2.4–2.6.) Severe distortion of parameter estimation tends to occur only when $|r| > 0.7$ (Dormann *et al.* 2013), and this threshold is breached in only a very few cases. In these cases, the variance-inflation factor 5 (VIF) was calculated. Results never exceeded 10, which is often used as a threshold for indicating severe collinearity, although even higher VIFs are often acceptable (O'Brien 2007)." Clearly if the average is about 0.3, there are many instances where the correlation is similar to or lower than that. One possible exception as cited here certainly does not negate a broadly evident feature of the data as a whole, and the implications that follow from that. Further, as pointed out in MARAM/IWS/DEC14/Peng/B4, even if cases where $|r| > 0.7$ are excluded from the overall set of results, the broad conclusions are unaltered.

The problems associated with even these weak correlations are dealt with in Bergh (2014: MARAM/IWS/DEC14/Peng/A10)

Item 3

Robinson and Butterworth (2013) also assume that fish are similarly abundant around neighbouring islands and that these islands thus can be used as controls.

This reflects a serious misunderstanding of the implicit assumption (see also the more detailed explanation provided in Appendix B of MARAM/IWS/DEC14/Peng/B4). First note the island factor β_i in equation (1) above allows for the possibility of widely different catchabilities (or "available abundances") (see that Appendix B) at the different islands (i) – there is NO requirement for "similar abundance". The implicit assumption, which is far weaker than the authors of this quotation suggest, amounts, essentially only to positive correlation. It is difficult to envisage a plausible situation where that would not apply. Deviations from proportionality will be absorbed into the composite residual $\varepsilon_{y,i}$. It would require some extreme correlation structure, related also to the catches made, to result in large biases in estimates of λ, and no examples of that have been provided by the authors of the quotation to allow the associated necessary review of their plausibility.

Item 4

This assumption is still to be tested using the small-scale fish surveys discussed below and requires further interpretation in the light of shifts in the centre of gravity of catches (Fairweather et al. 2006) and deterioration of seabird habitats off northwest South Africa (Waller et al. 2014).

These shifts and argued deterioration are irrelevant in this context. The separations between the island pairs in question are at a much smaller spatial scale. The analyses of these small scale surveys (MARAM/IWS/DEC14/Peng/B6) did not reveal any inconsistency with this assumption, though process errors associated with these surveys were also shown to be high, meaning that their information content is limited.

Item 5

The alternative assumption, i.e. that catches made in the vicinity of an island represent the availability of fish near that island, was adopted by Sherley et al. (2013). Those authors showed that, for African Penguins at Robben Island, breeding success and chick-fledging rates were positively related to local food availability, indexed through the annual industrial catch of anchovy made within 56 km (30 nautical miles) of the colony. They further found chick-fledging rates were depressed in 2-chick broods during years when anchovy contributed < 75% by mass to the diet of breeding birds and concluded that these results highlighted the importance of ensuring adequate local food availability for penguins during their reproductive cycle. Similarly, Durant et al. (2010) suggested fishing in the vicinity of Dassen Island might cause reduced participation by penguins in breeding and recommended that management of the purse-seine fishery be adjusted spatially in order to ensure adequate local food supplies for breeding African Penguins.

See the response to Item 1 above regarding the fundamental flaw in making this alternative assumption

Item 6

That locations of catches reflect the distributions of epipelagic fish is not a novel concept. It was used by Fairweather et al. (2006) to describe an eastward shift of sardine off South Africa between 1997 and 2005. Later, Sabarros et al. (2012) used catch per effort information, validated against fishery-independent hydroacoustic survey data matching in time and space, to identify locations of peaks of abundance (PoA) in epipelagic fish around the South African coast and magnitudes of the peaks. They demonstrated that at the 17 colonies of African Penguins in South Africa, numbers breeding were positively related to the magnitude of the nearest PoA of anchovy and sardine (combined) and negatively to the distance of the PoA from the colony. Similarly, numbers of Cape Gannets (which *also feed mainly on anchovy and sardine) breeding at their three South African colonies were positively related to the magnitude of the nearest PoA and negatively related to its distance from the colony.*

To assert that some broad indications of fish distribution are provided by catches is quite different to making assumptions that catch is proportional to biomass, which is one that is seriously questioned in fisheries (see the response to Item 1 above). It is in any case quite incorrect for the South African anchovy, where much of the abundance is on the Agulhas Bank and unfished because of lower densities – indeed before surveys commenced in the mid-1980s, the extended distribution of this species into this area was not known.

The claim in Sabarros *et al.* (2012) that the pelagic CPUE which they define is use-able as an index of abundance, and that this has been validated against hydroacoustic survey data, is scarcely credible. Fig. S2.2 of that paper is reproduced As Figure 1 at the end of this document. Coetzee (pers. commn) comments that: “This plot is incorrectly labelled. It is not backscattering but density (g.m^{-2}), so is in fact proportional to biomass. Sabarros and co-authors appear to have matched the data in time and space by using only May and November catch data that occurred within 10 nm of the central position of each density position. Obviously these densities are not accurate indications of biomass; they would need to be weighted by interval length, line length, stratum, area etc. to calculate the biomass.” One notes further that the catches considered occur over periods of a month, during which the fish could move substantially, and the data plotted are not species-specific. The r^2 value for the regression line shown is only some 4%, and hardly indicative of some meaningful relationship, particularly when one notes that the data points about that line typically range from about four orders of magnitude above to four orders below the line. In any case, CPUE is scarcely used anywhere worldwide in the assessment of pelagic species because of its known unreliability as an index of abundance, *inter alia* because of likely non-linearity in the relationship (a factor Sabarros *et al*., 2012 ignore in their analyses) (see also the response to item 1 above).

To quote from the above: “To assert that some broad indications of fish distribution are provided by catches is quite different to making assumptions that catch is proportional to biomass…”. Assuming then that the implication is that to use catch data for broad indications of fish distributions is fine? Since the availability of a resource to predators and the fishery is in part a function of both the amount present and its distribution (in 3 dimensions in the ocean), the assertion that catches could provide some information to indicate the local availability of anchovy to penguins should not be controversial. We have not, contrary to the claim above, asserted that catches around a colony are proportional to the true, unknown biomass around a colony. We have, suggested that, since fishermen tend to behave in a similar way to central-place foraging predators (Bertrand et al. 2007) and since they appear to be good at finding dense shoals of fish (Bertrand et al. 2004), the catch made near to islands through the course of a whole year might provide some information on the relative availability (i.e. availability from one year to the next) of fish to birds breeding at those islands. This is based on the documented premise that the birds in question need to eat the fish in question (or feed it to their progeny) and that the birds in question are limited in the spatial distribution during breeding. Therefore, biomass estimates that cover the entire coastline from the Orange River to Cape Infanta may also not provide accurate information on the local availability of prey for breeding seabirds.

Item 7

Given the sophistication of South Africa’s purse-seine fishery and its ability to find fish over wide areas, as demonstrated by Fairweather et al. (2006), it might be expected that the distribution of catches, at least within the area of operation of the fishery, partially reflects the local availability of *fish species targeted by the fishery. In view of this, it seems premature to conclude that positive λs emanating from GLMs demonstrate a beneficial influence of fishing on penguins (Robinson and Butterworth 2014a). Rather they may be interpreted as confirming the importance of good local availability of prey for penguins.*

The many problems and associated inconsistencies with this last assertion have been explained elsewhere (see the response to Item 1 above). No cogent rebuttal of the GLM analyses by Robinson and the reliability of their resultant *λ* estimates has been offered by the authors of this quotation.

See Bergh *et al.* (2014: MARAM/IWS/DEC14/PENG/A2), Hagen *et al.* (2014: MARAM/IWS/DEC14/PENG/A3 and Bergh (2014: MARAM/IWS/DEC14/Peng/A10)

Item 8

Indeed, Sherley (2014) carried out an analysis replicating that of Robinson and Butterworth (2014a) for one penguin time-series, but in addition used AICc-based model selection to compare objectively a series of candidate models containing catches in the vicinity of islands and annual biomass estimates. He concluded: "much of the variance in the Active nest proportion that can be explained by catches in the vicinity of the islands can also be explained by the annual biomass estimates and vice-versa. This would seem to support the explanation mentioned on pg. 92 of Robinson (2013), but later discarded, that 'fishery catches are naturally higher when a high abundance of fish is present in dense shoals—precisely the feeding environment which favours penguins'. In other words, both the fishing industry and the penguins are able to find sardine and anchovy close to Robben and Dassen islands in years when fish are abundant close to these islands" (Sherley 2014a).

Counters to these arguments are provided above, and the comment by Robinson quoted is in the context of "other things being equal" – in practice they are not, which is one of the fundamental reasons why catch does not provide a reliable index of abundance (see the response to item 1 above). But furthermore and importantly, Sherley (2014) has completely misunderstood the nature of the power analysis computations being carried out, as explained further in Item 14 below. The issue here is Type II, not Type I error. To suggest that model selection be used in circumstances of time series of insufficient length to detect alternative further effects is hardly scientifically appropriate.

The important point here is not whether or not model selection should be used to determine which models to use for the power analysis. The important point in the original quote is that the amount of deviance explained in the penguin response variables is effectively the same whether catches around the island or total biomass are used, because the two are confounded. See Bergh *et al.* (2014: MARAM/IWS/DEC14/PENG/A2 and Bergh (2014: MARAM/IWS/DEC14/Peng/A10)

Item 9

Should this be the case, it need not be "surprising" (Robinson 2013) that penguins and fishers both benefit from a ready availability of fish near islands – provided catches do not always reduce the local availability of prey below the threshold required by penguins to meet their food requirements. That threshold will depend inter alia on the size of the colony, reducing as numbers of birds at the colony decreases (e.g. Gaston et al. 2007). For example, a greater density of prey in the neighbourhood of Dassen Island would have been required to sustain the penguin colony there in 2004 (when 25,000 pairs were breeding) than in 2013 (when 2,600 pairs bred). However, this effect is not considered in equation (1). A density dependent response in the recruitment of immature penguins to Robben Island (Crawford et al. 2007) confirms the likelihood that densities of prey in the vicinities of colonies will influence the population dynamics of African Penguins. The need to understand how local food availability may be modified by fishing, and at what levels of local prey availability *penguins may be adversely influenced by catches near islands, was a strong motivation to initiate small-scale surveys of fish abundance around colonies of African Penguins.*

The density dependent response estimation in Crawford *et al*. (2007) uses a method well known amongst fisheries scientists to be flawed, as was originally pointed out by John Pope. The regression indicated in the equation on the right hand column of pg 573 of that paper includes the independent variable P on the right as well as the left hand side of the equation in a form that makes a negative correlation inevitable, but does not in fact provide any confirmation of the relationship claimed. Figure 2 shown at the end of this document uses results from the Robben Island penguin dynamics model of Robinson (2013), which uses a statistically justifiable estimation approach, to assess this relationship. Though some density-dependence is indicated, the effect is much weaker than indicated by Crawford *et al.* (2007), and with an r^2 ~ 0.2 which is much less than the r^2 ~ 0.8 claimed by Crawford *et al.* (2007). In any case, Figure 8 of MARAM/IWS/DEC14/Peng/3a shows a trendless relationship between penguin recruitment success and anchovy recruit biomass, hence providing no indication that reducing the extent of fishing would have an impact – a conclusion supported by the "river model" results of MARAM/IWS/DEC14/Peng/B5, which indicate that over the first decade of the current century, the fishery reduced the anchovy abundance off the west coast by typically only some 10% of the amount that would otherwise have been present. To put the claim above that a greater prey density was needed to feed penguins at Robben and Dassen in 2004 than in 2013, given the earlier higher numbers, in an appropriate context, one should note that the annual food requirements of penguins of a little more than some 20 000 tons (Robinson 2013, pg 161) constitute a mere 0.5% of the average annual production of sardine and anchovy resources over the first decade of the current century of about 4 million tons (de Moor and Butterworth, 2010). Thus consumption by penguins is negligible compared to the other sources of natural mortality on these fish, so that changes in penguin numbers by even, say, three-fold above their current levels would have a minimal impact on the abundances of their prey.

A few problems are evident here.

1) Figure 8 of MARAM/IWS/DEC14/Peng/B3a actually shows a trendless relationship between the model predicted penguin recruitment success and anchovy recruit biomass. This is troublesome because the methods of that paper state "The relationship between $\overline{H_y^*}$ (reproductive success, sic.) and the fish biomass index $B_{H,y}$ is assumed to be constant in expectation for the base case, given that analyses[3] did not provide any indication of dependence" with the footnote stating: "3 For example, when allowing for a linear relationship between anchovy recruit biomass and penguin reproductive success, the estimate for the slope parameter came out very close to zero (see Supplementary Figure S9)". The problem being that the Supplementary Figure S9 also shows the relationship between the model predicted penguin recruitment success and anchovy recruit biomass.

In actual fact, the relationship between the estimated anchovy recruit biomass and the measures of reproductive success supplied to the authors of MARAM/IWS/DEC14/Peng/B3a for the period 1989 to 2012 (omitting the year 2000, as shown in Table 1 of MARAM/IWS/DEC14/Peng/C1) weighted by the annual sample size (varying from 10 to 160) yields a slope of 0.51 (s.e. = 0.19, t_{21}=2.68, p = 0.01, Figure R1). While the relationship only explains 22% of the variability, this is appreciably different from the slope of 0 claimed in Figure 8 and Figure S9 in MARAM/IWS/DEC14/Peng/B3.

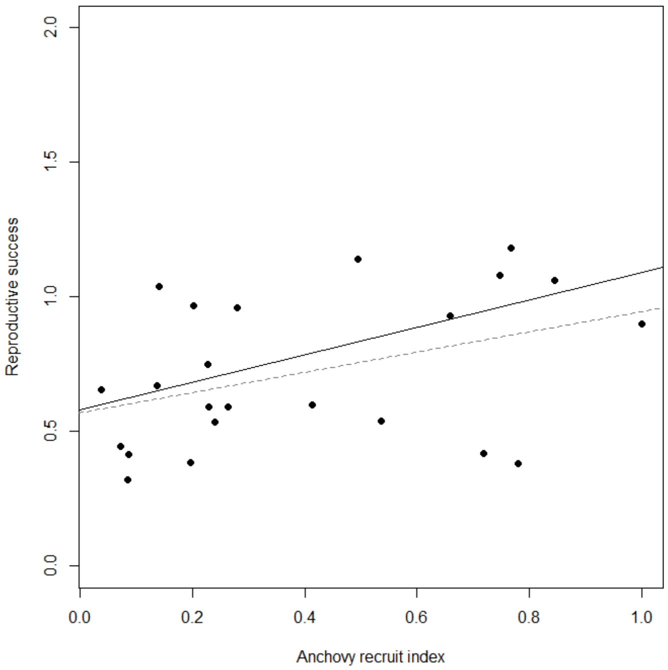


Figure R1: The fitted relationship between anchovy recruit biomass (converted to a proportion of the maximum biomass observed) and penguin reproductive success at Robben Island from 1989 to 2012 (omitting the year 2000). The solid line shows the weighted linear model fit, the dashed line the fit without weights.

Even if one accounts for the first order correlation present in both the breeding success and anchovy recruitment data by regressing the studentised prewhitened residuals from an AR–1 autocorrelation model, the result is still significant, although the slope estimate reduces to 0.47 (s.e. = 0.19, t_{20}=0.47, p = 0.019, Figure R2).

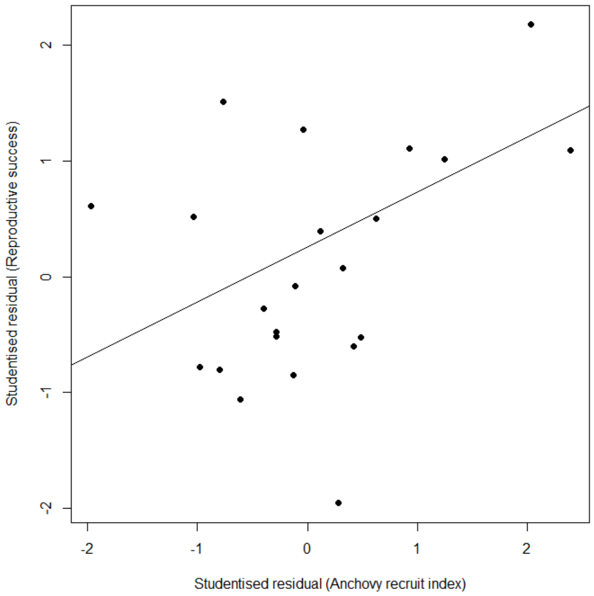


Figure R2: The fitted relationship between the studentised prewhitened residuals from an AR–1 autocorrelation model for anchovy recruit biomass (converted to a proportion of the maximum biomass observed) and the studentised prewhitened residuals from an AR–1

autocorrelation model for penguin reproductive success at Robben Island from 1989 to 2012 (omitting the year 2000). The solid line shows the weighted linear model fit.

Setting up a model in which reproductive success is totally divorced from the availability of the main prey item used for provisioning breeding chicks (Crawford et al. 2011) will lead to unrealistic population projections.

2) Comparing the results of Crawford *et al.* (2007) with Figure 2 is comparing two different things.The analysis in Crawford et al. (2007) dealt with counts of birds moulting into adult plumage in relation to the number of breeding pairs. Figure 2 deals with a density dependent relationship between the number of breeding pairs and the number of birds recruiting into the population after they have survived 1 or 2 years at sea. Figure 2 examines the relationship between breeding pairs and reproductive success; the number of chicks per pair surviving to fledging. These birds would still need to survive at sea before recruiting into the breeding population. These are, therefore, different processes being examined. And, irrespective of the above, there is still a density-dependent relationship.

3) The statement that "changes in penguin numbers by even, say, three-fold" above is a moot point; however, the original statement was in reference to changes in penguin numbers at Dassen Islands between 2004 and 2013. To put this into the correct context, the combined population at Robben and Dassen islands in 2004 was ca. 33 000 breeding pairs or ca. 105 000 individuals. In 2013 it was ca. 3 700 breeding pairs or ca. 11 800 individuals. This is far more than a 3-fold change and, the claim above that a greater prey density was needed to feed penguins at Robben and Dassen in 2004 than in 2013 is well founded. Thus, they would have been consuming roughly 10 times the amount of food in 2004 than in 2013.

Arguments that the implications of Clark's (1976) model of the relationship between predation and shoal size have been mis-stated

Item 10

Robinson (2013) cites Clark (1976) to suggest a possible mechanism for fishing benefiting penguins – "that fishing vessels tend to break up large shoals of pelagic fish, and predators are more likely to encounter prey if there are many small shoals rather than a few large shoals" (pg. 176). However, the argument above is applied inconsistently by Robinson (2013) and seemingly at odds with the original sentiment of Clark (1976). Robinson (2013 pg. 92) also states that "One possible mechanism underlying the apparent benefit of fishing to penguins is that the shoaling behaviour of small pelagic fish is a predator defence mechanism: although larger shoals are more readily located, surface to volume effects mean that in a larger shoal an individual fish is less likely to be eaten" (pg. 92). Clark (1976) states "Since predators are assumed to have fixed appetites, we can assume that the rate of predation is proportional to the rate of detection of schools. The rate of detection is in turn proportional to the visual volume of the school, provided the latter is small in relation to the total volume of seawater over which predators search". In other words, large schools are easier to detect and to extract food from.

Of course this is part of Clark's argument, but not all of it, and the authors of the quotation evidence a complete failure to understand his analyses. What Clark shows is that as a result of the surface to volume effect, the predation probability for an individual forage fish increases as shoal size drops. Consider the same forage fish biomass, divided either into a few large shoals, or into many smaller shoals (e.g. as a result of disturbance caused by fishing). A single large shoal is indeed easier to find than a single small shoal. But in each case the probability of finding a single shoal has to be multiplied by the number of shoals. The combined surface area is larger in the case of the smaller shoals, hence rendering it easier for predators to find a shoal in that case, and consequently to forage more successfully.

Unless purse-seine fishing vessels set their nets and catch nothing, considering the same biomass in one large shoal and many small shoals is not informative. The scenario above works in theory, but in practise, is not likely to occur. Empirical evidence that the above happens on a regular basis is needed seriously consider this argument.

Consider the following, more likely scenario:
We start out with a one shoal of fish. Let's make the assumption, that it approximates a sphere of 10 m radius. This would yield a volume of 4188.79 m^3 of fish, with a surface area of 1256.64 m^2.

Now, imagine that a purse-seine vessel, set their nets *just* to disrupt the shoal and break it up into 10 smaller shoals – *but removed no fish at all* – the hypothetical result would be 10 spheres 418.88 m^3 each, with an area of 270.73 m^2 each and a combined surface area of 2707.34 m^2.

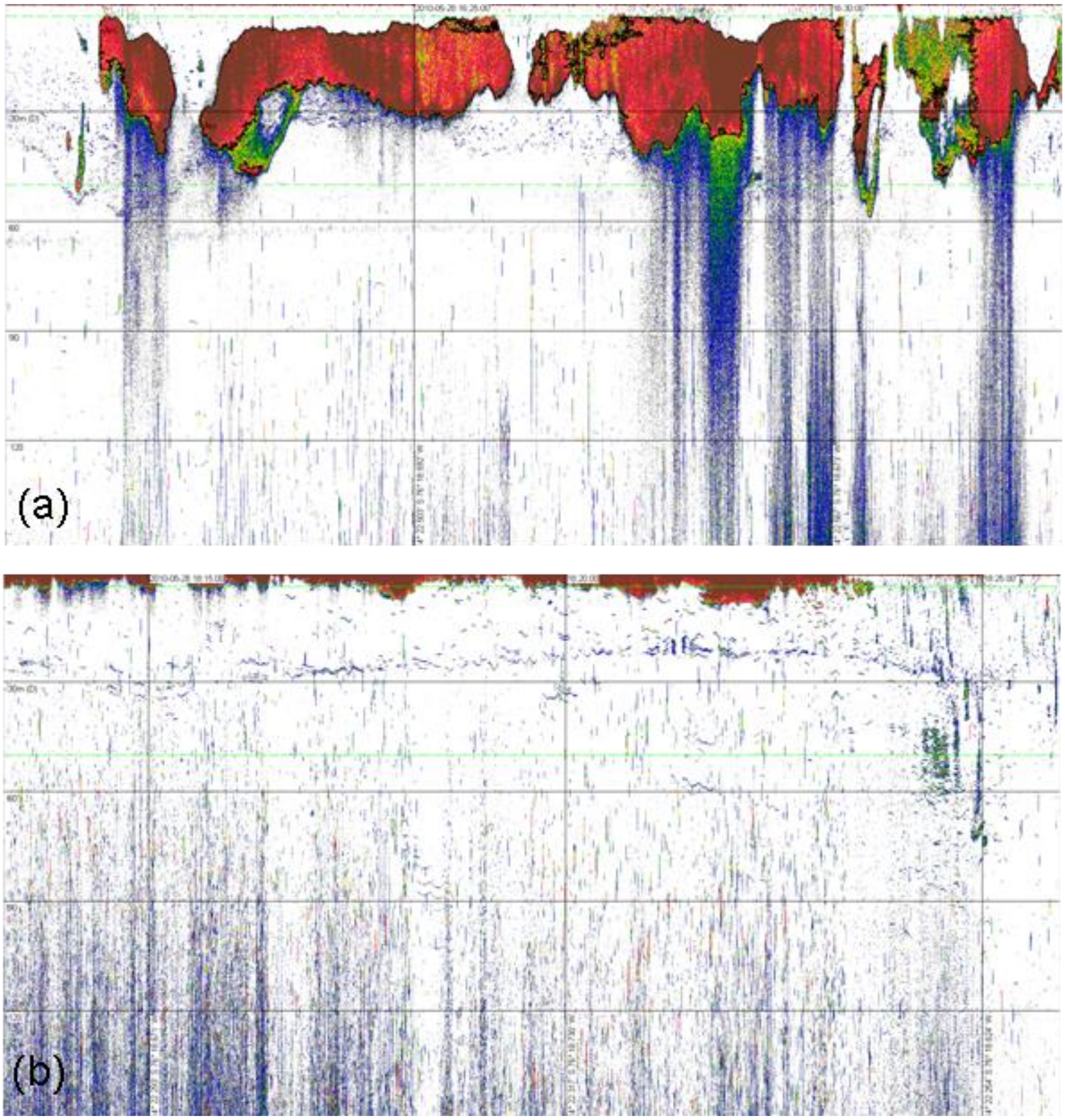
Thus, potentially easier to detect, assuming that penguins (a) forage at random and (b) forage only by visual means (neither of which is the case; Weimerskirch 2007, Wright 2011)

But, if the purse-seine vessel was to *remove some of the fish* in the process of setting their nets. Removing 70% (4188.79 m^3 x 0.3 = 1256.64 m^3) of the volume of the original shoal and breaking the remaining fish up into 10 shoals would yield the hypothetical result:

10 spheres of 125.66 m^3 each, with an area of 121.33 m^2 each, resulting in a combined surface area of 1213.27 m^2. Thus, there are now fewer fish and each of these 10 smaller shoals are harder to find (following the argument above) than the original large shoal.

Whether a reduction of 70% of a shoal is reasonable or not can be debated, but it would seem to be the case based on the below – which is reproduced from the Appendix of Bertrand et al. (2012):

"Figure S1. Echograms collected onboard an industrial fishing vessel exactly at the same location before a set (a) and just after a set (b). Red echoes correspond to anchovy aggregations; blue echoes correspond to plankton, bubble or acoustic shades. Horizontal lines delimitate 30m-depth segments, vertical lines delimitate 5 minutes-length segments. *Courtesy of M. Gutiérrez*."



Please note also, that this figure also does not appear to support the assertion that large single large schools are broken up into many smaller schools by fishing. At least, it does not appear to have occurred in this case. An investigation into whether the South African industry could start collecting data of this kind would be helpful.

Item 11

Furthermore, tight schooling behaviour makes feeding less efficient for planktivores so that pelagic fish will in any event need to spread out to feed (Eggers 1976). By working together, seabirds targeting fish schools benefit by disrupting the cohesiveness of predator avoidance tactics (Shealer 2002) and individual foraging success may increase with increasing group size (Götmark et al. 1986). Adult African Penguins tend to forage in groups (Frost et al. 1976, Wilson and Wilson 1990) and, based on observations of head-dipping movements that may signal readiness to dive, some synchronous diving, groups of penguins circling shoals of pelagic fish and the position of bite marks on fish (Wilson and Duffy 1986, Hockey et al. 2005), it has been inferred that at least some African Penguins forage co-operatively, *herding prey into dense schools (rather than splitting such schools) and then striking them from below (Wilson and Wilson 1990, Ryan et al. 2012). The conspicuously striped plumage of adult African Penguins appears to promote dense, defensive schooling of small pelagic fish, creating so-called 'bait balls' that are easier to exploit (Wilson et al. 1987). Co-operative foraging by groups of African Penguins that numbered between 25 and 165 individuals was recently observed in Algoa Bay (Ryan et al. 2012).*

Foraging strategies of seabirds are constrained by the dispersion and availability of different prey resources, the energetic costs of foraging and the rate at which food must be delivered to the nest during breeding (Lack 1968, Weimerskirch et al. 1994). Thus, prey supply has an important impact on bird biology, affecting activity, distribution, energetics, competitive abilities, breeding success and survival (e.g. Furness and Monaghan 1987, Montevecchi et al. 1988, Garthe et al. 1999). Since swimming is slower and more energetically expensive than flying (Pinshaw et al. 1977, Schmidt-Nielsen 1999), penguins require predictable food resources close to their colonies during breeding (Sherley et al. 2013). While volant seabirds (for example, albatrosses and petrels) may exploit food sources distant from their breeding sites (Weimerskirch et al. 1993, Péron et al. 2010), penguins are more limited in their foraging capabilities (Wilson 1985). For this reason penguins are especially sensitive as marine sentinels: they reflect the rate and nature of changes occurring in their marine environment (Boersma 2008). Effectively, any alterations in the marine environment caused by either natural phenomena and/or human-induced activities require flexible behavioural responses (Crawford 1998, Pichegru et al. 2010, Baylis et al. 2012) but African Penguins are constrained by their mode of locomotion and fidelity to sites once breeding (Hockey et al. 2005).

While this is interesting in a natural history context, it relevance to the problem under consideration is questionable. The reasons are given in detail in Appendix A of MARAM/IWS/DEC14/Peng/B4, and indicate why the only viable approach to solving that problem is provided by empirical approaches which measure the net effect of the numerous mechanisms at work.

An understanding of the species ecology is fundamental to placing the obtained data and results into a meaningful biological context and to construct effective conservation plans. To dismiss the above as merely 'interesting in a natural history context' is to miss the point. The research described in the original quote indicates that African penguins evolved to prey on small pelagic fish which in turn evolved to form large, dense schools as an anti-predator mechanism. Penguins do not require fishing vessels to break shoals of fish up into smaller shoals in order to forage successfully. In fact, the evidence indicates that it would make it more difficult for them to forage.

Fish might well be easier to exploit in small shoals, but not necessarily to easier locate. Much of the experimental evidence on foraging animal suggests that food is easier to find when it is clustered or aggregated in the environment, rather than when it is more dispersed (Wilson et al. 1987, Wilson 2003, Weimerskirch 2007). There are also questions about individual return rates for animals foraging in groups, and it is often the case that it is more profitable for individuals to forage in groups at sites where resources are abundant than alone at smaller resources aggregations (Beauchamp 1998).

So, fish in a small shoal would likely be easier to catch, but only once the shoal has been located. And if penguins and other seabirds foraged at random, then they would

of course be more likely to encounter many small shoals than one large one. Unfortunately, they do not forage at random, but use a variety of tools, depending on the spatial scale involved. For an example of a few of the mechanisms used by Northern Gannets, which also forage on pelagic resources, see Table 1 in Wakefield et al. (2013). Clark (1976) also only deals with the visual volume of school, but penguins may in fact only use visual contact with shoals over very small spatial scales. As they are in the water the whole time, penguins can even use scent (Wright et al. 2011) and can hear underwater (Frost et al. 1975). Large shoals would be easier to detect by smell and it is likely to be easier to hear feeding aggregations of other marine animals occurring on large shoals. Feeding aggregations on large shoals also last longer, so there is more chance of penguins finding them through local enhancement or social facilitation from seeing other seabirds feeding. Large shoals would also be easier to spot from the air, so other seabirds that predominately use vision (like Swift Terns or Cape Gannets) are probably more likely to locate large shoals. Cape Gannets use local enhancement to locate prey patches and are attracted to larger aggregations of conspecifics from further afield than they are to smaller aggregations (Thiebault et al. 2014). They also dive more in medium or large aggregations than small ones and appear to capture more prey when foraging in a group than alone (Thiebault et al. 2014). Penguins likely use the presence of other predators as a guide to where feeding is occurring and, because the number of foragers on a patch of food would progressively increase in time until the prey escapes or is depleted, large shoals would likely be easier to locate through local enhancement than small shoals.

In fact, in general, if you watch footage of predators of small pelagic fish -- dolphins in particular -- they generally do not try to split fish away from the a large shoal into smaller shoals, but actually pack the fish into very dense bait balls and often succeed in consuming every fish in the bait ball. To quote Simeone and Wilson (2003): “Wilson et al. (1987), summarising data on the feeding habits of Spheniscus penguins, report that school fish are herded by the birds swimming round the prey in tight circles before diving underneath and swimming rapidly up through the fish catching individuals as they do so. This behaviour is thought to be advantageous to the penguins because pelagic school fish, which are particularly hard to see from most angles due to the orientation of light-reflecting crystals in their scales (Denton 1971), can be best viewed as a silhouette against the water surface and because fish perceive the penguins least well if they are attacked from below (Wilson and Duffy 1986)”.

Furthermore, if it were shown that fishing results in more shoals (which has not been demonstrated), questions would still remain because (a) fishing clearly reduces the instantaneous biomass of fish in a target shoal, (b) unless predators happen to be right on the scene, the fish can probably quickly get back into another relatively large shoal and (c) it makes the prey environment more heterogeneous and therefore more difficult for foragers to predict where they might be aggregations of prey (seabirds are also fairly consistent in the areas they exploit; e.g. Irons 1998, Patrick et al. 2014).

Until empirical evidence is provided to show that purse-seine vessels break up shoals without substantially reducing the amount of fish available in the immediate vicinity, the purported mechanism for how purse-seine fishing may benefit foraging penguins must be viewed with caution.

Arguments based on comparisons with occurrences in Namibia

Item 12

Advantages postulated for colonial breeding in seabirds and water-birds include the acquisition of information that facilitates food finding (Erwin 1978, van Vessem and Draulans 1986) and it is noteworthy that, after Namibia's sardine collapsed, at Possession Island colonies of penguins fragmented as birds fed predominately on squid, which may have been present in densities too low to favour co-operative hunting (Cordes et al. 1999). The sine qua non for African Penguins hoping to breed successfully at colonies and after that to survive to moult will be a sufficient density of prey in the neighbourhood of colonies. If that is prevented by excessive catches near colonies, it will be detrimental to penguins.

Certainly, but the comparison with Namibia is quite misleading. There fishing in the 1960-80 period reduced sardine biomass by certainly one order of magnitude if not two. In contrast the impact of current fishing mortalities on the SA anchovy population, which dominates the small pelagic biomass off the Robben and Dassen island penguin colonies during their peak breeding and fledging period, and is in any case generally undercaught compared to the TAC awarded, is only slight (Butterworth and de Moor, 2010).

Arguments related to the length of closure periods

[The source of the quotation following is Pichegru *et al*. (2014).]

Item 13

The final design of the feasibility study was agreed by consensus but was not based on the ornithologists' best understanding of the biology of African Penguins. In particular, it was noted that the longevity of penguins, their delayed age at breeding and the long periods over which processes such as recruitment to colonies were expected to operate required long-term closures around colonies (see e.g. Crawford 2010, Pichegru et al. 2010b, Wanless and Moseley 2010) rather than rapid alternations of closures between "paired colonies", which were favoured in order to provide estimates of process error (Butterworth 2010). Therefore, the inconclusive results of the feasibility study to date are not entirely unexpected.

None of the arguments made here to support long-term closures are in any way clear. The mechanisms suggested need to be elaborated in mathematical form so that it is evident exactly what they are suggested to be and how they are proposed to operate, so that their plausibility can be properly assessed. This is a pre-requisite to any attempted justification of the final statement made. Despite frequent requests, no response to this request for the detail necessary to justify these concerns has been made available.

Arguments that the GLM formulation that provides the basis of the power analysis should be structured on the basis of some model selection criterion

[The source of the quotation following is Sherley (2014).]

Item 14

Using AICc-based model selection, I show that there is no statistical support to use the estimates from the models presented in Robinson and Butterworth (2014). By comparing the parameter estimates from the best supported models and those with year as a fixed effect, I show that the estimates drawn from the over-parameterized models presented by Robinson and Butterworth (2014) can be unreliable. In addition, in four of the six catch series analysed here, there is little evidence that the catches made in the vicinity of the island add substantially to the deviance explained over and above that explained by the overall measures of prey availability.

This quotation serves to summarise what is a complete failure to understand the purpose of the feasibility study and the method used to analyse the data forthcoming from it, as was first proposed in 2007 and later endorsed in slightly modified form at the 2010 international stock assessment review workshop as the form of analysis to be used. With short time series showing inadequate data contrast, it is obviously not going to be possible to obtain statistically significant estimates of the effect of catches on penguin response variables, given residual noise. The whole purpose of the feasibility study, to be followed perhaps by an experiment, was to extend data series to be able to attain such significance, with the initial feasibility study to indicate first how long this would probably take. The document from which this quotation is taken indicates that model selection under AIC_c in some cases excludes selection of catch as an explanatory variable. **But that is exactly what is to be expected for a limited data set** (as were those for a number of the penguin response variables at the time the feasibility study commenced) – roughly speaking the AIC criterion will, for a single additional estimable parameter, not select models where that parameter estimate is not statistically significant at the 15% level. Crucially though, a non-significant result does **not** necessarily imply absence of the associated effect, particularly given few data. It would hardly be precautionary to conclude in such cases that fishing has no impact on penguins. Obviously these are the very cases where a power analysis needs to be conducted to be clear on how much longer monitoring needs to continue to confirm whether a current non-significant catch effect might become significant, and such an analysis in turn clearly requires a model (desirably models to check robustness, as in Robinson's work) which includes catch as an explanatory variable. In essence then, the quotation's appeal to model selection exercises to effectively exclude catch from analyses in these cases is misguided and irrelevant.

Arguments that the effect of closure/non-closure of an area operates as a step-function

Item 15

Reservations have been raised in local discussions that as GLM analyses of the results from the feasibility study, such as those now reported in MARAM/IWS/DEC14/Peng/B4, did not contrast "open" and "closed" years but considered only the relative level of catch made within certain distances of colonies, the benefits of precluding fishing within the immediate precincts of islands may have been veiled.

The first GLM analyses of penguin response variables of this type (Brandao and Butterworth, 2007) was indeed structured in this "step function" manner – assuming the presence of absence of a multiplicative effect of fixed magnitude depending on whether an area around an island was open or closed to fishing. But in early discussions around that time, it was rapidly realised that this was inappropriate. The reason is evident from inspection of Figure 3, which shows the time series of sardine and anchovy catches made within different distances (and particularly within the sometimes closed area within 10 nm) of these islands. What is immediately apparent is that catches when this area when open span a wide range, including some very some years of very small catches. It would seem to make little sense to assume that the possible effect of these very small catches on penguin reproductive success is the same as that of much larger catches, but quite different to that in the absence of any catches. This is why the simplest form of relationship (linear proportionality) that avoids such a seemingly implausible assumption came to be used instead. Now clearly the real relationship between the response variable and catch in equation (1) above would not be exactly linear (indeed it is obvious that linearity cannot be extrapolated to levels where the catch rises to a very large proportion of overall abundance). But the assumption of an appreciable discontinuity (step-function) at the origin in the relationship is scarcely plausible – this amounts to claiming that just a single haul by a purse-seiner near an island during a year would (in expectation) result in an appreciable change in reproductive success at the colony that year.

This comment advocates the use of catch as a way of determining the experimental outcomes for closure, citing instances when catch was very low, implying that this is tantamount to sampling a closed island situation. There are two possibilities for explaining low catches near an island, 1) fish were present but fishing vessels decide not to fish there (the reasons for this are multiple and varied) or 2) there were no/few fish close to the island to catch. In practice, the two scenarios are indistinguishable. The statement above (from B10) is therefore is only true if the reason for the catch being so low relates to option 1) and is entirely unrelated to local abundance levels (Bergh 2014: MARAM/IWS/DEC14/A10). If on the other hand option 2) applies and local catches are on average low when local abundance levels are low, then there is really no idea as to what would happen under average local biomass conditions when catches are kept low by closures. If these catches being low do tend to coincide with low levels of local abundance, then our impression of the effect of closures on penguins only applies when local abundance is poor, so we would then tend to conclude that closures are bad for penguins.

Given this problem, there is the temptation to introduce biomass as a covariate to remove this complication Bergh (2014: MARAM/IWS/DEC14/Peng/A10) which builds on Bergh et al. (2014: MARAM/IWS/DEC14/PENG/A2) shows that such an approach is unbiased if the biomass covariate that is used measures local abundance without error. However, if this measure of biomass is an imperfect measure of local abundance, then serious biases arise, even at a local biomass to catch correlation of as low as 0.200. The first of these biases is that where there is no direct relationship between catch and penguins, a positive relationship will emerge. The second is that the estimate of the importance of biomass for penguins is negatively biased. Thirdly, in order to use this schema to predict what closure would do, one has to account for the fact that a reduction in catch causes a relative increase in biomass, with a positive knock on effect for penguins. Considerable care is required to implement this correctly. The approach only therefore seems reasonable if all the biases can be ironed out of the method and if the knock on effect mentioned is correctly calculated. Approaches such as are reported in Robinson (2013) to simply determine the impact of closure by setting the catch to zero in the regression equation do not address the knock on effect or correct for the inherent biases highlighted in Appendix A of Bergh (2014). There may nevertheless be some potential to revise the Robinson (2013) method to address the problems that are raised by Bergh (2014). The merits of this approach do have to be weighed up against the relatively more simple approach of using GLMs based on island, island closure, year (and month and even chick/nest) and pelagic biomass as predictors, in which the closure effect is more simply determined. Bergh (2014) recommends use of the latter approach, but there is a need for additional experimental time to provide estimates with sufficient power. The same may nevertheless be true of a revised Robinson (2013) approach.

Arguments that since "existing evidence" is that the effects of fishing are negative, this should be the default conclusion

Item 16

[The source of the quotation following is Weller and Sherley (2014).]

The correlation of various penguin survival parameters (here, breeding probability and survival rates) to available prey biomass is borne out by a large body of research (Annex 1; see also Crawford et al., 2014). Breeding success and timing, colony formation, and survival of various age classes have repeatedly been shown to be both positively and negatively driven by food availability. In this regard it is the conflicting finding of Robinson (2013), where fishing (regardless of the corresponding reduction in local food biomass) is interpreted as having a beneficial effect on penguin recruitment, that requires further confirmation due to its unexpected nature. Crawford et al. (2014) address this in detail.

Note first that Robinson's finding is mis-stated here – that has never been implied to apply "regardless", as is clear from responses made under Items 12 and 15 above, but rather to pelagic fish catches, abundances and fishing mortalities in the recent ranges to which the GLM analyses pertain (i.e. interpolation, not extrapolation). But more importantly, as pointed out in many places above, and in other documents in this MARAM/IWS/DEC14/Peng/B series, many of the arguments raised, by, for example, Crawford *et al.* (2014), are problematic. If earlier analyses had already indicated an appreciable negative impact of pelagic fishing close to islands on penguin reproductive success as clearly as implied, there would have been no need in the first case to have initiated a feasibility study to be followed perhaps by an experiment to determine the net effect empirically. In these circumstances it hardly seems appropriate to claim that the studies referenced should provide the default conclusion. To the contrary, as explained in Appendix A of MARAM/IWS/DEC14/Peng/B4, it is necessarily empirical studies which must lead to such a conclusion, and the previous agreement to pursue the island closure studies, whose results are reported in MARAM/IWS/DEC14/Peng/B4, surely implicitly renders those results the basis on which any default conclusion would be drawn

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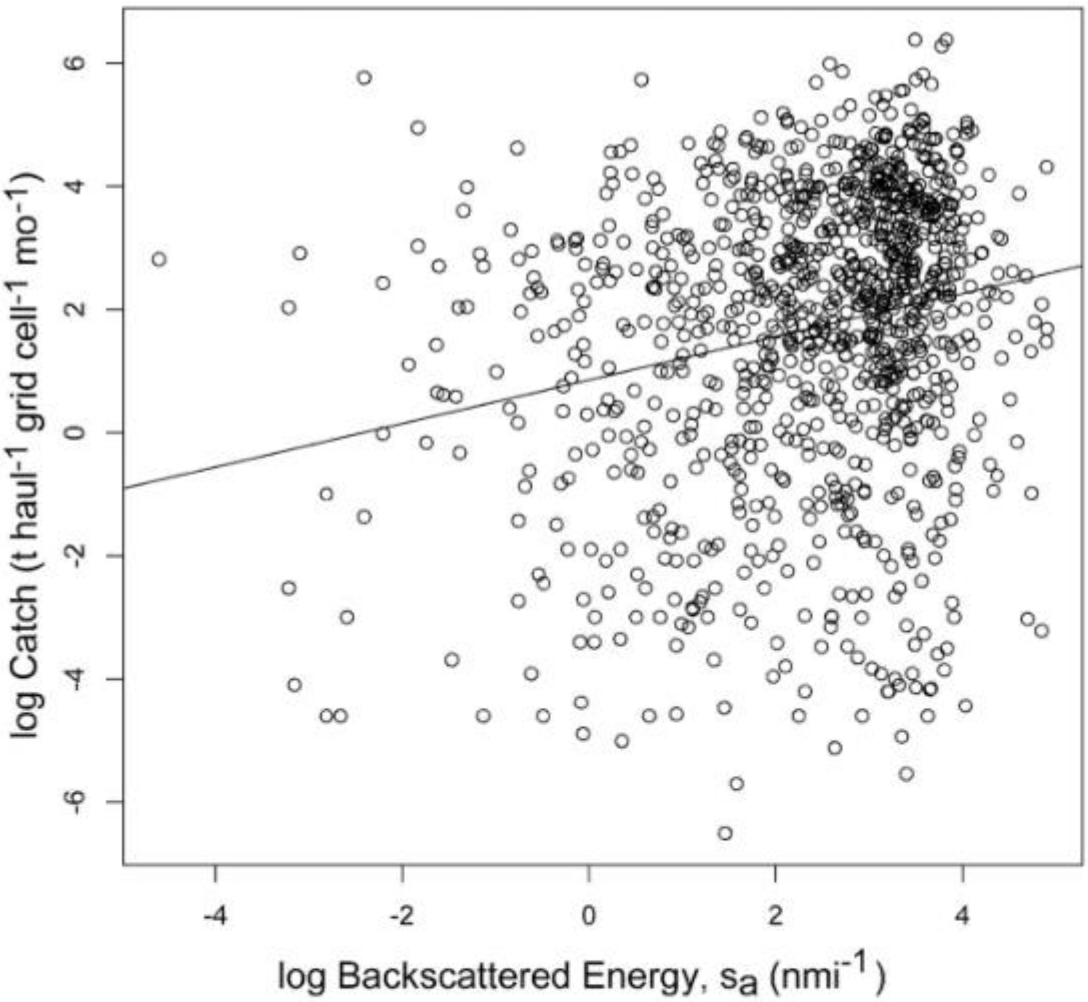

Figure 1: Relationship between raw catch data and hydroacoustic data that concur in time and space, as reported in Sabarros *et al*. (2012).

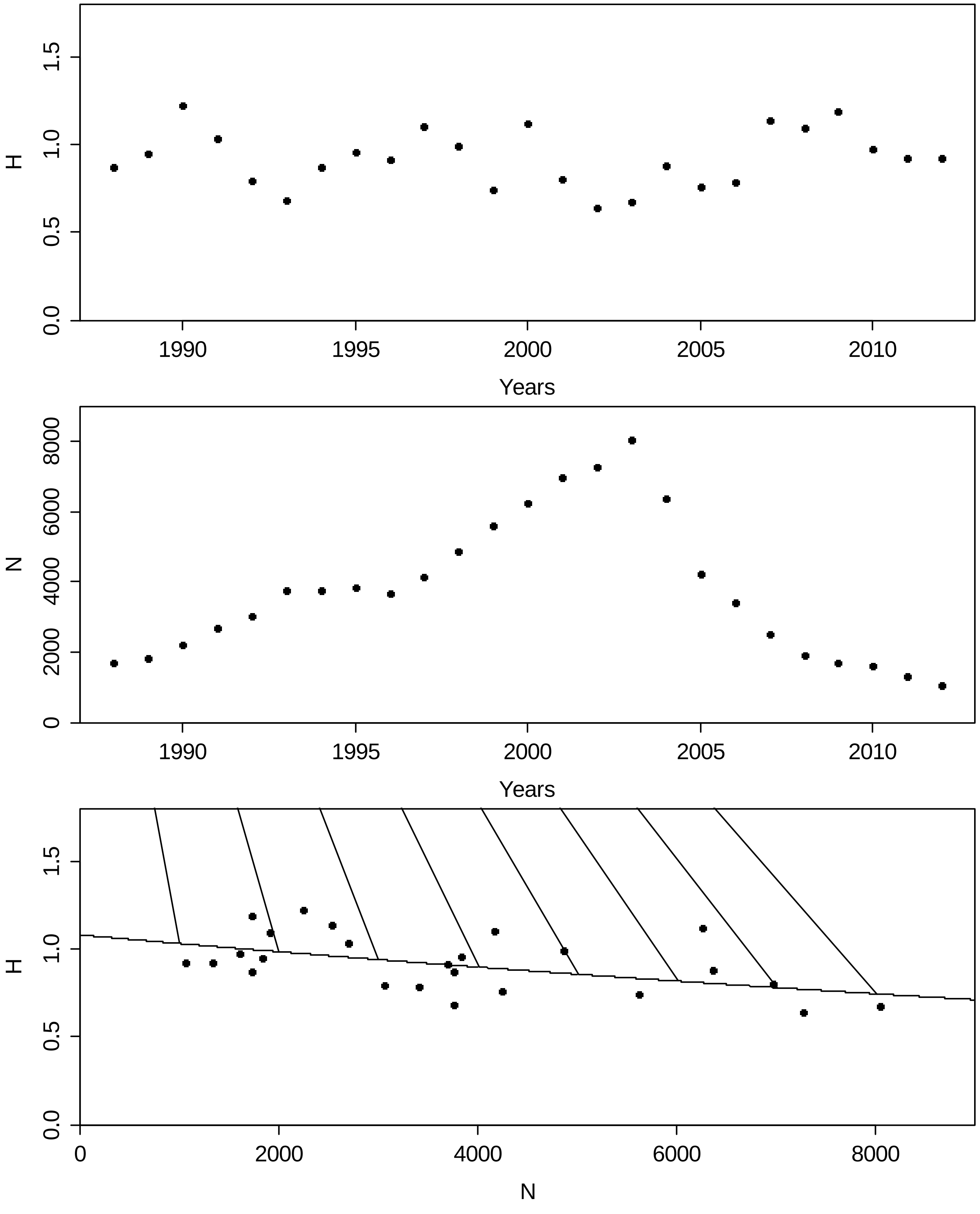

Figure 2: Top: Annual reproductive success H. Middle: Number of adult female penguins N. Bottom: Regression of $\ln H$ versus N.

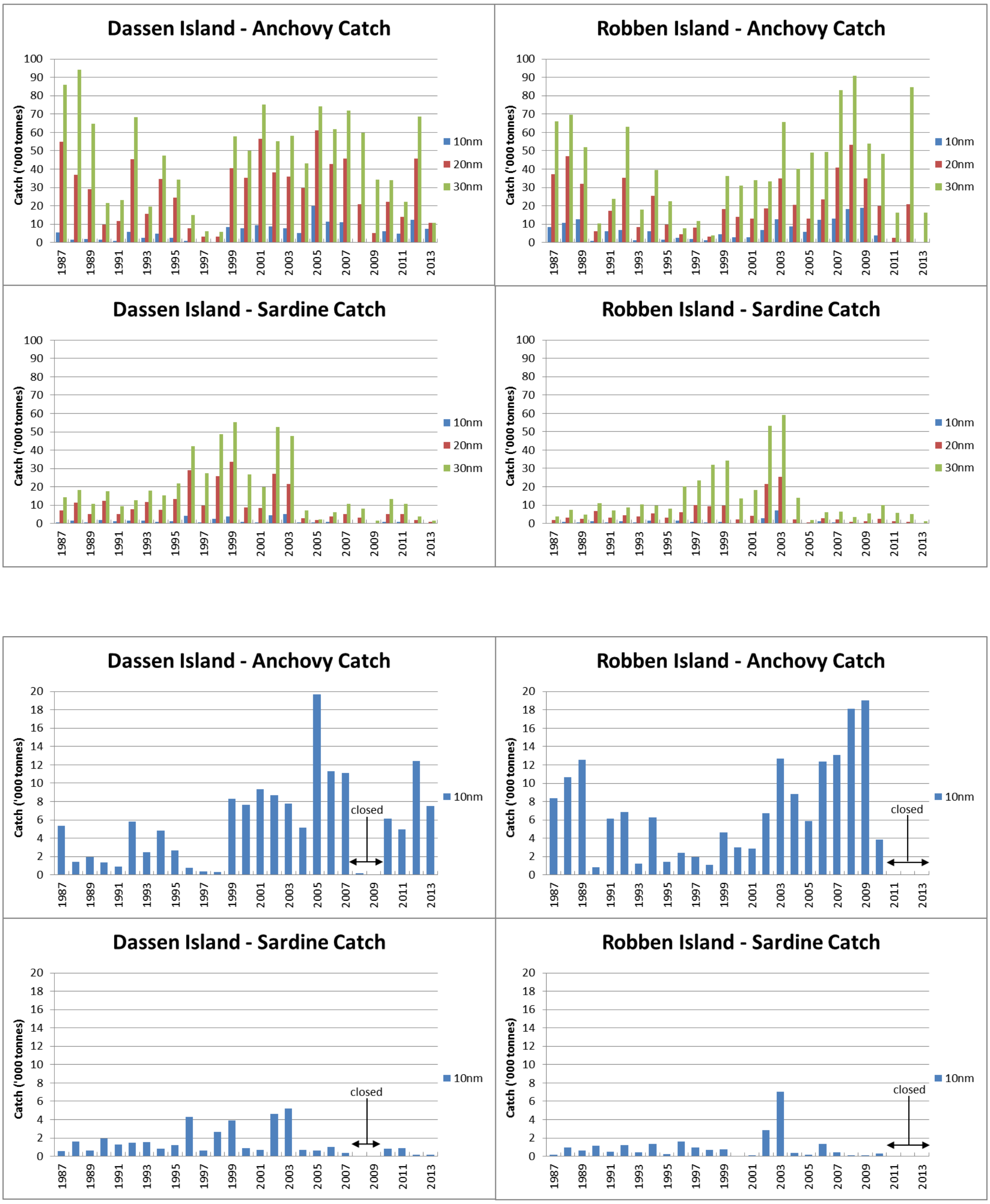

Figure 3: The time series of annual anchovy and sardine catches within 10, 20 and 30 nm of Dassen and of Robben Islands are shown in the upper group of plots. The lower group shows only the catches within 10 nm, together with indications of when these areas were closed to pelagic fishing.