Review of Science in Support of an Atlantic Bottlenose Dolphin Take Reduction Plan

by

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March 1, 2002

Executive Summary

1. Overall, the available data appear to be used appropriately. Genetic sampling, telemetry, stable isotope and photo ID data and other considerations were used appropriately to develop management units. Line transect data were used appropriately to estimate abundance, particularly those data collected in the February 2002 line transect survey. Observer by-catch data and logbook records were used appropriately to estimate by-catch in the management units.

2. Overall, the analyses conducted are appropriate. AMOVA was appropriately applied to test for genetic differentiation of bottlenose dolphins sampled from the different management units. The data analysis methods for the line transect data, especially those used in the February 2002 line transect survey were particularly appropriate. However, some improvements could be made for example, to the statistical methodologies applied to estimate summer and winter coastal habitat zones for bottlenose dolphin and to estimate abundances where ratio estimators were applied to both line transect and strip surveys.

3. Overall, the data appear to be adequate for the analyses conducted, although, only marginally. The basis for identifying individual stock units is still provisional because of the sparseness of the genetic sampling program in some regions, particularly on the southern half of the coast where it appears there could still be more than one distinct breeding population within the management units. There were large gaps in the coverage of the coast in the 1995 winter and summer line transect surveys that lead to the use of an ad hoc ratio estimator to estimate abundance using also the strip survey data where line transects were missing. Results from the ratio estimator are highly uncertain, and if used, corrections are needed to its variance estimator.

4. In most analyses, the main assumptions are stated and acknowledged. However, the overall protocol applied did not allow for many of the key uncertainties from the analyses to be quantitatively demonstrated. For example, for the PBR calculations, the PBR was calculated based on the best estimate of abundance and 95% confidence intervals for the best estimate. However, there were many assumptions made in the estimation of abundance, for example, that the proportion of the population observable in strip surveys was constant across between areas that were ignored in the final estimates produced. For example one cross-comparison of the ratio and line transect estimators for Cape Lookout to Murrell's Inlet suggested a potential 8-fold positive bias in an abundance estimate. The uncertainty in abundance estimates that arose from large uncertainty in key assumptions that were used to produce them were thus under-represented in final abundance estimates and 95% confidence intervals for them.

5. Except for the representation of uncertainty, the interpretation of the data and analyses are appropriate. The major features in the data and results from analyses are taken into account in the management units developed, abundance estimates, and by-catch mortality estimates. The limitations and sparseness of the data are to some extent taken into account in each major component of the analyses. In most instances, either the most scientifically justifiable assumption is chosen or the slightly more conservative assumption is taken.

However, as mentioned above, uncertainty in many key assumptions made in abundance estimation were under-represented in the estimates of abundance obtained. Monte Carlo approaches to providing probabilistic estimates of abundance that more thoroughly account for uncertainty in key assumptions and parameters required for abundance estimation could help to more adequately quantify uncertainty in abundance estimates.

Review of Documents on Stock Structure

Issues Addressed:

Are the *data used appropriately*?
Are the *analyses* conducted *appropriate*?
Are the *data adequate* for the analyses conducted?
Are *assumptions* inherent in the analyses *appropriately considered*?
Are the *interpretations* of the data and analyses appropriate?
Suggestions for alternative methods where better methods exist.
Do the Analyses represent the best analysis of the available information?

Paper #1: Preliminary stock structure of coastal bottlenose dolphins along the Atlantic coast of the US. June 2001. Staff, SE FSC

Main Conclusions Drawn in Paper:

(1) The main conclusion from this analysis of a variety of data was that the results suggested a high level of stock structuring of bottlenose dolphin populations along the Atlantic coast.

(2) The offshore range of the coastal morphotype of bottlenose dolphin was identified. The range differs north and south of Cape Hatteras. This range extends 12 km offshore north of Cape Hatteras in summer and 27 km offshore south of Cape Hatteras in winter.

(3) Several different bottlenose dolphin management units were identified along the Atlantic coast based on the analysis of genetic, photo ID and other data. .

Are the *data used appropriately*?

The available data were used appropriately. Genetic sampling data, photo ID data, stable isotope data, sighting location data from aerial surveys, telemetry data and other considerations were used appropriately to develop bottlenose dolphin management units along the Atlantic coast.

Genetic Analyses to delineate Spatial Distributions of Coastal Biotypes and Stock Structure

Are the *analyses* conducted *appropriate*?

The classification and regression analysis (CART analysis) used to analyze the large vessel genetic sample data to define boundaries of coastal and offshore biotypes were appropriate. The results, however, were of limited use because sampling coverage was temporally and spatially sparse. The logistic regression approach to estimate the proportion of each morphotype within the intermediate zone between the coast and offshore is also appropriate. However the results were not useful because sampling coverage was too low in this intermediate zone.

AMOVA (analysis of molecular variance) was appropriately applied to test for genetic differentiation of bottlenose dolphins sampled from the different management units. This is a common mainstream approach to test for differences in genetic composition among groups of organisms and it is appropriate for the data obtained and the questions of interest about stock structure. The test statistics computed from AMOVA and used to test for differences appear to be appropriate.

Are the *data adequate* for the analyses conducted?

It is acknowledged in the paper that there is an unequal distribution of samples both spatially and temporally. This applies to virtually all of the data used in the paper, particularly the genetic samples, the photo ID data and the stable isotope data. For the genetic analyses to evaluate the offshore extent of the coastal stocks, the inshore coverage of a large vessel survey carried out in the summers of 1997, 1998 and 1999 was relatively limited due to the sampling being limited to >100fm in the north and > 10 fm in the south. Much of the sampling occurred in offshore habitat outside of the main near-shore habitats for bottlenose dolphin. Of skin biopsies from 237 animals, only 9 were coastal biotypes and only two coastal biotypes were sampled from Cape Hatteras north. It was acknowledged that sampling was too sparse to define the boundaries of the range for coastal and offshore biotypes. Additional coastal biopsies were obtained from a few discrete areas within two – three km of shore. These areas included Jacksonville Florida, Charleston South Carolina, Virginia and one from New Jersey. These data were insufficient to provide precise delineation of the habitats for coastal and offshore biotypes in a CART (classification and regression analysis). Also, samples were mainly taken in summer, so it was acknowledged that the results could not be generalized to other seasons and used in combination with the winter survey of abundance south of Cape Hatteras.

To test for homogeneity in stocks among coastal regions, genetic data were collected for 389 coastal bottlenose dolphins in four different locations, Jackonsville, Florida, Charleston, South Carolina, (southern) North Carolina, and Virgina. Sample sizes were no less than forty-two animals for each location. The sample sizes per location appear to be sufficiently large for statistically testing for genetic differences among the locations. The number of locations tested was sufficiently large enough and geographically separate to test for regional differences in genotypes. However, as pointed out, there were large gaps in the biopsy sampling. For example, Georgia was not sampled and in areas such as Charleston county, and Jacksonville, Florida, mainly estuarine biopsies were collected and sampling in exposed coastal areas was minimal. It was pointed out that collections of samples from

even a larger number of locations and across multiple seasons could reveal (i) even a larger number of sub-populations, (ii) the southern extent of winter migrations of northern animals, and (iii) the extent of intermixing of coastal and estuarine animals. The choice of the locations for population sampling and testing was supported by photo ID observations of sets of animals whose movements were tracked in these regions. The photo ID record suggested that animals from each of the locations sampled were likely to be ones that came only from that area and not other areas.

Are *assumptions* inherent in the analyses *appropriately considered*?

The genetic data suggested a high degree of genetic differentiation among groups of bottlenose dolphin sampled along the Atlantic coast. The five different groups from different parts of the coast that were evaluated were found to have differences in mitochondrial and microsatellite DNA. It is generally the case that when genetic differences are found between different candidate stocks, this indicates that there is very low genetic exchange among the groups and the groups showing genetic differences should be treated as separate management units, as pointed out in the paper. However, should there be a failure to detect significant differences, this would mean that the results are equivocal and would not necessarily imply that the different groups of organisms evaluated are from a single homogeneous population. Thus, the genetic data were used in a highly appropriate manner to help define separate bottlenose dolphin management units.

Stranded animals were used in the genetic analyses with the assumption that the stranded animals did not wander outside of their normal range. Of the stranded animals, only animals with signs of human interaction were reported to be used and this measure was taken to reduce the chance that the animals had wandered outside of their normal range. However, there is still a possibility that some of these animals did wander outside of their normal range. Thus, it would be informative to test whether the genetic composition of stranded animals in a given region differed from those collected from dart biopsy and live capture. AMOVA methods analogous to a two-way analysis of variance procedure or nested analysis of variance procedure could potentially be used to test for differences in the genetic composition of animals that results from the sample source within a region. The lower differences in the test statistic, F_{ST} between Virginia and North Carolina could potentially be explained by some of the stranded animals in the Virgina sample possibly wandering in from North Carolina. Without testing this latter possibility, the paper did acknowledge that the lower value for this statistic could result from more mixing of samples between these two stocks or other stocks than between the other sampled stocks. The paper also suggested that the lower statistic obtained for this pairing could indicate more mixing between the two locations.

Photo ID Analysis

Are the *analyses* conducted *appropriate*?

The analyses of the photo ID data, providing that there is sufficient temporal and spatial coverage and the identification of individuals is to a rigorous standard also provide an appropriate approach for identifying migratory patterns of individuals from different areas and drawing inferences about stock structure. The photo-ID methods applied to coastal bottlenose dolphin are based on ones developed for other marine mammals and have been applied for decades to other marine mammal populations. Appropriate measures were taken to ensure that image clarity was to a high standard and that the dorsal fin was sufficiently distinctive for the animal to be positively photo-ID'd. Image processing was done with the use of a computer-assisted computer matching system. It does not appear that the processing of all images has been completed due to the large number of comparisons to be made (3,456, 751). Computer-identified matches are required to be verified by all of the contributors from each site. Multiple observations of some individuals indicated that individuals along different parts of the coast had different migratory habits as indicated above. The database for photo ID'd dolphins includes 2,377 dolphins and 323 have been identified at more than one site. Detailed spatial and temporal movements of matched dolphins will be investigated once the sighting histories have been analyzed.

The photo ID data, quite a large data set from several years of observations, has also been used appropriately to help identify separate breeding populations along the Atlantic coast. Records of the date and location for the sighting of individual dolphins showed distinct patterns in spatial and seasonal movements for animals from many different locations along the coast. Overall, it was found that movement of animals along the coast in southern locations (south of North Carolina) appears to have been much more restricted than those from northern locations (North Carolina and up). These observations were appropriately used to also help identify bottlenose dolphin management units and boundaries for them. They were also appropriately used to identify seasonal management units for the northern part of North Carolina, New Jersey and Virginia.

Are the *data adequate* for the analyses conducted?

Photo ID sampling has been concentrated more in estuaries and bays in the south and less outside of these areas. In contrast, in Northern areas, there has been more sampling in offshore waters and less in bays and estuaries. Thus, the lower movement rates observed in the south could result from observing only animals in estuaries and bays which may tend to migrate less than animals inhabiting waters outside of the bays and estuaries. This could suggest that there are two coastal sub-biotypes, one that inhabits bays and estuaries that tends to remain quite localized and another biotype that remains mostly outside of bays that migrates further up and down the coast and does so on a seasonal basis. Furthermore few researchers survey year-round. The management unit boundaries thus reflect the limits of the sampling resolution obtained in the photo-ID sampling and genetic sampling. Further investigation could reveal more refined stock structure for example within Georgia.

Are *assumptions* inherent in the analyses *appropriately considered*?

Analyses are still incomplete and it is appropriately acknowledged that the final analysis will need to account for the catalogue size and survey effort at different field sites.

Presumably these will be used to give indications of the reliability of the movement patterns inferred for animals originating at various sites along the coast. Lower effort and/or sample size at a given site would imply less certainty about the frequency of occurrence of animals from other sites at this site and the geographic extent of local individuals. The seasonal patterns in observer effort should also be taken into account for evaluating the reliability of inferences about seasonal movements of individuals. The spatial and temporal limitations in survey effort are well-acknowledged and there are recommendations for concentration for example on oceanic waters, particularly in winter months.

Analysis of spatial patterns in the distribution of sightings

Are the *analyses* conducted *appropriate*?

Given the sparse genetic sampling to delineate spatial boundaries between coastal and offshore biotypes, the analysis of spatial patterns in the distribution of sightings to evaluate the distribution of the coastal and offshore morphotypes (detailed in Garrison June 2001) is particularly appropriate. The spatial coverage of the transects from the coast was reasonably extensive along the coast, except for some blank spots due to poor weather conditions and the analysis of the relative proportion of sightings at each particular distance band from the coast could give an indication of the size of the area next to the coast inhabited by the coastal morph. An ad hoc non-parametric approach was taken to identify the distance off the coast most frequented by the coastal morph and thereby coastal strata for abundance estimation of the coastal morph. Significant spatial boundaries were identified from this analysis during both summer and winter.

Are the *data adequate* for the analyses conducted?

Line transect data from summer and winter aerial surveys were used for the spatial analysis of distribution in sightings to evaluate the distribution of the coastal and offshore morphotypes. There was non-overlapping spatial coverage along the coast in summer and winter. In the summer, the line transect data covered only the area from Cape Hatteras, North Carolina north to Sandy Hook, New Jersey. In the winter transects covered from Cape Hatteras, North Carolina south to Ft. Pierce Florida. It had been found that there was a gap in spatial distribution at a distance of 12km from shore north of Cape Hatteras in the summer and a gap at a distance of 27 km from shore in the winter south of Cape Hatteras. Because there was no coverage on both sides of Cape Hatteras in both summer and winter, it is unclear whether the distances of 27km and 12 km can be said to be seasonal for each region or permanent. i.e., until a winter survey is conducted both north and south of Cape Hatteras, it cannot yet be deduced that the 27 km distance applies only in winter in areas to the south of Cape Hatteras. It cannot be certain that this distance reduces to only 12km to the south of Cape Hatteras in the summer. Likewise, based on the survey designs implemented, it cannot be deduced that the 12 km distance in summer will revert to a 27 km distance in winter north of Cape Hatteras since there were no surveys north of Cape Hatteras in the winter, at least until February 2002. Also, these line transect surveys were

conducted only in 1995. There was no temporal replication of these line transect surveys until February 2002. Thus until there is further replication of the transect surveys across years, the extent to which the boundaries are constant from year to year cannot be evaluated. However, in the biopsy data, it is pointed out that no animals sampled inside of 27km south of Cape Hatteras were of the offshore type and this to some extent lends support to setting a boundary no further than 27 km offshore for the coastal biotype. The same goes for the 12 km distance for areas north of Cape Hatteras.

Are *assumptions* inherent in the analyses *appropriately considered*?

In the spatial analysis of the distribution in sightings to evaluate the distribution of the coastal and offshore morphotypes assumptions were appropriately considered. However, the assumption that the boundary identified in the summer north of Cape Hatteras holds also for the area to the south of Cape Hatteras in summer was given relatively little consideration. The analogous assumption that the boundary identified in the winter to the south of Cape Hatteras also holds for the area to the north of Cape Hatteras in the winter was also given little consideration. More recognition needs to be given to the need for more extensive line transect surveying both north and south of Cape Hatteras in both summer and winter to more rigorously test assumptions about offshore boundaries for the abundance estimation of the coastal biotype.

Stable Isotope Analysis

It is stated that stable isotope ratios of carbon, nitrogen and oxygen were measured in teeth from bottlenose dolphins ranging from Virginia to South Carolina throughout the year. However, it was not stated how many samples were taken, nor how many in each season, nor how many from each section of this part of the coast. On Figure 6, it appears that there were approximately 30 samples from Virginia and these were taken after 120 Julian days. There were approximately 60 samples from North Carolina and these were taken throughout until about 310 Julian days, though there were observations until about 320 Julian days. There appear to have been about 11 South Carolina samples and these were taken between 60 and 240 Julian days. Thus, it does not appear that the coverage of each area was entirely throughout the year and the number of samples was quite small in some instances, especially for South Carolina. The manner of collection of the tooth samples was not stated, i.e., whether they came only from stranded animals, and if so whether there was an attempt to sample animals most likely not to have wandered from other areas. It was also not stated how many samples would be required to make reliable inferences about stock differentiation. Thus it is not possible to evaluate whether sufficient data were obtained to be able to use the stable isotope analyses to provide evaluations of stock structure. The symbols on Figure 7 which plots the spatial distribution of samples showing different stable isotope ratios for oxygen were indistinguishable, so it was not possible here to visually evaluate the validity of the conclusion that depleted oxygen samples were found from Cape Hatteras to Cape Look-out from mid-Feb. to March. A color PowerPoint slide of the same figure (provided by Dr. Aleta Hohn (NEFSC), Figs. 1 and 2) however gives a very strong indication that oxygen depleted samples are heavily clustered in this part of the

coast in this period. The difficultly of viewing relevant results was also difficult because on Fig. 7 there were panels showing spatial distributions of samples for Dec-Feb. and Mar.-May but none specifically highlighted the period from mid-Feb. to March. The fraction of observed animals with low stable isotope ratios in each different region in this period and other periods was also not reported. No statistical tests were reported to have been conducted to test whether the null hypothesis of homogeneity in stable isotope ratios across areas could be rejected on a seasonal basis or across the year. It is mentioned that sample sizes were quite low so the statistical power (i.e., probability of correctly rejecting null hypotheses) of any statistical tests would also be quite low. Methods analogous to AMOVA could potentially be used to test for differences in isotope composition among animals from different pre-assigned coastal regions.

Telemetry

Sample sizes are quite low (15 animals fitted with transmitters) and periods of observation quite short (appear to be 3 months or less per animal) so these analyses are currently of limited use in delimiting stock boundaries. However, these limitations are acknowledged. It is however concluded that based on the telemetry data, not all bottlenose dolphins migrate but that some remain in estuaries. This conclusion remains tentative because of the short duration of the transmitter remaining on the animal. It could be that a longer period of observation or observation during a different set of seasons would have lead to the observation of a migration on these same animals. Maintaining electronic tags on an individual animal for up to a year would be far more preferable, but limitations in the technology might prohibit such operations. It might be possible to use these techniques to more accurately map the winter and summer offshore boundaries of the coastal bottlenose dolphin, if sufficiently large sample sizes could be obtained per season.

Are the *interpretations* of the data and analyses appropriate?

With the limitations in the spatial and temporal extent of sampling kept in mind, the interpretations of the analyses in this paper are appropriate for the definition of provisional management units for coastal bottlenose dolphin along the Atlantic coast. There is very clear evidence for at least four different stock units. It is also appropriately acknowledged that the stock units set up in this paper will change over time as sample and survey data and analyses are updated. Some conclusions from some of the analyses seem a little too strong. For example, there is a conclusion that telemetry data demonstrate that some animals do not migrate. This is despite the relatively short period that transmitters are placed on these animals. Additionally it is concluded that there is isotope evidence (based on depleted oxygen observations in teeth) that there is a distinct stock that occupies coastal waters in central North Carolina in winter. This is despite the absence of statistical tests of hypotheses regarding the proportion of animals with particular isotope ratios in different regions. However, overall, designations for the management units are well supported by the various sources of evidence. For example the identification of a winter mixed stock unit in northern North Carolina is reasonably well supported by the genetic data and photo ID data on migrations of northern animals. The assumption that the offshore distribution

for example in the Northern Migratory unit shifts from 12 km in the summer to 27 km in the winter is tentative and requires further surveying to evaluate the validity of these assumed boundaries. Mistakes in their delineation could result in substantial biases in abundance estimates in winter and summer.

Do the Analyses represent the best analysis of the available information?

Overall, the available data have been thoroughly analyzed to construct provisional management units. Improvements could be made to some of the methods of analysis but it is unlikely that they would give rise to a substantially different set of management units. Perhaps the most influential detail in the management units constructed is the designation of offshore boundaries for the management units. This was done using an ad hoc methodology whose estimation properties have not been very well explored or understood.

Paper #2: Seeking hiatus in sightings for bottlenose dolphin during summer and winter aerial surveys. June 2001. by Lance Garrison.

Are the *data used appropriately*?

As mentioned above, in the absence of suitable genetic samples, it was appropriate to try to use the available transect survey data to attempt to delineate offshore boundaries for the coastal biotypes.

Are the *analyses* conducted *appropriate*?

The general statistical approach to the analysis appears to be appropriate. This approach computes the cumulative density function of distances from shore of sightings of bottlenose dolphins along the line transects. It also computes a cumulative density function of potential observations of distances from shore of sightings if the sightings along each transect happened to be uniformly distributed along the transect. The comparison of these two cdfs appears to be a sound approach to identifying the hiatus in the distribution of the coastal and offshore biotypes. In the evaluation of the observed densities of observations as distance from shore increases, the comparison of the former with the latter cdf as pointed out controls for the relative amount of sampling effort at each distance from the shore. Deviations in the empirical cdf from the randomly simulated one should indicate deviations from a uniform spatial distribution of animals. The empirical cdf is much steeper and has a higher derivative than the random one nearest to shore indicating higher densities of animals near shore. The point at which the derivatives of the two cumulative density functions becomes equal as distance from shore increases should provide an indication of the edge of the main distributions of coastal biotypes. A non-parametric approach was taken to evaluate the distance from shore at which the derivatives for the two different cdfs became equal to each other. A comparison of cumulative of random and empirical cdfs was conducted across the entire coastline sampled for each replicate transect survey in the summer and the single one carried out in the winter. The analysis conducted should provide reliable results under the following conditions.

(1) *The spatial coverage of the transects is extensive and representative of each of the different regions along the coast if it is of interest to obtain an unbiased estimate of a mean "coastal biotype-hiatus distance" from the coast across all regions sampled.*

(2) *The transects extend far enough offshore to go beyond the extent of coastal biotypes.*

(3) *Distances from shore of each observation can be measured accurately for each observation when the transect is not perpendicular from shore.*

(4) *There are sufficient numbers of transects to allow for precise and unambiguous estimation of the break point in near-shore coastal abundance.*

(5) *The distance from shore of the coastal biotype main habitat zone does not vary appreciably along the entire coast for each survey in given season.*

(6) *The distance from shore of the coastal biotype main habitat zone does not vary appreciably across years for a given season due to inter-annual variation in e.g. oceanographic conditions.*

Are the *data adequate* for the analyses conducted?

This will be evaluated based on the following set of assumptions.

(1) *The spatial coverage of the transects is extensive and representative of each of the different regions along the coast if it is of interest to obtain an unbiased estimate of a mean "coastal biotype-hiatus distance" from the coast across all regions sampled.*

There were some breaks in the survey transect coverage in the winter survey particularly in the mid-sections of the coast off of Georgia and South Carolina. These breaks were said to have resulted from poor weather conditions. Given that they occurred mainly in the central part of the coast, it could be that the central coastal area is under-represented in the estimation of a mean distance from shore across coastal regions.

(2) *The transects extend far enough offshore to go beyond the extent of coastal biotypes.*

The transects in the northern and southern extents of the winter survey tend to extend little beyond the 12 km boundary and on the southern end of the survey area several observations lie at the eastern end of the transects. This under-sampling of areas beyond 12 km from shore in the south could thus lead to a negative bias in the coastal biotype boundary. The same pattern was observed for the winter survey. In the northern and southern ends of the sample area, survey transects were shortest and did not extend much beyond the 27km boundary. On many of these, observations were found near the eastern ends of the transects. Moreover, even on transects extending well beyond 27km, in the central sampled area where there were transects, there were many observations extending along the full length of these transects. This gave rise to a second mode in the derivatives of the empirical cdf and opens the question whether the coastal biotype boundary should be on the

right edge of the first mode or the right edge of the second mode. Genetic sampling of animals in winter in this particular offshore zone would aide the interpretation of this secondary offshore sighting mode.

(3) *Distances from shore of each observation can be measured accurately for each observation when the transect is not perpendicular from shore*. In the calculation of the empirical cdf it was not stated explicitly that the distances from shore computed for each observation were the absolute distance from shore or the along-transect distance from shore. Also, to make the random cdf comparable, the distances along the transects would need to be converted to the absolute distance from shore. It is also not stated what method was used to compute the distance from shore of each observation when transects were not perpendicular to shore. This is not a trivial computation for instances when the shore-line is convoluted and the transect is not running perpendicular to shore. The method used to compute distances of observed and simulated observations from shore should be described to indicate that the distance calculations used were obtained in a rigorous manner and not taken simply as the length along the transect. At the top of p. 2 it is stated "To provide a null distribution for comparison, I randomly assigned each sighting to a 1-km bin along the length of its individual transect line. For example, an actual sighting at 3km (actually > 2 and <=3) along a transect 54 km long could obtain a random value between 1 and 54 km from shore." It should be added "provided that the transect is perpendicular from shore". If only the distance along the transect was used to calculate the cdfs, then the results obtained indicate only distances along the transect lines, irrespective of their alignment to the shore. The distance values used to delineate summer and winter habitat boundary values would be incorrect and biased. This would be so for both the winter and summer surveys because many of the transects are not aligned perpendicular to shore in both surveys. However, NMFS has indicated that distance from shore was estimated from GIS ARCVIEW software and that distance along the transects was not taken as a proxy for distance from shore.

(4) *There are sufficient numbers of transects to allow for precise and unambiguous estimation of the break point in near-shore coastal abundance.*

The decision rule used to identify the break in near-shore densities of coastal animals is where the value for the derivative of the empirical cdf first falls below that of the one generated from a uniform random distribution. For this to provide reasonably precise and reliable results, there must be a fairly large number of transects and high density of observations at each distance bin from shore. Viewing the Figures 3-6 there appear to be many bins for which there are no observations. The successive derivatives of the empirical cdf thus produce a very jagged surface (Figs 3b-6b). This would have smoothed if there had been more observations per bin. Because the derivative surface is very jagged, the distance at which there is a break in the density of coastal animals cannot be precisely determined. A parametric estimate of the relationship between derivative of the cdf and distance from shore might provide a more reliable estimate of the offshore break in coastal biotypes. This could be provided by either GAM modeling with the use of environmental covariates or co-kriging also with environmental covariates. Alternatively a bootstrap approach could be applied to estimate a confidence interval and median value for the

offshore break in density of coastal animals. For each survey, the transects could be randomly resampled with replacement, and sighting observations along each transect could be randomly resampled with replacement and the empirical cdf and the derivatives at distance from shore recomputed multiple times.

(5) *The distance from shore of the coastal biotype main habitat zone does not vary appreciably along the entire coast for each survey*.

It is quite likely that the offshore extent of coastal biotypes varies along the length of the sampled coast. If there were sufficient numbers of transects in each coastal region or management unit then the offshore extent could be estimated separately for each management unit. If the three summer surveys were pooled, this test could potentially be feasible. Bootstrapping could be used to compute confidence intervals for the break for each management unit.

(6) *The distance from shore of the coastal biotype main habitat zone does not vary appreciably across years for a given season due to inter-annual variation in e.g. oceanographic conditions.*

Because there was only one sample year used in the analysis this was not possible. But with the 2002 line transect survey, differences between the two years could be evaluated.

Are *assumptions* inherent in the analyses *appropriately considered*?

The six assumptions appear to be considered only in a limited fashion. The limitations in the coverage and sampling intensity of the 1995 line transect survey appear to be acknowledged. For example, it is concluded that the results of the hiatus analysis are tentative at best. There is also a recommendation for future biopsy survey efforts to attempt to more accurately delineate the spatial boundaries between coastal and offshore biotypes.

Are the *interpretations* of the data and analyses appropriate?

It is concluded at the end of the paper that "the spatial boundaries identified in this analysis are therefore reasonable habitat areas for abundance estimation of the coastal morphotype of bottlenose dolphin and likely include the bulk of the population." The strength of this conclusion could be debatable until more thorough data analysis methods are developed and applied to the sighting location data from the line transect surveys and additional, more intensive line transect surveys and biopsy surveys are carried out.

Suggestions for alternative methods where better methods exist.

As mentioned above, some alternative methods of analysis that improve the statistical rigour of the analysis could include extension of the current non-parametric approach with the application of bootstrap methods, or a switch to parametric approaches such as GAM modeling or co-kriging that could also incorporate environmental covariates to model the offshore break in abundance of coastal biotypes.

Do the Analyses represent the best analysis of the available information?

The analyses represent a reasonable first attempt at analyzing the available line transect data to delineate offshore boundaries for the coastal biotype. However, there is considerable room for improvement. Due to the relative sparseness of sampling transects, the improved methods could provide potentially more reliable estimates of the offshore break in the coastal biotype. New analyses using data from new surveys could also lead to a marked revision in the provisional winter and summer boundaries provided by the analysis.

Paper #3: Abundance estimates for Atlantic bottlenose dolphin: Combining strip transect data and line transect abundance estimation. Lance Garrison and Aleta Hohn.

Are the *data used appropriately*?

Large gaps in line transect aerial survey coverage exist for winter and summer surveys of bottlenose dolphin abundance in 1995. The two surveys do not overlap temporally or spatially. During the summer the coverage was between Cape Lookout, North Carolina and to the North Carolina/Virginia border. In the winter the coverage was between Cape Hatteras, North Carolina and Florida. The coverage does not permit estimation of abundance in the northern and north North Carolina management units in summer and winter. However, another set of "strip" surveys was conducted parallel to the coastline during 1998 and 1999 that extend between the North Carolina/South Carolina border and the Maryland/Delaware border. These latter surveys cannot be used for abundance estimation because they cover only a very narrow strip along the edge of the coast. A ratio estimator was applied to estimate the abundance of animals in the Northern and north North Carolina management units in winter and summer. This used the density estimates from line transect surveys where there was coverage and encounter rate estimates from the strip surveys and also where there was no line transect coverage. In principle, these data were appropriately used for abundance estimation, given the incomplete coverage of the line transect surveys. However, to provide reliable estimates of abundance, various conditions must hold and these are explained below.

Are the *analyses* conducted *appropriate*?

The density in the area not covered by the line transect survey, D_y, was estimated by:

$$D_y = (E_y/E_x)D_x$$

where E_y and E_x are the mean encounter rates in strata x and y, respectively, and D_x is the estimated mean density in stratum x. For the estimate of D_y to be unbiased, the following conditions must hold.

(1) The proportion of the population in the near-shore section of the coastal waters strip surveyed must be the same between the area sampled, x, and area not sampled, y, by the line transect survey, i.e., $p_x = p_y$.

(2) The proportion of the population that inhabits both area x and area y, that resides in area y has not changed between the time when the line transect survey was implemented and the time that the strip survey was implemented ($d_{y/x,\,t1} = d_{y/x,\,t2}$) where

$d_{y/x,\,t1} = D_{y,t1}/ D_{x,\,t1}$ and $d_{y/x,\,t2}= D_{y,t2}/ D_{x,\,t2}$.

(3) The probability of sighting animals in the strip survey must be the same between the area surveyed and area not surveyed by the line transect survey ($q_{S,y,\,t2} = q_{S,x,\,t2}$).

(4) The estimate of the sighting probability in the line transect survey in area x must be unbiased ($q_{T,x,\,t1}$).

Thus if all of these factors are taken into account, we have:

$$(1)\quad \hat{D}_{y,t2} = \frac{q_{S,y,t2}\,p_{S,y,t2}\,D_{y,t2}}{q_{S,x,t2}\,p_{S,x,t2}\,D_{x,t2}}\,q_{T,y,t2}\,D_{y,t1}$$

Rearranging slightly we have:

$$\frac{\hat{D}_{y,t2}}{q_{T,y,t2}\,D_{y,t1}} = \frac{q_{S,y,t2}\,p_{S,y,t2}\,D_{y,t2}}{q_{S,x,t2}\,p_{S,x,t2}\,D_{x,t2}}.$$

This equation should make it obvious that the assumptions 1, 2, 3 and 4 above must hold for the ratio estimator of density in area y to be unbiased.

The summer and winter abundances of animals in the areas not sampled by line transect ($area_y$) are computed by multiplying the ratio density estimate by the spatial area of area y. This calculation uses the following assumption:

(5) The offshore boundary of 27 km in the winter computed only from winter line transects south of Cape Hatteras also holds for the area to the north of Cape Hatteras ($b_{wN} = b_{wS}$); the offshore boundary of 12 km in the summer computed only from summer line transects north of Cape Hatteras also holds for the area to the south of Cape Hatteras ($b_{sN} = b_{sS}$).

The variability in the estimate D_y was obtained by the ratio in the sampling CVs:

$CV_y = (CV_y/CV_x)CV_x$

While this latter equation appears to be an intuitive method to calculate the CV in the density estimate D_y, it is ad hoc and not correct, according to probability theory. An approximation of the correct variance estimator of D_y could be worked out using the delta

method (i.e., Taylor series expansion, see G.A.F. Seber (1982), p. 7-8, "The estimation of animal abundance", Charles Griffin and Co. Limited). Another simple approach would be to use Bootstrapping. This could take a few different forms. A non-parametric - unconditional bootstrap approach (Smith et al. 1991, Can. J. Fish. Aquat. Sci. Spec. Publ., p. vii) could involve randomly resampling the strip transects in areas y and x, and also randomly resampling the line transects in sampled area x, computing the resulting "new" values for E_y, E_x, and D_y, and computing a new value for D_y, and then repeating this procedure to compute a distribution of estimates of D_y. A parametric - conditional bootstrapping approach could assume that E_y, E_x, and D_y were lognormally distributed. The distribution of estimates for D_y could be computed from repeatedly re-simulating estimates of E_y, E_x, and D_y, and, from each new set of values for these latter quantities, computing a new value for D_y. In an Excel spreadsheet I tried the latter approach, using inputs for the winter northern management unit, and found that the bootstrap estimates of variance for D_y were appreciably larger (CV=1.02) than those obtained by the ad hoc equation above (0.629). The median from the parametric bootstrap was 0.434 compared to 0.425 in the paper. Therefore, due to an incorrect method for variance calculation, uncertainty in the estimates of abundance in the management units where the ratio estimator has been used is under-estimated. Because these abundance estimates and the uncertainty estimates for abundance are used in the calculation of PBR, the PBR estimates obtained would have been obtained using uncertainty estimates biased too small. The net result would be that all other things being equal, the PBR estimates would be biased too high.

Are the *data adequate* for the analyses conducted?

The data appear to be only marginally adequate for abundance estimation. This is because their use requires that four assumptions hold, as outlined above. To a small extent the combined effect of the assumptions was tested in the paper by comparing ratio estimates for particular sections of coast for which line transect estimates could also be obtained. In some cases the estimates were quite similar in others not. For example, for the area between the North Carolina/Virginia border to Cape Hatteras compared to that north of the North Carolina/Virginia border the ratio of encounter rates was calculated to be 1.309 compared to 1.489 for the ratio of densities from line transect estimation. In contrast, for a pair of more southerly sections of coast these corresponding ratios were 0.016 for the densities compared to 0.114 for the encounter rates, indicating that the biomass estimates for this region have an 8-fold positive bias. This indicates a major deviation from the assumptions of the ratio estimator. Data within 1995 line transect surveys could be further re-evaluated to test some of the assumptions. For example, in the line transect data, the proportion of the population in the area surveyed by the strip survey could be evaluated for adjacent pairs of areas along the coast to see whether assumption 1 could be rejected based on the available data.

With the recent February 2002 line transect survey which covered the entire range of the management units, there is no longer a need for a ratio estimator for winter abundance estimation. At the same time, the appropriateness of assumptions 1, 3, and 4 for the ratio

estimator could be more rigorously tested. Assumption 2 would require simultaneous strip and line transect surveys of the same sections of coast with more than one set of observers in the line transect survey, as in the February 2002 line transect survey. Assumption 5 at least with regards to the winter offshore boundary could also be tested more rigorously with the February 2002 data.

Are *assumptions* inherent in the analyses *appropriately considered*?

As mentioned above, the authors recognize that there are some fundamental assumptions being made with the use of the ratio estimator. Analogous assumptions to the ones stated above are outlined on the bottom of page 4 and the top of page 5 in the paper. The authors also point out that coverage by the strip transect survey was not complete in summer and winter yet density estimates were extrapolated across the entire region with the assumption that density values were constant across the regions. The authors appropriately test the combined effect of the assumptions by comparing ratio estimates of abundance with line transect estimates, in sections of coast where both sets of estimates can be obtained. However, in the end, only point estimates of abundance with incorrectly calculated confidence intervals were provided. The sensitivity of estimates to deviations in assumptions was not evaluated and formally demonstrated. This would be appropriate in order to provide decision makers with a better understanding of the large uncertainty in the estimates provided due to the several key assumptions inherent in the ratio estimators used.

Are the *interpretations* of the data and analyses appropriate?

The authors conclude that "Despite the uncertainties, the current analysis fills significant gaps in the abundance estimates." For summer, a minimum abundance estimate is provided for North North Carolina and Southern South Carolina management units. For winter, a complete estimate is provided for the North Carolina region. Due to the tenuous assumptions made and the failure to corroborate the assumptions in the analyses, the estimates provided are at best marginally appropriate for management purposes. Improved evaluations of the uncertainty in the estimates would be facilitated by further analysis of the range of estimates that could be obtained based on plausible deviations in the various assumptions made.

Suggestions for alternative methods where better methods exist.

As mentioned above, the variance estimates obtained needs to be corrected for example with the application of a bootstrap variance estimator. Additional, Monte Carlo simulation methods could be used to further quantify the uncertainty in the ratio estimates. This could be done by using data analysis and expert judgment to specify pdfs for the various "bias" parameters shown in equation 1 above (For example, see Branch, 2001. S. Afr. J. Mar. Sci. 23: 181-203). A Monte Carlo simulation could then be applied to evaluate the resulting distribution of a "bias corrected" ratio estimate of abundance.

Do the Analyses represent the best analysis of the available information?

The reliability of the estimates of abundance provided for the management units where the ratio estimator has been applied is low because of the many tenuous assumptions made in the ratio estimator. One alternative approach would be to simply use the line transect estimates of abundance for sections of the coast that had line transect survey coverage. If abundance estimates were desired for sections of coast where no line transect coverage was present, then adjacent line transect density estimates could be applied to the unsampled areas. This abundance estimator would require two assumptions: that the density was the same between the two adjacent areas at the time of the survey and that the sighting probability used was unbiased. Thus, fewer and possibly less debatable assumptions would be required for this alternative estimation approach than the ratio estimator approach taken in the current paper and though biased, this latter estimate could arguably be more reliable than the one provided with the ratio estimator.

Paper #4: Abundance estimates for Atlantic Bottlenose Dolphin stocks during summer and winter, 1995. Lance Garrison and Cynthia Yeung.

Are the *data used appropriately*?

The line transect data were used in a standard manner (Buckland et. al 1993, Distance sampling. Chapman and Hall) to compute abundance estimates in the summer and winter of 1995 for the areas that were surveyed and are appropriate for abundance estimation.

Are the *analyses* conducted *appropriate*?

Conventional methods (Buckland et al. 1993) and software (DISTANCE software) for abundance estimation using line transect data were applied and are appropriate. One quantity required for the calculations is the probability of sighting animals within a given distance from the transect line. Some evaluation was applied to evaluate this probability. This involved fitting a monotonically decreasing function for the probability of sighting to the observed numbers of sightings at increasing distances from the trackline. It was found that sightings occurred at a lower frequencies near to the trackline than distances of about 100m perpendicular to the trackline. Due to the software having only monotonically decreasing sighting functions, the observations at distances less than 100m were left-truncated. This resulted in the removal of 23 groups or 20.9% of the sightings for summer and 26 groups or 23% of sightings for summer. These removals are costly because it reduces the number of observations available for abundance estimation and can decrease the precision in abundance estimates. The removal of these data does not appear to be absolutely necessary since it is a limitation of the software applied and a consequence of the software having available in it only monotonically decreasing functions. Other functions (e.g. 2^{nd} or 3^{rd} order polynomials) could have been applied that were more flexible and allowed for a lower numbers of sightings close to the trackline. The right-truncation of the observations at >450m due to sparseness of observations beyond this point

Are the *data adequate* for the analyses conducted?

The line transect data were slightly sparse for abundance estimation in the areas covered by the line transect surveys. This is partly due to the fairly wide spacing between the transects in both winter and summer surveys and due to the gaps in spatial coverage in the winter survey due to poor weather conditions. The result was that the variance in the abundance estimates was quite high – most CVs in abundance estimates were over 40% and up to almost 80%. The winter 2002 line transect survey had much closer line transects and this gave more acceptable CVs in estimates of abundance.

Are *assumptions* inherent in the analyses *appropriately considered*?

A key assumption made was that the probability of sighting animals on the trackline was 100%. This was given due consideration at least in other documents reviewing this assumption and is believed to be a reasonable assumption because the sighting probability increases with group size and group sizes of sighted animals were on average quite large. There is a slight bias in this interpretation because the estimation of mean group size is also biased towards larger group sizes since smaller groups would have lower sighting probabilities and would be under represented in the estimates of observed mean group size. But even with this bias accounted for, the range group sizes was still large enough to lead to sighting probabilities on the trackline (or those 100m out from the trackline) being close to 1. It is also assumed that all coastal morphotypes occur exclusively inside the inshore area of each management unit. It is acknowledged that some coastal animals are known to wander outside of the boundaries applied and that this would be more likely to result in a negative bias than the positive bias that might result from offshore animals wandering into the provisional management units. However, no attempt was made to test the sensitivity of abundance estimates to variations in the 12 km and 27 km boundaries used for the summer and winter abundances. The lower frequency of sightings below the plane on the trackline than sightings at around 100m from the trackline was considered to result from difficulties in sighting animals directly beneath the aircraft. The gaps in survey coverage were also acknowledged as problematic for abundance estimation. It is also assumed that all animals within the 12km and 27 km boundaries are coastal animals. Until recently there has been no genetics sampling within the intermediate offshore area 6km -27 km from shore (Garrison, June 2001). Thus, abundance estimates provided using these boundaries and assuming that all animals in these boundaries are the coastal biotype could be strongly positively biased. A very recent fall-winter biopsy indicates that 3 of 20 animals sampled 8km from shore near Cape Lookout were of the offshore biotype (Dr. Aleta Hohn, pers. Commn. Fig. 3). This suggests that biomass estimates of coastal bottlenose dolphin could be substantially positively biased.

Are the *interpretations* of the data and analyses appropriate?

The baseline estimates of abundance appear to be appropriately justified. However, additional analyses could have been carried out to quantitatively demonstrate the degree of sensitivity of results to the various assumptions made. Sensitivity analyses could have been conducted for assumptions dealing with for example sighting probability as a function of

distance from the trackline, and the offshore boundaries for the management units in winter and summer.

Suggestions for alternative methods where better methods exist.

As mentioned above, the authors should investigate the use of more flexible density functions (e.g., ones that permit a dip in sighting probabilities for the area nearest to the trackline) for the probability of making a sighting at different distances from the trackline.

Do the Analyses represent the best analysis of the available information?

The analyses could be improved with the few alternative methods of analysis suggested above. The abundance estimates would be unlikely to change appreciably. However, the CV in the estimates could decrease to some extent and this would be important for the PBR calculations.

Paper #5: Summary of abundance estimates and PBR for coastal Tursiops for waters between New York and Florida during 1995-2000. D. Palka, Lance Garrison, A. Hohn and C. Yeung.

Are the *data used appropriately*?

This paper summarizes information on abundance estimates, PBR and by-catch estimates provided in other papers (Garrison and Hohn 2001 (ratio estimates of abundance for areas not sampled by line transect); Garrison and Yeung 2001(line transect estimates of abundance); Garrison July 2001 (Florida bycatch); Palka and Rossman 2001 (bycatch in North Carolina management units)). Thus, the comments applied to the other papers that provide inputs to this paper also apply to this paper and are therefore not reported here.

Are the *analyses* conducted *appropriate*?

The synthesis of by-catch and abundance estimates from other papers for the various management units is appropriate. However, as mentioned in other parts of this review, some improvements could be made to some of the contributing analyses to improve the rigor of the analyses and more robustly account for uncertainty in the estimates. This is particularly in regard to the paper that estimated the 12 km and 27 km offshore boundaries to the summer and winter management units, and the abundance estimates provided from ratio method estimation. The comparison of abundance estimates from the different surveys between 1995 and 2000 is also appropriate for evaluating the reliability of abundance estimates used in the PBR calculations. The estimates of bycatch were obtained from the papers Garrison (July 2001 (Florida bycatch)); and Palka and Rossman (2001 (bycatch in North Carolina management units)). Only the bycatch estimates obtained for these fisheries were used as estimates of bycatch mortality in the various management units. However, another paper provided as background information, titled "Bottlenose

dolphin (Tursiops truncates): western north Atlantic coastal stock" indicates that there are bycatch mortalities in several other coastal fisheries. For example in the beach haul seine fishery two dolphins were taken in 166 observed hauls. Interactions with crab pots are listed as a possible source of mortality in 22 stranded bottlenose dolphin carcasses. Bycatch mortality has also been observed to occur in other fisheries such as Virginia pound nets, shrimp trawls, and Menhaden purse seine. Yet in Palka et al. (2001) bycatch mortalities from these fisheries are excluded from the total bycatch mortality estimates provided for various management units. The comparisons of the gillnet bycatch estimates with values for PBR are premature because these other sources of bycatch mortality are excluded. The gillnet bycatch mortality estimates used could only be interpreted as negatively biased estimates of total bycatch since these other sources of bycatch were not included in the bycatch figures applied. The PBR calculations themselves are standard and use reasonable input assumptions, e.g., for r_{max} and the recovery factor F_r, (e.g., based on work by Wade 1998, Calculating limits on the allowable human-caused mortality of cetaceans and pinnepeds, Marine Mammal Science 14: 1-37.).

Are the *data adequate* for the analyses conducted?

The combined set of data are only marginally adequate for abundance estimation and the determination of PBRs in the management units. As mentioned above, the bycatch estimates applied in the comparison with PBR estimates will be negatively biased because they include only estimates from southern shark gillnet and mid-Atlantic gillnet fisheries, when it is known that bycatch occurs in other fisheries.

Are *assumptions* inherent in the analyses *appropriately considered*?

Overall, assumptions in the estimation of abundance and by-catch have been appropriately considered. For example additional attention is given to the probability of detecting animals on the trackline of line transect surveys (g(0)). Also the various reasons for why some abundance estimates varied between comparable surveys were also considered (e.g., differences in mean track length and spatial coverage, group size estimates and altitude flown). With reference to the distribution of group sizes of animals and how the probability of detection approaches 1 with large group size, the authors maintain that the negative bias resulting should be quite small. The abundance estimates for the North Carolina mixed and South Carolina management units were found to be statistically not different from the sum of the geographically analogous summer abundance estimates. This consistency gives some confidence to the abundance estimates obtained. However, additional sensitivity tests should be carried out to evaluate the sensitivity of key abundance estimates to the key assumptions, particularly the boundaries of 12km in winter and 27 km in summer. It is assumed that bycatch in fisheries other than the southern shark gillnet and mid-Atlantic gillnet fisheries is zero, despite observations indicating fishery by-catch mortalities in several other fisheries including beach haul seine, crab pot, Virginia pound net, shrimp trawl, and menhaden purse seine fisheries. In a draft paper January 2002 on bottlenose dolphin it is indicated that the main bycatches occur in the southern shark gillnet and mid-Atlantic gillnet fisheries, so the majority of bycatch should be taken into account by using

estimates from only these two fisheries. However, further work is needed to provide estimates of annual bycatch rates in the other fisheries where bycatch mortalities are known to occur.

Are the *interpretations* of the data and analyses appropriate?

The classification of management units as "strategic", where the PBR and by-catch could be compared, could not be disputed except for Florida. On p. 6. it is stated that "In all management units where there was information on bycatch, the bycatch estimate exceeded the PBR value. In figure 2, this was so for the North Carolina and Virginia management units. However for Florida the value for PBR was 74 and the value for bycatch was 54, i.e., the estimated bycatch was less than the estimated PBR. However, the estimates are highly imprecise and given the large CVs (e.g., 0.63 for bycatch), it could not be concluded that the estimated PBR was significantly greater than the bycatch. Thus a more accurate conclusion could be that in all management units the bycatch was not significantly larger than the PBR. The finding that the bycatch values are not significantly less than the PBR estimates where they are available could be altered if new surveys, and improved abundance estimators were applied. The abundance estimates could increase if the offshore boundaries of the management units were extended with new data and new methods of analysis, and the abundance estimates became more precise from improved survey design and the use of improved estimators of abundance. The by-catch estimates could change with updates in the analysis with new bycatch observations. Observer coverage has been quite low and unbalanced in time and space; the estimates obtained are imprecise and could be biased if the observer samples were non-representative. Improved observer coverage and additional observations could conceivably provide by-catch estimates considerably different from the current ones. Also, when the additional sources of bycatch mortality are factored in, the bycatch estimates will increase.

Suggestions for alternative methods where better methods exist.

The protocols used to account for uncertainty in abundance estimates could be improved upon and this could possibly increase the widths of the confidence intervals in estimates of abundance. However, the February 2002 line transect estimates of abundance and improved abundance estimation methodology used in it (Garrison and Hoggard 2002, A preliminary abundance estimate for the North Carolina winter management unit of *Tursiops truncatus* from a winter 2002 mid-Atlantic aerial line transect survey) should address the most fundamental shortcomings pointed out in this review.

Do the analyses represent the best analysis of the available information?

Improvements could still be made to the methods used to estimate the summer and winter offshore extents of coastal bottlenose dolphin particularly in regards to the replacement of the non-parametric estimator of the abundance break with a parametric estimator; such an estimator should provide far more reliable results with use of the February 2002 line transect survey data.

Paper #6: Mortality estimate for Atlantic bottlenose dolphin in the directed shark gillnet fishery of Florida and Georgia. Lance Garrison

Are the *data used appropriately*?

The observer data on shark fishery captures of bottlenose dolphins in Florida and Georgia in 1999, 2000 and 2001 and fishing effort for this fishery from the NMFS fishing vessel logbook data base were used to estimate bottlenose dolphin mortality in this fishery.

Are the *analyses* conducted *appropriate*?

The use of a delta estimator (Pennington 1993) appears to be appropriate because as it is stated "catch rates are generally log-normally distributed and by-catch events are rare" and the method has been applied in other similar situations.

Are the *data adequate* for the analyses conducted?

The data on observed kills of bottlenose dolphins on the observed vessels is very sparse with only 9 animals being killed since 1999. The sparseness of observations would make it difficult to make precise estimates of mortality when expanded out by the amount of effort in each season and geographic area. The CVs in the estimates of mortality are thus very high, ranging from 0.49 to 1.

Are *assumptions* inherent in the analyses *appropriately considered*?

The poor observer coverage in some combinations of geographic areas and seasons is taken into account in the definition of geographic strata for abundance estimation. The aggregation of geographic areas and provision of a single annual estimate rather than seasonal estimates is perfectly sensible given the sparseness of observations. The finding that the number of observed trips when there was about 100% coverage of fishing trips was approximately twice the number of trips in the logbook data base in February 2000 and 2001 is cause for concern and indicates that the amount of reported effort in logbooks might be only half of the real amount of effort. It is stated that it is unknown whether this reflects the reporting rate at other times of year. It is also stated that by-catch estimates could be 50% too small if this was the average reporting rate for the whole year. A positive source of bias is also noted, the use of strike sets as opposed to drift sets. However, it is reported that the proportion of strike sets relative to drift sets in the observer database is low. This remains an uncertain source of bias because these sets cannot be distinguished in the logbook database. Another requirement of the analysis is that where there was not 100% observer coverage, the method of sampling vessels for observer coverage gives a representative sample of the total population of potential vessel trips. Nearly 100% coverage occurs in February due to 100% coverage being mandated during the right whale season January – March for the shark gillnet fishery. However, due to the large expense of the coverage, funding soon runs out and coverage is much less for the rest of the year when

the fishery moves north. As it is pointed out in the paper, bycatch rates are really only representative of the covered period, and there is some question about extrapolating these rates across the annual effort.

Are the *interpretations* of the data and analyses appropriate?

The abundance estimates obtained are highly imprecise but otherwise obtained in a reasonable manner.

Suggestions for alternative methods where better methods exist.

None.

Do the Analyses represent the best analysis of the available information?

Due to the sparseness of data, and the reasonable rigour in the analysis, further analysis would not be worthwhile.

Paper #7: Bycatch estimates of coastal bottlenose dolphin (*Tursiops truncatus*) in US mid-Atlantic gillnet fisheries for 1996-2000. Debi Palka and Marjorie Rossman

Are the *data used appropriately*?

The observer data on bycatch observations, and NMFS vessel logbook data on fishing effort were used appropriately to estimate bycatch of bottlenose dolphin in mid-Atlantic gillnet fisheries. Of the observed takes in gillnets one animal was observed to escape and was eliminated from the analysis. However, there were only about a dozen observations (in some places it says 12 (abstract) in others it says 13 (p. 3) so it is difficult to know the actual number of observed takes). It could be argued that another analysis could be performed with the animal that survived included along with the others that died in the analysis. The model developed would then be for the number of takes and bycatch deaths could be modeled as the product of predicted takes and the chance of death (n-1)/n where n is the number of takes with n-1 being the number of deaths. This latter GLM approach might be justified due to the sparseness of the number of observed takes. It could also be applied as a test of the validity of the conclusions with the GLM applied only to the animals that died. The best model chosen with the animals that died should not change with the addition of one take observation. If it did, this would indicate that the number of takes observed is still too small for the highly sophisticated modeling approach chosen.

Are the *analyses* conducted *appropriate*?

The GLM modeling approach used appears to be appropriate for the modeling and estimation of bycatch. The reasons for choice of this approach over others is not given but

a reference which documents the choice is cited (Rossman and Palka 2000). A very large number of potential explanatory variables and factors are evaluated to statistically derive a model of takes that can be applied to the NMFS logbook data base on fishing effort to statistically estimate the total bycatch. A few other covariates that could also potentially explain the number of takes that were not evaluated were the estimated density of animals in the management unit where the operation takes place and an index of the density of key prey items in the location of the fishing operation. It was mentioned that cyanid fishes are one of the main prey items of coastal bottlenose dolphins and that cyanid fishes are also targeted in some fishing operations. An alternative model that could be tried could attempt to evaluate whether bottlenose dolphin density in the locality of the fishing operation was also a significant covariate. However, it would be doubtful that an unbiased index of cyanid density at the locality of the fishing operation could be constructed for use in the GLM modeling since chance of capture of cyanides in gillnets would depend on the mesh sizes used. The bootstrap procedures used to compute confidence intervals in bycatch estimates were appropriate for this purpose.

Are the *data adequate* for the analyses conducted?

There were only 12 (or 13, p. 3) bottlenose dolphin takes over 7000 observed hauls. This resulted partly from the relatively low percentage coverage of fishing operations with between 1.7% and 2% of recorded operations being observed in state operations where takes were highest. This is a highly limited number of positive observations and it could be expected that only very simple models of mortality could be found to suitably explain the occurrence of mortality in fishery operations. Moreover, it could also be expected that if a high enough number of alternative models of potential covariates is searched over, just by chance alone, a good fit to the data could be expected to be found. Therefore the extremely intensive modeling effort for this very small number of positive observations may be overdoing it and could potentially lead to spurious relationships being found. Moreover, although it is normally considered in GLM modeling, interaction effects among the covariates and factors of interest were ignored in this analysis, though they could potentially be present between the factors considered. It is conceivable that there could very well have been depth by mesh size interaction effects. The number of positive observations was so low that detection of such interaction effects would be impossible. Indeed the full model option tested in table 4 had 11 covariates and factors. This would appear to be an excessive number of different covariates and factors to explain the incidence of takes when there were only (lets say) 11 mortalities in total. Landings were used as an index of fishing effort. This appears to be fine if the relative abundance of target species does not vary much over the period of interest. Some supporting evidence should be provided to verify this assumption.

Are *assumptions* inherent in the analyses *appropriately considered*?

The assumption that there are sufficient numbers of observed takes in the variety of fishing conditions of interest to be able to statistically select a model of takes does not appear to have been considered. Also, the assumptions inherent in the use of landings as a proxy for

effort appeared to be glossed over in the paper. Further work is required to substantiate the validity of this proxy for fishing effort. Another assumption is that the behaviour of fishermen is the same on observed and non-observed trips. For example, fishermen on non-observed trips might be more likely to use fishing methods that have a higher chance of catching and leading to the death of bottlenose dolphins. The protocol determining which vessels observers were placed on was not mentioned. If this was a non-random choice, this could also result in bycatch rates from observed vessels not being representative of the entire population of fishing operations. This assumption however was not mentioned. Outside of the paper, NMFS scientists indicated the following sampling protocol:

> The observer program is mandatory. Observers are assigned to a port location on the basis of current fishing effort out of a port and go to the docks in the morning to find a boat going fishing that day. In some cases … they make contacts the evening before. In theory, sampling is random within a port. At the time a vessel leaves the dock, the observer cannot be certain whether fishing will occur very near shore or way off shore or even in the estuaries, although the fishermen may begin the trip with a target species and relative fishing location in mind.

Thus there is an attempt at making the sampling of fishing trips random and this should help to ensure that the extrapolation of bycatch rates from the observed trips to the unobserved trips is valid.

Are the *interpretations* of the data and analyses appropriate?

It was found that mesh size and distance offshore were the best explanatory variables for takes of bottlenose dolphin in gillnet fisheries. For example, bycatch rate was highest with the largest mesh size category and within state waters. These appear to be intuitively plausible explanatory results. However, considerably more data on takes would need to be obtained to identify potentially more refined models that used more of the potential explanatory variables available. The precision in the annual estimates of bycatch of bottlenose dolphins appears to be moderate with the CV in estimates between 33 and 37% per year. Some extrapolation of landings to distance from shore categories was required because most of the states did not disaggregate landings data by federal versus states waters. The method used for this which used vessel trip report and observer data appears to be appropriate. The analyses indicating the probability of observing a take if the true takes were at various levels was useful in demonstrating the importance of having higher levels of observer coverage, especially for state waters. Relatively high takes could still be accompanied by 0 observations of takes with the levels of observer coverage in state waters prior to 2000. For example with 0.08% coverage and 100 actual takes there would be only a 56% chance of observing a single take. While it is mentioned that the residuals were reasonably well behaved, this does not appear to be so in some of the figures. For example in Figure 5, there are many positive residuals at low take numbers. This indicates that considerable amounts of variation are not well explained and that additional explanatory variables might be needed to more accurately model the bycatch rates.

Suggestions for alternative methods where better methods exist.

None. However, a check on the reliability of the results could be obtained if a season and area based estimator was developed as in the previous paper and applied to these data. It would be useful to evaluate the sensitivity of bycatch estimates to alternative model forms since with so few observations of captures, the uncertainty in the structural formulation of a bycatch model should still be quite high.

Do the analyses represent the best analysis of the available information?

The models chosen for bycatch rates in gillnet fisheries appear to be intuitively plausible and were supported by the AIC criterion. It seems unlikely that the estimates provided would vary considerably with further analyses of the same data. However, increased observer coverage to allow for a higher number of observations of takes could help to improve the refinement of the model developed for bycatch estimation.

Paper #8: A preliminary abundance estimate for the North Carolina Winter Management Unit of *Tursiops truncates* from a winter 2002 mid-Atlantic aerial line transect survey. June 2001. Staff, SE FSC

Together with the other two reviewers, I received a draft of this paper on February 25, 2002. This is a draft in progress and some of the analyses (e.g., stratification) have been updated in subsequent drafts. Therefore, the comments I provide below pertain only to the draft that I received and could be dated due to more recent updates in this analysis. However, some of the general concerns still apply, for example, on changes in the way strata are defined as surveys are updated, and the need for transparent, rigorous criteria for the revision of sample strata for abundance estimation.

Are the *data used appropriately*?

The line transect data were used appropriately to obtain abundance estimates of *Tursiops truncates* in two different strata for the North Carolina Winter Management Unit. The strata were within the 0-20 m isobaths and within the 20-40m isobaths.

Are the *analyses* conducted *appropriate*?

The analyses conducted were appropriate to estimate abundance in the two different strata. Standard line transect methodology (Buckland et. al 1993, Distance sampling. Chapman and Hall) and software (DISTANCE) were used appropriately. A published method was applied to the dual sets of observer estimates obtained to estimate g(0), the sighting probability on the trackline. Due to the early state of the analyses, this estimate is preliminary and will be updated with further analysis of the February 2002 line transect survey data.

Are the *data adequate* for the analyses conducted?

The sampling design for the February 2002 aerial line transect survey was well-designed and rigorously carried out. The intent was to produce abundance estimates with CVs less

than 26% and this was achieved with the resulting CV in the 0-20m strata being 24.9%. This good achievement in precision was achieved because spatial coverage within each strata was very good with tracklines only 2-5km apart. Additionally, two sets of observers were aboard the aircraft with one group in the forward bubble and a second group in the belly of the aircraft. This increased the chance of sighting groups of animals and thereby increased the precision in the estimate because more groups were sighted by having two groups of observers. This also permitted the estimation of the sighting probability on the trackline, g(0), a key parameter in line aerial line transect abundance estimation.

Are *assumptions* inherent in the analyses *appropriately considered*?

A key assumption about g(0) in the abundance estimation from the 1995 aerial line transect survey (Garrison and Yeung 2001) was empirically evaluated and updated in this paper. Two sets of observers on the same aircraft and Palka's method (1995, Rep. Int. Whale. Commn Spec. Issue, 16:27-50) were applied to obtain a preliminary estimate of g(0).

Are the *interpretations* of the data and analyses appropriate?

It was stated in the conclusions that "the estimate of g(0) for this survey is not different from 1". However, it might be too early to conclude this because as the authors mention, the estimation is still preliminary. Furthermore, while it is reported that Palka's (1995) method was applied to estimate g(0), and the abundance estimate obtained was slightly less than the uncorrected estimate, the actual value for g(0) was not specified in the paper. The coefficient of variation in the estimate of g(0) was not also provided in the paper, although it was stated that the estimate was not significantly different from 1. However, the Palka (1995) – corrected estimate was only slightly less than the uncorrected estimate so this conclusion about g(0) being not significantly less than one is plausible at this preliminary stage in the analysis.

The authors obtained abundance estimates using a stratification scheme different from the previous abundance estimation of coastal bottlenose dolphin. Instead of using the winter habitat area of 0-27km offshore (Garrison June 2001, Garrison and Yeung 2001), Garrison and Hoggard (2002) used instead the area within 0-20m depth. The specific reason for not providing an abundance estimate for the previously established winter habitat area for coastal bottlenose dolphin was not provided. This makes comparisons with the previous estimates based on the 1995 survey difficult. The estimate from the Feb. 2002 survey for the coastal stratum in the winter mixed North Carolina management unit was 21,293 (CV=24.9). In contrast, the estimate obtained from the 1995 winter survey was 4,734 animals (CV=49.1) (Garrison and Yeung 2001). This latter value is not comparable with the Feb. 2002 value for at least two reasons. The first is that the 4,734 estimate appears to have been computed for the area only from Murrell's Inlet SC to Cape Hatteras and not the entire winter mixed management unit for North Carolina (though this is not apparent of Table 1 of Garrison and Yeung 2001 since they state the area from Cape Lookout North Carolina – North Carolina/Virginia was 3309 km^2, yet this latter area appears instead to be the substratum area only between Cape Lookout and Cape Hatteras). Garrison and Hohn

(2001) used a ratio estimator to estimate the abundance of 1740 in the remaining area between Cape Hatteras and the North Carolina Virginia border to give a total of 6474 which was reported as the total abundance estimate for the winter mixed stock northern migratory, northern North Carolina and Southern North Carolina management unit in Palka et al. (2001). The 1740 estimate appears to have been obtained, as mentioned above by assuming a winter habitat zone for coastal bottlenose dolphin of 0-27km offshore. Using the 20 m isobath, Garrison and Hoggard (2002) state that this follows approximately the 12km offshore boundary previously identified by Garrison (June 2001) for waters to the north of Cape Hatteras and the 27km boundary south of Cape Hatteras. Thus the February 2002 estimate is also not comparable with the estimate of 6,474 for the same management unit in the previous year because the February 2002 estimate used a much smaller sub-stratum area for the substratum north of Cape Hatteras, approximately 12km offshore rather than the 27km used for the 2001 estimate. It would be useful therefore if an abundance estimate for the winter North Carolina management unit was obtained using the exact same strata as was used in the Garrison and Hohn (2001) and Garrison and Yeung (2001) papers. It would also be illustrative if the estimates provided for the winter NC management unit could also be broken down by the sub-stratum north of Cape Hatteras and that south of Cape Hatteras with the substratum areas provided also. In a subsequent analysis, not included in this paper, the 27 km offshore boundary was applied to points south of Cape Hatteras and a 12 km boundary was applied to points north of Cape Hatteras. In contrast, the Garrison and Hohn (2001) paper used the 27 km boundary north of Cape Hatteras for the estimation of abundance in winter. A clear justification needs to be provided for this update in the offshore winter boundary North of Cape Hatteras.

Suggestions for alternative methods where better methods exist.

None. However, Garrison (June 2001) attempted to identify summer and winter offshore boundaries for coastal bottlenose dolphin using the 1995 line transect data. The analysis was ad hoc and the interpretation of the empirical results was not easy because of the sparseness of the data and the non-parametric numerical approach taken which together generated very jagged (imprecise) output. It would appear that the results of the winter February 2002 survey could provide much more precise empirical information about offshore breaks in the abundance of animals. A similar or updated estimation approach to that provided in Garrison (June 2001) could be applied to provide a more rigorous basis for coastal stratum boundary identification.

Do the Analyses represent the best analysis of the available information?

As mentioned by the authors in this paper, the abundance estimates are still preliminary because this is the initial analysis of new survey data. The survey design and implementation for this line transect survey appear to be excellent and provide an excellent set of data for abundance estimation. The general methodology applied appears to be appropriate. However, the justification for redefining the offshore breaks for stratum definition for abundance estimation needs more rigorous justification. It does not appear to be acceptable to change the stratum used for abundance estimation every time a new survey

is carried out. Objective, transparent and rigorous criteria should be developed to outline when it might be appropriate to modify the boundaries of strata used for abundance estimation.

Conclusion: Do the analyses represent the best analysis of available information?

Overall, the analyses represent the best analysis of available information. In most instances, available data are sparse but come from a wide variety of sources. The limitations in the data as well as the main features in the data appear to be appropriately taken into account in the identification of provisional management units. The method used to define the offshore boundary for the coastal strata was ad hoc and could be updated and the new method applied to the February 2002 line transect data to re-evaluate the validity of the 27km winter offshore boundary. An improved summer line-transect survey is also needed to re-evaluate the validity of the summer 12km offshore boundary. The estimation of abundance of animals in the provisional coastal management units using line transect data was done using standard abundance estimation methods and provide credible estimates of abundance, particularly from the most recent winter 2002 line transect survey. Estimates of bycatch in southern shark gillnet and coastal mid-Atlantic fisheries were used as the total bycatch in some of the management units and compared with estimates of PBR. However, there are other sources of bycatch mortality in other fisheries including the beach haul seine, crab pot, Virgina pound net and other fisheries, that were not included in the total bycatch estimates applied. However, at the time, it does not appear that rigorous estimates of bycatch in the other fisheries exist and it appears that the largest number of bycatches occur in the gillnet fisheries. Thus the bycatch estimates from the gillnet fisheries provide a reasonable minimum estimate of bycatch. However, efforts are still needed to provide average annual estimates of bycatch in the other fisheries. Furthermore, more genetics sampling in winter and summer of animals found in waters within 27 km, particularly in the poorly sampled band between 6km to 27 km offshore, is needed to estimate the proportion of animals in the near shore regions that are of the offshore biotype. Until this is done, and given the recent finding that 3 of 20 animals biopsied at 8km offshore hear Cape Lookout were of the offshore biotype, it is likely that the estimates of abundance of coastal animals in the provisional management units will be positively biased.

Appendix A: email correspondence from National Marine Fisheries Service Staff on the Review of Science in Support for a Take Reduction plan for *Tursiops truncatus*

This appendix contains e-mail correspondence between NMFS scientists Dr Aleta Hohn, Dr. Lance Garrison and Dr. Robin Baird and the reviewer Dr. Murdoch McAllister. The correspondence was to provide additional information on the science behind the current take reduction plan and to clarifies issues regarding some of the sampling designs and methodologies applied.

Correspondence with Dr Aleta Hohn

Date: Mon, 25 Feb 2002 12:02:55 -0500
From: "Aleta Hohn" <Aleta.Hohn@noaa.gov>
Organization: NOAA Beaufort Lab
X-Mailer: Mozilla 4.75 [en] (WinNT; U)
X-Accept-Language: en
To: A.R.Hoelzel@durham.ac.uk, mcsk@natur.gl, m.mcallister@ic.ac.uk
CC: Lance Garrison <Lance.Garrison@noaa.gov>, mcsk_gl@yahoo.com
Subject: new abundance document

Attached is Lance's recent paper on winter abundance estimates of Tursiops. We hope these new results (still in draft) will replace the hodge-podge estimates for winter that we had to rely on in the documents you have. The summer estimates will be updated this summer, also by surveying the entire coast rather than just north of Cape Hatters.

Aleta

 MATS2002_Prelim_Abundance_f.doc

 aleta.hohn.vcf

From: "Aleta Hohn" <Aleta.Hohn@noaa.gov>
To: robin.baird@noaa.gov,jennifer.lawrence@noaa.gov
Cc: m.mcallister@ic.ac.uk
Date: Wed, 27 Feb 2002 07:19:08 -0500
X-Mailer: Netscape Webmail
Subject: Fwd: Re: new abundance document
X-Accept-Language: en

Robin and Jenn -

Would you mind faxing a copy of Paul Wade's PBR paper from Marine Mammal Science in 1999 or so to Murdoch. Or if you have a PDF version, Rogin, that would be even better. Sorry I didn't see this before I left

yesterday. Robin - can you think of any other key PBR papers that might be helpful to Murdoch?

Thanks.

Aleta

Return-Path: <m.mcallister@ic.ac.uk>
Received: from relay-east.nems.noaa.gov ([205.156.4.216]) by
nsmail.ccfhrb.noaa.gov (Netscape Messaging Server 4.15) with
ESMTP id GS5ID000.DXJ for <Aleta.Hohn@nsmail.ccfhrb.noaa.gov>;
Tue, 26 Feb 2002 12:39:00 -0500
Received: from nems.noaa.gov (scan-east.nems.noaa.gov
[205.156.4.217]) by relay-east.nems.noaa.gov (Netscape Messaging
Server 4.15) with ESMTP id GS5I1E00.F1P; Tue, 26 Feb 2002 12:32:02 -0500
Received: by nems.noaa.gov; id MAA13073; Tue, 26 Feb 2002 12:32:01 -0500 (EST)
Received: from mr4.cc.ic.ac.uk(155.198.5.114) by gummo.nems.noaa.gov via csmap (V4.1)
id srcAAASFaaHz; Tue, 26 Feb 02 12:32:01 -0500
Received: from icex9.cc.ic.ac.uk ([155.198.3.9])
by mr4.cc.ic.ac.uk with esmtp (Exim 3.31 #1)
id 16flOw-0003Yd-04; Tue, 26 Feb 2002 17:28:30 +0000
Received: from kant.ic.ac.uk (kant.env.ic.ac.uk [155.198.98.108]) by icex9.cc.ic.ac.uk with SMTP (Microsoft Exchange Internet Mail Service Version 5.5.2653.13)
id D448NGG8; Tue, 26 Feb 2002 17:28:24 -0000
Message-Id: <5.0.0.25.1.20020226171758.03088560@icex9.cc.ic.ac.uk>
X-Sender: ic\mmmcal/m.mcallister@icex9.cc.ic.ac.uk
X-Mailer: QUALCOMM Windows Eudora Version 5.0
Date: Tue, 26 Feb 2002 17:27:24 +0000
To: "Aleta Hohn" <Aleta.Hohn@noaa.gov>, A.R.Hoelzel@durham.ac.uk,
mcsk@natur.gl
From: Murdoch McAllister <m.mcallister@ic.ac.uk>
Subject: Re: new abundance document
Cc: Lance Garrison <Lance.Garrison@noaa.gov>, mcsk_gl@yahoo.com,
manoj@rsmas.miami.edu
In-Reply-To: <3C7A6E3F.99D535F4@noaa.gov>
Mime-Version: 1.0
Content-Type: text/plain; charset="us-ascii"; format=flowed

Dear Aleta,

I'd be grateful if references to some key papers on the concept of potential biological removal could be e-mailed to me. Due to the deadline date for the review being Monday 4 March, I'd also be grateful if a section of a key paper dealing with the term Fr (PBR= Nmin *1/2 * Rmax * Fr) and how it should be interpreted and determined for a given stock be FAXed to me at +44-207-589-5319. I will be requesting a few other clarifications in the next day or two.

Best wishes,
Murdoch McAllister

At 12:02 PM 2/25/02 -0500, Aleta Hohn wrote:
Attached is Lance's recent paper on winter abundance estimates of Tursiops. We hope these new results (still in draft) will replace the hodge-podge estimates for winter that we had to rely on in the documents you have. The summer estimates will be updated this summer, also by

surveying the entire coast rather than just north of Cape Hatters.

Aleta

Date: Mon, 04 Mar 2002 10:34:08 -0500
From: "Aleta Hohn" <Aleta.Hohn@noaa.gov>
Organization: NOAA Beaufort Lab
X-Mailer: Mozilla 4.75 [en] (WinNT; U)
X-Accept-Language: en
To: m.mcallister@ic.ac.uk, A.R.Hoelzel@durham.ac.uk, mcsk@natur.gl
CC: manoj@rsmas.miami.edu
Subject: Index of /~ahohn/CIE

Murdoch, Michael, and Rus --

I have taken some slides from a Powerpoint presentation of the stock id project that will allow you to see the isotope ratio results in color. I've also included some of the satellite tracking results. Because the file is 3.5MB, I have put it on my web site for you to access.

http://shrimp.ccfhrb.noaa.gov/~ahohn/CIE/

The PPT file is the only one in this subdirectory and is called "for cie.ppt".

We have not yet analyzed the isotope data quantitatively although I do think we will need to do so eventually. The initial stages of this project were conducted by a grad student at the Univ. of Virginia, where we are collaborating with Dr. Steve Macko, a well known isotope specialist. Unfortunately, the grad student decided to bail out and did not finish all the samples we hoped to run. Additional funding was not available for a couple years so new results were generated. We now have another grad student working on the project and actively running samples. To be honest, I haven't thought through the tests that we should and I imagine it will depend in part on the final temporal and spatial distribution of samples and samples sizes within each stratum. Some interesting comparisons will be available. For example, each satellite tagged animal will also have an isotope and genetic result so we can compare the results across various methods. I am certainly open to any suggestions you may have. The initial results are interesting, however, and visually fairly compelling with regard to continuing work with this method.

I have returned from the Take Reduction Team meeting and will reply to further questions as soon as I receive them.

Aleta

Index of /~ahohn/CIE

Name	Last modified	Size	Description
Parent Directory	04-Mar-2002 10:14	-	
For CIE.ppt	04-Mar-2002 10:21	3.5M	

Apache/1.3.20 Server at _default_ Port 80

 aleta.hohn2.vcf

Date: Mon, 04 Mar 2002 10:43:04 -0500
From: "Aleta Hohn" <Aleta.Hohn@noaa.gov>
Organization: NOAA Beaufort Lab
X-Mailer: Mozilla 4.75 [en] (WinNT; U)
X-Accept-Language: en
To: Murdoch McAllister <m.mcallister@ic.ac.uk>
CC: A.R.Hoelzel@durham.ac.uk, mcsk@natur.gl,
Lance Garrison <Lance.Garrison@noaa.gov>,
mshivlani@rsmas.miami.edu,
Marjorie Rossman <Marjorie.Rossman@noaa.gov>,
Mike Tork <Mike.Tork@noaa.gov>
Subject: Re: new abundance document

I will try to answer for the observer program but have cc'd Marjorie Rossman and Mike Tork who work with the observer program and observer data above so they can correct me if I don't get it right.

The observer program is mandatory. Observers are assigned to a port location on the basis of current fishing effort out of a port and go to the docks in the morning to find a boat going fishing that day. In some cases I believe they make contacts the evening before. In theory, sampling is random within a port. At the time a vessel leaves the dock, the observer cannot be certain whether fishing will occur very near shore or way off shore or even in the estuaries, although the fishermen may begin the trip with a target species and relative fishing location in mind.

Aleta

Murdoch McAllister wrote:

> Hello,
>
> I have a question for NMFS scientists regarding the observer data used in
> the estimation of bycatch rates:
>
> What were the protocols applied for determining which fishing vessels
> observers were placed on in the various shark and mid-Atlantic gillnet
> fisheries, when observer coverage was not 100%?

>
> Regards,
> Murdoch McAllister
>

Date: Mon, 04 Mar 2002 13:36:00 -0500
From: "Aleta Hohn" <Aleta.Hohn@noaa.gov>
Organization: NOAA Beaufort Lab
X-Mailer: Mozilla 4.75 [en] (WinNT; U)
X-Accept-Language: en
To: Murdoch McAllister <m.mcallister@ic.ac.uk>
CC: A.R.Hoelzel@durham.ac.uk, mcsk@natur.gl, manoj@rsmas.miami.edu
Subject: Re: Index of /~ahohn/CIE

Murdoch -

The protocol was to first run the samples from a fishery interaction or live capture. Unfortunately, the student did not follow that protocol. Samples were chosen randomly rather than selectively from the universe of samples that were collected. The only exception were samples from South Carolina (all from animals caught to tag then released) and samples from Virginia, most of which were from fishery interactions. The new samples being run are focusing on live capture and entanglements.

No trouble.

Aleta

Murdoch McAllister wrote:

Dear Aleta,

Sorry to trouble you again. I have a few additional questions about the method of collection of samples for isotope analysis; these might have already been addressed somewhere in the documentation provided but I have not been able to find where.

(1) How were animals selected for isotope sampling?
(2) Were there protocols applied to try to ensure that the animal sampled (e.g., if it was a stranded animal) did not stray from other areas (as in the genetic sampling)?

Regards,
Murdoch

Date: Wed, 06 Mar 2002 10:50:38 -0500
From: "Aleta Hohn" <Aleta.Hohn@noaa.gov>
Organization: NOAA Beaufort Lab
X-Mailer: Mozilla 4.75 [en] (WinNT; U)
X-Accept-Language: en
To: Murdoch McAllister <m.mcallister@ic.ac.uk>
CC: Lance Garrison <Lance.Garrison@noaa.gov>,
mshivlani@rsmas.miami.edu
Subject: Re: new abundance document

Gillnet fisheries have been banned, with few exceptions, in state waters (< 3 mi) of South Carolina, Georgia, and Florida so there is no federal gillnet observer program. There has been localized state or NMFS observer effort (as research, not through the Woods Hole lab) for a number of the highly regulated small-scale gillnet fisheries still allowed in SC and Georgia that has shown no indication of interactions with Tursiops. Most of these fisheries use small nets with small mesh, are highly limited seasonally and spatially and include few fishermen.

The shark fishery in Florida occurs outside 3 miles and is observed primarily due to right whale entanglements. These observations confirmed that bottlenose dolphins were being taken. That fishery ranged into federal waters off Georgia until recently. I believe it now operates only off the coast of Florida (still only in federal waters).

The federal observer program that collected the data in the various Palka documents operates from North Carolina through Maine. The coastal morphotype of bottlenose dolphin doesn't occur further north than NJ for the most part (occasionally a group will be seen off NY). Takes have occurred only in coastal waters of New Jersey, Virginia, and NC, with the largest problem being in NC and Virginia.

On a slightly different matter but pertaining to abundance, on Tuesday morning of last week I received some genetic results from biopsy sampling we've been conducting locally all winter. The abundance estimate from the surveys we just completed (the Garrison and Hoggard ms. sent last week) presumed all that all Tursiops within 27km of the beach south of Cape Hatteras were of the coastal morphotype. We had a group of about 20 dolphins from which we got 3 samples that turned out to be of the offshore morphotype 8 km from shore. We had very little biopsy effort that far offshore because mostly we are working out of small boats. That means that the abundance estimate generated from the recent surveys is not just of the coastal morphotype and could potentially be highly positively biased. We have no way to interpret those results right now. I have attached a PPT image showing the biopsy samples we have collected locally over the past few months.

Please don't hesitate to ask additional questions.

Aleta

Murdoch McAllister wrote:

> Hello,
>
> I have yet another question. Hopefully it will be my last for now.
>
> Estimates of bycatch in the paper by Palka, Garrison, Hohn and Yeung 2001
> use estimates of bycatch in gillnet fisheries in Florida and Georgia and US
> mid-Atlantic gillnet fisheries. Estimates of bycatch in other Atlantic
> coast fisheries were not provided in the documents that were to be
> reviewed. It appears in the Palka et al. (2001) paper that it is assumed
> that the bycatch mortalities in fisheries other than these two sets of
> gillnet fisheries are zero. Why was this assumption made?
>

> Murdoch

Date: Wed, 06 Mar 2002 11:54:41 -0500
From: "Aleta Hohn" <Aleta.Hohn@noaa.gov>
Organization: NOAA Beaufort Lab
X-Mailer: Mozilla 4.75 [en] (WinNT; U)
X-Accept-Language: en
To: Murdoch McAllister <m.mcallister@ic.ac.uk>
CC: Lance Garrison <Lance.Garrison@noaa.gov>,
mshivlani@rsmas.miami.edu
Subject: Re: new abundance document

Murdoch -

You are reading these papers carefully.

For most of the other fisheries, information about bnd takes comes from opportunistic reports of animals in gear (these reports are often to stranding network members or to researchers studying those fisheries for another reason) or to stranded animals with specific types of net or line marks. Fisheries included in these categories include Virginia pound net, stop net fishery in NC, crab pot entanglements. There is a formal observer program for the beach haul seine but only preliminary estimates of abundance are available. The Take Reduction Team has those estimates - 2 observed takes in 5 years for average annual mortality of 10 dolphins/yr (range of 10-30). It's a high take rate because there are only about 12 fishers in the fishery and it is very temporally and spatially limited so even one take results in a high estimate of mortality. There have been extensive observations of shrimp trawls because of sea turtle takes but no direct observations in commercial boats of bnd takes. However, there was a research take testing shrimping gear so we know it is possible. Furthermore, there is anecdotal info that shrimp boats kill bnd not during trawling but when cleaning the gear - dolphins come by to feed on bycatch discards then get entangled in the lines. We have no official observed takes of this yet. Bnd have been observed taken in menhaden gear in the Gulf of Mexico but not in the Atlantic. On the other hand, there has been little if any observer coverage in the Atlantic. Right now this fishery is not thought to be a large issue. An observer program has started on the Virginia pound nets and the stop net fishery in NC. Both of these fisheries are difficult to observe well because fixed gear that could entangle dolphins is in the water 24 hrs per day for days on end. For both fisheries, the active fishing occurs within the fixed gear at specific times.

We have no number to place on mortality in the above fisheries. It's a definite hole in our data set. But I think all would agree that the gillnet fishery is by far the worst with regard to catching dolphins. The first issue for the NMFS and Take Reduction Team is to reduce dolphin mortality to below PBR. Then second is to reduce mortality to a level approaching a zero mortality rate (not zero mortality). Our thought is that once we accomplish the first goal, primarily by changing the way the gillnets fish, we will need to put more effort into these other fisheries.

Does that answer your question?

Aleta

Murdoch McAllister wrote:

> Aleta,
>
> Thanks for your reply and update of your genetics findings. One additional
> question: Regarding the estimation of bycatch in past years, were there
> any efforts planned or ongoing to try to estimate bottlenose dolphin
> bycatch in fisheries other than the gillnet fisheries (i.e.,beach haul
> seine, crab pot, Virginia pound net, shrimp trawl, and menhaden purse seine
> fisheries)? A paper drafted in 2002 in the set of background papers
> indicated bycatch of bnd in several other fisheries apart from the gillnet
> fisheries. What is current belief about the total number of bnd
> incidentally killed in these other fisheries?
>
> Thanks
> Murdoch
>

Correspondence with Dr Lance Garrison

Date: Mon, 04 Mar 2002 09:04:34 -0500
From: "Lance Garrison" <Lance.Garrison@noaa.gov>
Organization: NOAA
X-Sender: "Lance Garrison" <Lance.Garrison@hqmail.nmfs.noaa.gov>
X-Mailer: Mozilla 4.75 [en]C-CCK-MCD (Windows NT 5.0; U)
X-Accept-Language: en
To: Murdoch McAllister <m.mcallister@ic.ac.uk>
Subject: Re: method to compute distance of sighting from coast

Hi Murdoch,

The distance from shore was calculated using the GIS software ARCVIEW. The map co-ordinates of sightings (Latitude, Longitude) are projected into kilometers using a Universal Transverse Mercator - Zone 18 projection. The perpendicular distance from the land coverage I am using are then calculated by the spatial analyst package in arcview. Incidentally, before I know how to use arcview, I also calculated these as a distance from the starting point of the transect, which was generally at the shoreline, using a geodetic inverse method. Both methods give the same answer, within rounding error.

I'm not sure that I put these in the paper, hope this clears things up.

Lance

Murdoch McAllister wrote:

> Dear Lance,
>
> I have a question for you regarding the paper "Seeking a hiatus in
> sightings ..."
>
> How did you determine the distance from shore of each of the sightings

> along a transect? Perhaps this was stated somewhere in the paper or
> another paper but I am currently unable to find a section describing how
> this was done.
>
> Regards,
> Murdoch
>
Date: Mon, 04 Mar 2002 11:04:54 -0500
From: "Lance Garrison" <Lance.Garrison@noaa.gov>
Organization: NOAA
X-Sender: "Lance Garrison" <Lance.Garrison@hqmail.nmfs.noaa.gov>
X-Mailer: Mozilla 4.75 [en]C-CCK-MCD (Windows NT 5.0; U)
X-Accept-Language: en
To: Murdoch McAllister <m.mcallister@ic.ac.uk>
Subject: Re: new abundance document

Murdoch,

I can speak about the Florida shark gillnet fishery as its a bit unique. In that case, there is a very small universe of vessels, currently only six boats. The owners are required to tell NMFS when they are going out to target sharks and therefore carry observers under the Highly Migratory Species Fisheries Management Plan (HMS-FMP). During the Right Whale season from January to March, 100% observer coverage is mandated. So an observer is placed upon every vessel, assuming that they have 1) called in and 2) funding is available for the observer. Because it is a small and localized group of vessels, generally NMFS knows when people are going out. Funding is the bigger issue. The observer program generally runs out of funds by the end of Feb. because they are required to have such high coverage. As a result, the fishery is largely unobserved during the remainder of the year when the effort shifts further north. As I point out in the paper, the bycatch rates are really only representative of the covered period, and there is some question about extraploating these rates across the annual effort.

Another note on fishery interactions in this area, is that there are other vessels operating gillnets in federal waters (> 3 miles offshore). These are not "shark" trips and therefore are not regulated under the HMS-FMP, even though they often land sharks. These gillnets are unobserved, their effort is difficult to define, the net and vessel characteristics are highly variable, and they may (likely ?) interact with protected resources including both turtles and mammals. This additional effort is a black hole that people have been trying to deal with for a long time.

Hope this helps,

Lance

Murdoch McAllister wrote:

> Hello,
>
> I have a question for NMFS scientists regarding the observer data used in
> the estimation of bycatch rates:
>

> What were the protocols applied for determining which fishing vessels
> observers were placed on in the various shark and mid-Atlantic gillnet
> fisheries, when observer coverage was not 100%?
>
> Regards,
> Murdoch McAllister
>

Date: Mon, 04 Mar 2002 11:05:47 -0500
From: "Lance Garrison" <Lance.Garrison@noaa.gov>
Organization: NOAA
X-Sender: "Lance Garrison" <Lance.Garrison@hqmail.nmfs.noaa.gov>
X-Mailer: Mozilla 4.75 [en]C-CCK-MCD (Windows NT 5.0; U)
X-Accept-Language: en
To: Murdoch McAllister <m.mcallister@ic.ac.uk>
Subject: Re: method to compute distance of sighting from coast

Yes, that is correct.

Murdoch McAllister wrote:

> Hi Lance,
>
> Thanks for your reply. So I take it that this means that for each sighting
> observation you computed the distance between the sighting and the nearest
> point of land as the distance from shore; is this correct?
>
> Thanks,
> Murdoch
>
> At 09:04 AM 3/4/02 -0500, Lance Garrison wrote:
> >Hi Murdoch,
> >
> > The distance from shore was calculated using the GIS software ARCVIEW. The
> >map co-ordinates of sightings (Latitude, Longitude) are projected into
> >kilometers using a Universal Transverse Mercator - Zone 18 projection. The
> >perpendicular distance from the land coverage I am using are then
> >calculated by
> >the spatial analyst package in arcview. Incidentally, before I know how
> >to use
> >arcview, I also calculated these as a distance from the starting point of the
> >transect, which was generally at the shoreline, using a geodetic inverse
> >method. Both methods give the same answer, within rounding error.
> >
> >I'm not sure that I put these in the paper, hope this clears things up.
> >
> >Lance
> >
> >Murdoch McAllister wrote:
> >
> > > Dear Lance,
> > >

> > > I have a question for you regarding the paper "Seeking a hiatus in
> > > sightings ..."
> > >
> > > How did you determine the distance from shore of each of the sightings
> > > along a transect? Perhaps this was stated somewhere in the paper or
> > > another paper but I am currently unable to find a section describing how
> > > this was done.
> > >
> > > Regards,
> > > Murdoch

Date: Wed, 06 Mar 2002 08:20:57 -0500
From: "Lance Garrison" <Lance.Garrison@noaa.gov>
Organization: NOAA
X-Sender: "Lance Garrison" <Lance.Garrison@hqmail.nmfs.noaa.gov>
X-Mailer: Mozilla 4.75 [en]C-CCK-MCD (Windows NT 5.0; U)
X-Accept-Language: en
To: Murdoch McAllister <m.mcallister@ic.ac.uk>
Subject: Re: g(0) in Feb 2002 paper

Hi Murdoch...

Second question first, I'm not sure which version of the paper was sent to you. But after some discussion, we did revise it to use the distance from shore strata defined in the previous analyses before presenting it to the take reduction team. The boundaries are 0-27 km from shore south of Cape Hatteras and 0-12 km from shore North of Hatteras, though the 0-12 km from shore stratum is actually based upon summer sightings. The areas are generally similar to the depth strata, however, and made very little difference in the overall estimate.

First question second, I have not yet completed the g(0) analysis including the variance. Using the "direct-duplicate" estimator shown in the paper, the g(0) was very close to one, actually 1.0027. Now that the survey has been completed, I will re-evaluate g(0) and its variance using several different methods that are out in the literature. These are considerably more complex than that used in the preliminary paper.

Lance

Murdoch McAllister wrote:

> Hi Lance,
>
> I am due to hand in my report to CIE today but I have a few additional
> questions about the estimation of g(0) in the new Feb. 2002 paper that you
> co-authored with W Hoggard.
>
> (1) If you managed to obtain these in the analyses conducted for the paper,
> what was the empirical estimate of g(0) and the CV in this estimate?
> (2) Why did you not stick with the 27 km offshore winter boundary as the
> winter stratum for winter abundance estimation as you did in your 2001 papers?
>
> Best wishes,

> Murdoch

Date: Wed, 06 Mar 2002 08:22:16 -0500
From: "Lance Garrison" <Lance.Garrison@noaa.gov>
Organization: NOAA
X-Sender: "Lance Garrison" <Lance.Garrison@hqmail.nmfs.noaa.gov>
X-Mailer: Mozilla 4.75 [en]C-CCK-MCD (Windows NT 5.0; U)
X-Accept-Language: en
To: Murdoch McAllister <m.mcallister@ic.ac.uk>
Subject: Re: Garrison and Yeung (2001)

That would be the 27km limit in the winter.

Lance

Murdoch McAllister wrote:

> Dear Lance,
>
> I have one more question regarding your Garrison and Yeung (2001)
> paper. In Table 1 for the North North Carolina Management Unit, there is a
> coastal area of 3309 km squared for Winter. What value for the offshore
> limit was used for this calculation (27 km or 12km)?
>
> Thanks,
> Murdoch

Correspondence with Robin Baird

From: "Robin Baird" <Robin.Baird@noaa.gov>
To: Murdoch McAllister <m.mcallister@ic.ac.uk>
Cc: "Aleta Hohn" <Aleta.Hohn@noaa.gov>,robin.baird@noaa.gov,
jennifer.lawrence@noaa.gov,mshivlani@rsmas.miami.edu
Date: Thu, 28 Feb 2002 08:40:19 -0500
X-Mailer: Netscape Webmail
Subject: Re: Fwd: Re: new abundance document
X-Accept-Language: en

Dear Murdoch,

Just to let you know that we will fax the Wade paper (and anything else
appropriate we can find) in the next couple of hours.

Robin

----- Original Message -----

From: Murdoch McAllister <m.mcallister@ic.ac.uk>
Date: Thursday, February 28, 2002 5:31 am
Subject: Re: Fwd: Re: new abundance document

> Dear Aleta,
>

> Thanks for checking into finding a paper on PBR. I'd be grateful
> if it
> could be FAXED by Friday 1, March, since I am due to send in my
> report to
> the CIE on Monday March 4.
>
> Best wishes,
> Murdoch McAllister
>
> At 07:19 AM 2/27/02 -0500, Aleta Hohn wrote:
> >Robin and Jenn -
> >
> >Would you mind faxing a copy of Paul Wade's PBR paper from Marine
> Mammal>Science in 1999 or so to Murdoch. Or if you have a PDF
> version, Rogin,
> >that would be even better. Sorry I didn't see this before I left
> >yesterday. Robin - can you think of any other key PBR papers
> that might
> >be helpful to Murdoch?
> >
> >Thanks.
> >
> >Aleta

Figure Captions

Figure 1. Distribution of isotope samples from Atlantic bottlenose dolphins analysed and stratified by the level of the oxygen^{18} isotope. Figure provided by Dr. Aleta Hohn (NEFSC): http://shrimp.ccfhrb.noaa.gov/~ahohn/CIE/

Figure 2. Seasonal distribution of samples from Atlantic bottlenose dolphins with depleted oxygen. Figure provided by Dr. Aleta Hohn (NEFSC): http://shrimp.ccfhrb.noaa.gov/~ahohn/CIE/

Figure 3. Bottlenose dolphin biopsies from central North Carolina in fall-winter 2001-2002 indicating the sample locations of each biotype. Figure provided by Dr. Aleta Hohn (NEFSC).

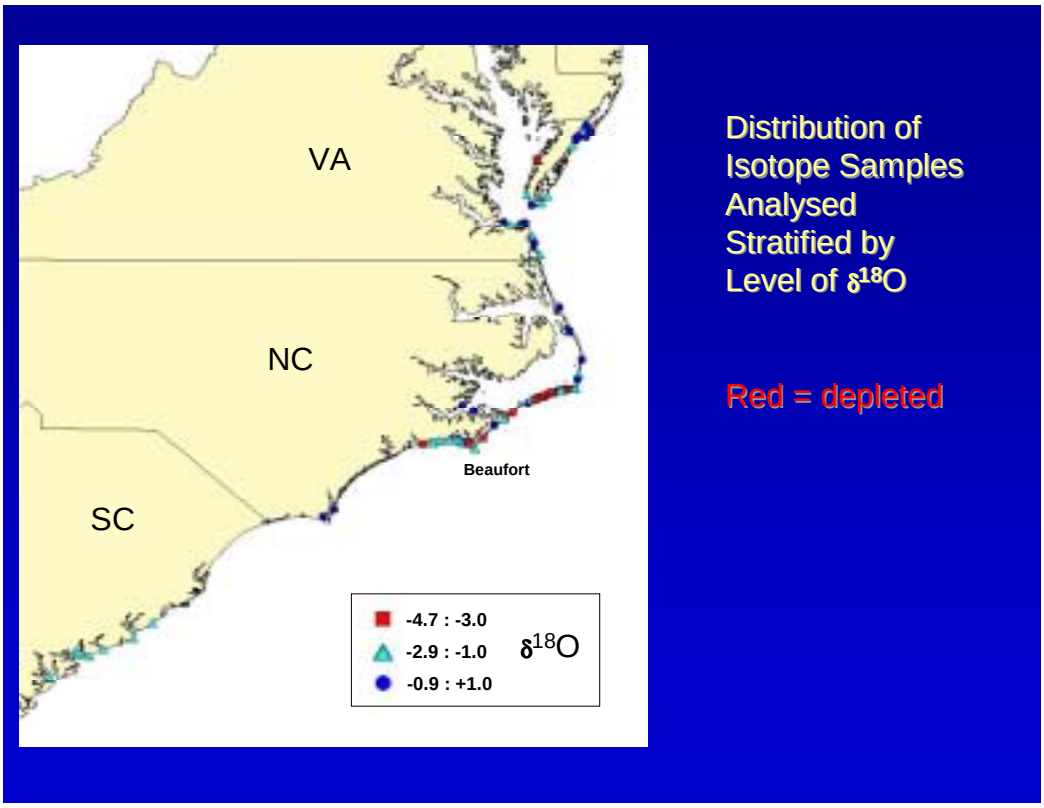
VA
NC
SC
Beaufort
-4.7 : -3.0
-2.9 : -1.0
-0.9 : +1.0
δ18O
Distribution of Isotope Samples Analysed Stratified by Level of δ18O
Red = depleted

Figure 1

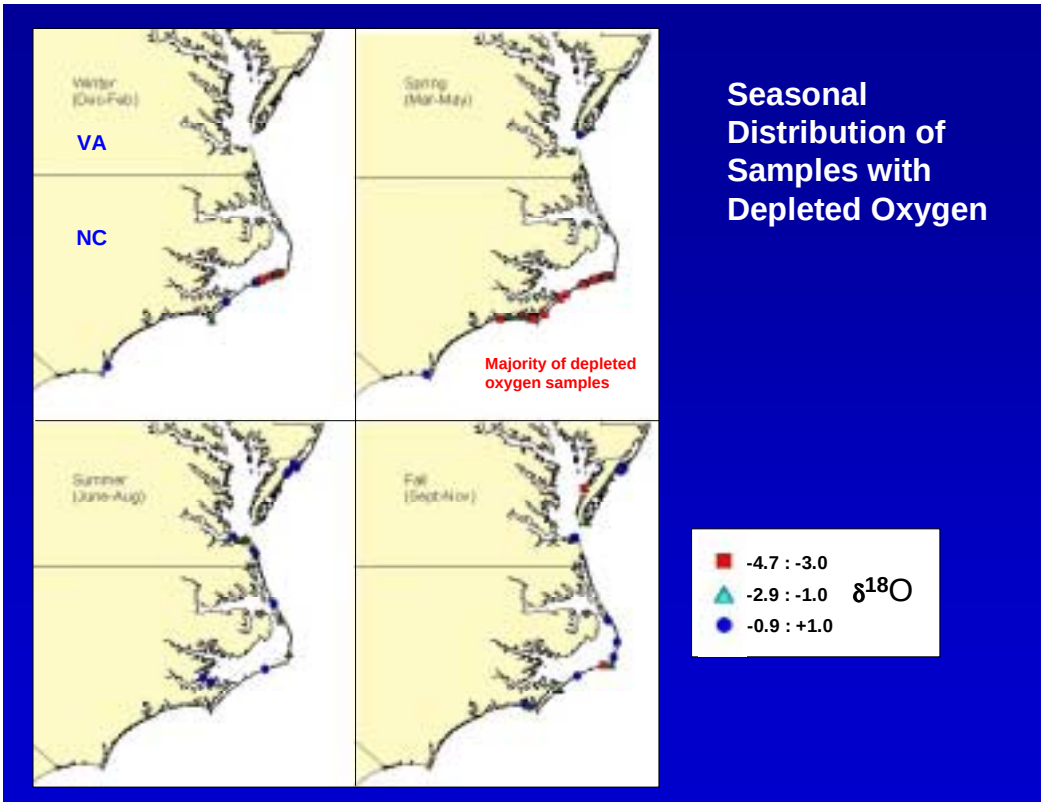
Winter
(Dec-Feb)
VA
NC
Spring
(Mar-May)
Majority of depleted oxygen samples
Summer
(June-Aug)
Fall
(Sept-Nov)
Seasonal Distribution of Samples with Depleted Oxygen
-4.7 : -3.0
-2.9 : -1.0
-0.9 : +1.0
δ18O

Figure 2

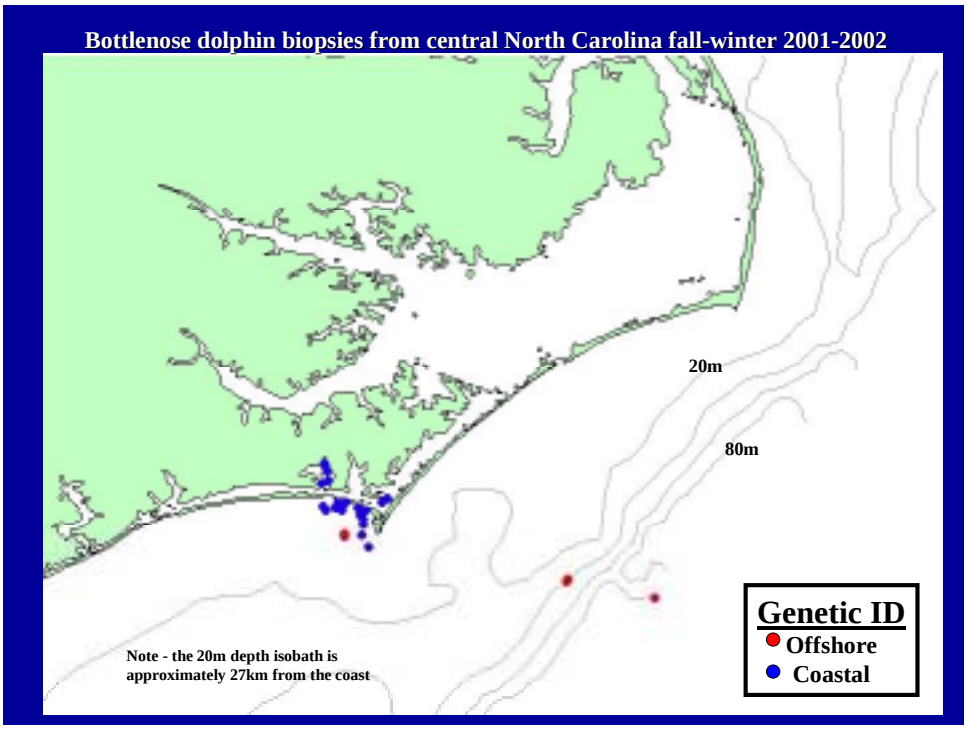
Bottlenose dolphin biopsies from central North Carolina fall-winter 2001-2002
20m
80m
Note - the 20m depth isobath is approximately 27km from the coast
Genetic ID
Offshore
Coastal

Figure 3

APPENDIX B: STATEMENT OF WORK

STATEMENT OF WORK

CONSULTING AGREEMENT BETWEEN THE UNIVERSITY OF MIAMI AND DR. MURDOCH MCALLISTER

March 12, 2002

GENERAL[1]

THE MARINE MAMMAL PROTECTION ACT REQUIRES THE NATIONAL MARINE FISHERIES SERVICE (NMFS) TO DEVELOP AND IMPLEMENT A TAKE REDUCTION PLAN TO ASSIST IN THE RECOVERY OR PREVENT THE DEPLETION OF STRATEGIC STOCKS OF MARINE MAMMALS THAT INTERACT WITH COMMERCIAL FISHERIES THAT FREQUENTLY (CATEGORY I) OR OCCASIONALLY (CATEGORY II) CAUSE INCIDENTAL MORTALITY OR SERIOUS INJURY TO MARINE MAMMALS. TAKE REDUCTION TEAMS ARE CONVENED TO DEVELOP DRAFT TAKE REDUCTION PLANS, WHICH ARE IMPLEMENTED BY NMFS THROUGH REGULATIONS. TAKE REDUCTION TEAMS CONSIST OF REPRESENTATIVES FROM THE COMMERCIAL AND RECREATIONAL FISHING INDUSTRY, CONSERVATION GROUPS, FEDERAL AND STATE GOVERNMENT, FISHERY MANAGEMENT COUNCILS, INTERSTATE FISHERIES COMMISSIONS, AND ACADEMIC AND SCIENTIFIC ORGANIZATIONS.

THE IMMEDIATE GOAL OF A TAKE REDUCTION PLAN IS TO REDUCE, WITHIN 6 MONTHS OF IMPLEMENTATION, THE INCIDENTAL MORTALITY OR SERIOUS INJURY OF A MARINE MAMMAL STOCK FROM COMMERCIAL FISHING OPERATIONS TO A SUSTAINABLE LEVEL, REFERRED TO AS THE POTENTIAL BIOLOGICAL REMOVAL LEVEL. THE TAKE REDUCTION PROCESS IS CONTENTIOUS, BRINGING TOGETHER PEOPLE WITH VERY DIFFERENT PERSPECTIVES TO DEVELOP A CONSENSUS-BASED APPROACH FOR REDUCING MARINE MAMMAL MORTALITY INCIDENTAL TO COMMERCIAL FISHING. THE INTENT IS TO DEVELOP A MANAGEMENT PROGRAM THAT MEETS CONSERVATION GOALS AND MINIMIZES THE POTENTIAL IMPACT ON THE FISHING INDUSTRY.

THE BOTTLENOSE DOLPHIN TAKE REDUCTION TEAM (TEAM) WAS CONVENED IN NOVEMBER OF 2001 TO ADDRESS MORTALITY OF THE WESTERN NORTH ATLANTIC COASTAL STOCK OF BOTTLENOSE DOLPHINS INCIDENTAL TO NINE CATEGORY II COMMERCIAL FISHERIES THAT OCCUR ALONG THE EAST COAST OF THE UNITED STATES. THE WESTERN NORTH ATLANTIC COASTAL STOCK OF BOTTLENOSE DOLPHINS IS A STRATEGIC STOCK. STRATEGIC STATUS WAS INITIALLY ASSIGNED BECAUSE THE STOCK IS DESIGNATED AS DEPLETED UNDER THE MARINE MAMMAL PROTECTION ACT AS A RESULT OF A LARGE-SCALE MORTALITY EVENT THAT OCCURRED IN 1987-1988. HOWEVER, THE STOCK ALSO QUALIFIES TO BE STRATEGIC BECAUSE MORTALITY AND SERIOUS INJURY INCIDENTAL TO COMMERCIAL FISHING EXCEEDS SUSTAINABLE LEVELS.

THE DATA USED IN THE ANALYSES CONDUCTED TO DEVELOP STOCK STRUCTURE, ABUNDANCE, AND FISHERY-RELATED MORTALITY INFORMATION FOR USE BY THE TEAM ARE NOT FINAL AND WILL CONTINUE TO BE SUPPLEMENTED BY RESULTS FROM ONGOING RESEARCH EFFORTS. THE DOCUMENTS SUBMITTED FOR REVIEW HAVE ALREADY BEEN PEER-REVIEWED BY THE ATLANTIC SCIENTIFIC REVIEW GROUP THROUGH A SYSTEM ESTABLISHED BY THE MARINE MAMMAL PROTECTION ACT. MEMBERS OF THE SCIENTIFIC REVIEW GROUPS ARE INDIVIDUALS WITH EXPERTISE IN MARINE MAMMAL BIOLOGY AND ECOLOGY, POPULATION DYNAMICS AND MODELING, AND COMMERCIAL FISHING TECHNOLOGY PRACTICES.

AS REQUIRED BY THE MARINE MAMMAL PROTECTION ACT, NMFS HAS CONVENED THE TEAM AND MUST USE THE BEST AVAILABLE INFORMATION TO SUPPORT THE TEAM. ALTHOUGH THE TEAM IS REQUIRED BY THE MARINE MAMMAL PROTECTION ACT TO SUBMIT A DRAFT TAKE REDUCTION PLAN TO NMFS IN MAY OF 2002, THEIR INVOLVEMENT IN THE PROCESS DOES NOT END AT THAT TIME. TAKE REDUCTION TEAMS CONTINUE TO

[1] See Attachment A for definitions of the following terms: strategic stock, potential biological removal level, depleted, optimum sustainable population, and fishery classifications (Category I, II, and III fisheries).

MEET WITH NMFS ON A REGULAR BASIS TO MONITOR PLAN IMPLEMENTATION AND RECEIVE NEW INFORMATION RESULTING FROM ONGOING RESEARCH EFFORTS. AT THESE MEETINGS, THE TEAM HAS THE OPPORTUNITY TO MAKE RECOMMENDATIONS TO REVISE THE REGULATIONS. ATTACHMENT B SHOWS THE CHRONOLOGY OF EVENTS RELATED TO CONVENING THE TEAM.

SPECIFIC

EACH REVIEWER SHALL ANALYZE SEVEN DOCUMENTS ADDRESSING BOTTLENOSE DOLPHIN STOCK STRUCTURE, ABUNDANCE ESTIMATES, AND MORTALITY ESTIMATES (TASK 2, ITEMS A-G). THE SEVEN DOCUMENTS REPRESENT AN INTERIM APPROACH TO BOTTLENOSE DOLPHIN SCIENCE. NMFS RECOGNIZES THAT THEY PRESENT WORK IN PROGRESS (E.G., STOCK IDENTIFICATION), *A POSTERIORI* ANALYSIS OF PREVIOUS DATA SETS USING NEW STOCK STRATA (E.G., SOUTHEAST SURVEYS FOR ABUNDANCE), AND, IN SOME CASES, LIMITED SAMPLE SIZES. HOWEVER, THIS INFORMATION IS A SIGNIFICANT IMPROVEMENT OVER THE INFORMATION AVAILABLE PRIOR TO THE COMPILATION OF THESE DOCUMENTS AND, THEREFORE, PROVIDES MORE APPROPRIATE RESULTS FOR USE BY THE TEAM. EACH REVIEWER SHALL REVIEW THESE DOCUMENTS IN THAT CONTEXT.

THE REVIEWERS SHOULD FOCUS ON THE FOLLOWING ISSUES WHEN WORKING ON TASK 2:

- ARE THE DATA USED APPROPRIATELY?
- ARE THE ANALYSES CONDUCTED APPROPRIATE?
- ARE THE DATA ADEQUATE FOR THE ANALYSES CONDUCTED?
- ARE ASSUMPTIONS INHERENT IN THE ANALYSES APPROPRIATELY CONSIDERED?
- ARE THE INTERPRETATIONS OF THE DATA AND ANALYSES APPROPRIATE?

ADDITIONALLY, THE REVIEWERS SHALL PROVIDE SUGGESTIONS FOR ALTERNATIVE METHODS FOR ANALYZING OR INTERPRETING THE INFORMATION, WHERE BETTER METHODS EXIST. EACH REVIEWER SHALL CONCLUDE, IN A WRITTEN REPORT, WHETHER THE ANALYSES REPRESENT THE BEST ANALYSIS OF THE AVAILABLE INFORMATION.

EACH REVIEWER'S DUTIES SHALL NOT EXCEED A MAXIMUM TOTAL OF SEVEN DAYS, INCLUDING SEVERAL DAYS FOR DOCUMENT REVIEW, SEVERAL DAYS TO PRODUCE A WRITTEN REPORT OF THE FINDINGS, AND SEVERAL HOURS FOR A CONFERENCE CALL. NO TRAVEL IS REQUIRED, AND THEREFORE EACH REVIEWER MAY PERFORM ALL REVIEW, ANALYSIS, AND WRITING DUTIES OUT OF THE REVIEWER'S PRIMARY LOCATION. A CONSENSUS REPORT IS NOT REQUIRED.

THE CENTER OF INDEPENDENT EXPERTS (CIE) SHALL SCHEDULE AND FACILITATE A CONFERENCE CALL WITH THE REVIEWERS AND NMFS SCIENTISTS AND MANAGERS TO PROVIDE AN OPPORTUNITY FOR THE REVIEWERS TO ASK QUESTIONS ABOUT THE SCIENCE AND THE TAKE REDUCTION PROCESS PRIOR TO THE REVIEWERS WRITING THEIR REPORTS AS DETAILED UNDER TASK 4 AND ANNEX I. THE AGENDA FOR THE CONFERENCE CALL SHALL CONSIST OF: 1) GENERAL/PROGRAMMATIC ISSUES; (2) QUESTIONS SPECIFIC TO EACH DOCUMENT LISTED IN TASK 2. THE CIE SHALL PROVIDE A TOLL-FREE PHONE NUMBER FOR THE CONFERENCE CALL. IDEALLY, ONE CONFERENCE CALL WILL BE HELD WITH ALL OF THE REVIEWERS. HOWEVER, MORE THAN ONE CONFERENCE CALL MAY BE ARRANGED IF ALL OF THE REVIEWERS ARE NOT AVAILABLE AT THE SAME TIME.

THE ITEMIZED TASKS OF EACH REVIEWER INCLUDE:

TASK 1: SURVEYING THE FOLLOWING SUPPLEMENTARY INFORMATION PROVIDED TO EACH REVIEWER PRIOR TO COMPLETING TASK 2 FOR ADDITIONAL CONTEXT AND BACKGROUND ABOUT BOTTLENOSE DOLPHINS, THE TAKE REDUCTION PROCESS, AND THE ATLANTIC MARINE MAMMAL SCIENTIFIC REVIEW GROUP REVIEW. THE REVIEWER SHOULD NOT ANALYZE THESE DOCUMENTS FOR THE WRITTEN REPORT.

THE FOLLOWING TWO DOCUMENTS PROVIDE AN OVERVIEW OF BOTTLENOSE DOLPHIN SCIENCE, AS PRESENTED IN NMFS STOCK ASSESSMENT REPORT PUBLICATIONS. THE FIRST DOCUMENT REPRESENTS THE CURRENT STATE OF KNOWLEDGE, SUMMARIZING THE DETAILED INFORMATION CONTAINED IN THE REVIEW DOCUMENTS IDENTIFIED IN TASK 2. THE SECOND DOCUMENT IS PROVIDED TO SHOW WHAT INFORMATION WAS AVAILABLE BEFORE THE REVIEW DOCUMENTS WERE PREPARED.

- NATIONAL MARINE FISHERIES SERVICE. NOVEMBER 2001. DRAFT 2002 STOCK ASSESSMENT REPORT FOR THE WESTERN NORTH ATLANTIC COASTAL STOCK OF BOTTLENOSE DOLPHIN (*TURSIOPS TRUNCATUS*).

- NATIONAL MARINE FISHERIES SERVICE. SEPTEMBER 2000. 2000 STOCK ASSESSMENT REPORT FOR THE WESTERN NORTH ATLANTIC COASTAL STOCK OF BOTTLENOSE DOLPHIN (*TURSIOPS TRUNCATUS*).

THE FOLLOWING TWO DOCUMENTS RELATE TO THE REVIEW CONDUCTED BY THE ATLANTIC SCIENTIFIC REVIEW GROUP, WHICH REVIEWED THE SAME OR EARLIER VERSIONS OF THE DOCUMENTS IDENTIFIED IN TASK 2.

- ATLANTIC SCIENTIFIC REVIEW GROUP REVIEW OF BOTTLENOSE DOLPHIN DOCUMENTS. OCTOBER 2001.

- NMFS RESPONSE TO THE ATLANTIC SCIENTIFIC REVIEW GROUP. NOVEMBER 2001.

NMFS GAVE TEAM MEMBERS THE OPPORTUNITY TO IDENTIFY QUESTIONS ABOUT THE DOCUMENTS IDENTIFIED IN TASK 2. ONLY ONE TEAM MEMBER PROVIDED COMMENTS. THE REVIEWERS ARE NOT REQUIRED TO RESPOND TO THESE COMMENTS.

- COMMENTS FROM ONE MEMBER OF THE TEAM FOR THE CIE PEER REVIEW. DECEMBER 2001.

THE FOLLOWING TWO DOCUMENTS IDENTIFY CONCERNS OF ONE REPRESENTATIVE FROM THE FISHING INDUSTRY ABOUT BOTTLENOSE DOLPHIN SCIENCE AND THE TAKE REDUCTION PROCESS AND NMFS RESPONSE.

- LETTER FROM RICK MARKS TO THE HONORABLE JAMES V. HANSEN AND THE HONORABLE DON YOUNG OF THE U.S. HOUSE OF REPRESENTATIVES RESOURCES COMMITTEE REGARDING THE BOTTLENOSE DOLPHIN TAKE REDUCTION TEAM PROCESS. AUGUST 2001.

- NMFS RESPONSE TO RICK MARKS LETTER TO THE HOUSE RESOURCES COMMITTEE. SEPTEMBER 2001.

THE FOLLOWING THREE DOCUMENTS PROVIDE DESCRIPTIVE INFORMATION ABOUT BOTTLENOSE DOLPHIN STRANDINGS ALONG THE EAST COAST OF THE U.S.

- PALKA, D., F. WENZEL, D. L. HARTLEY, AND M. ROSSMAN. JUNE 2001. SUMMARY OF BOTTLENOSE DOLPHIN STRANDINGS FROM NEW YORK TO VIRGINIA.

- HOHN A., P. T. MARTONE. JULY 2001. CHARACTERIZATION OF BOTTLENOSE DOLPHIN STRANDINGS IN NORTH CAROLINA, 1997-2000.

- HOHN A., B. MASE, J. LITZ, W. MCFEE, AND B. ZOODSMA. NOVEMBER 2001. CHARACTERIZATION OF HUMAN-CAUSED STRANDINGS OF BOTTLENOSE DOLPHINS ALONG THE ATLANTIC COAST FROM SOUTH CAROLINA TO FLORIDA, 1997-2000.

TASK 2: READING AND ANALYZING THE FOLLOWING DOCUMENTS (A-G) PROVIDED TO EACH REVIEWER. THIS IS THE PRIMARY TASK OF THE CONTRACT. THE REPORT IDENTIFIED IN TASK 4 AND IN ANNEX I SHOULD ADDRESS THESE DOCUMENTS.

STOCK STRUCTURE

a. NATIONAL MARINE FISHERIES SERVICE. JUNE 2001. PRELIMINARY STOCK STRUCTURE OF COASTAL BOTTLENOSE DOLPHINS ALONG THE ATLANTIC COAST OF THE U.S.

b. GARRISON, L. JUNE 2001. SEEKING A HIATUS IN SIGHTINGS FOR BOTTLENOSE DOLPHIN DURING SUMMER AND WINTER AERIAL SURVEYS. NATIONAL MARINE FISHERIES SERVICE.

ABUNDANCE ESTIMATES

C. GARRISON, L. AND A. HOHN. OCTOBER 2001. ABUNDANCE ESTIMATES FOR ATLANTIC BOTTLENOSE DOLPHINS: COMBINING STRIP TRANSECT DATA AND LINE TRANSECT ABUNDANCE ESTIMATION. NATIONAL MARINE FISHERIES SERVICE.

D. GARRISON, L. AND C. YEUNG. 15 JUNE 2001. ABUNDANCE ESTIMATES FOR ATLANTIC BOTTLENOSE DOLPHIN STOCKS DURING SUMMER AND WINTER, 1995. NATIONAL MARINE FISHERIES SERVICE.

E. PALKA, D., L. GARRISON, A. HOHN, AND C. YEUNG. 1 NOVEMBER 2001. SUMMARY OF ABUNDANCE ESTIMATES AND PBR FOR COASTAL *TURCIOPS* FOR WATERS BETWEEN NEW YORK AND FLORIDA DURING 1995 TO 2000. NATIONAL MARINE FISHERIES SERVICE.*MORTALITY ESTIMATES*

F. GARRISON, L. 2 JULY 2001. MORTALITY ESTIMATE FOR ATLANTIC BOTTLENOSE DOLPHIN IN THE DIRECTED SHARK GILLNET FISHERY OF FLORIDA AND GEORGIA. NATIONAL MARINE FISHERIES SERVICE.

G. ROSSMAN, M. AND D. PALKA. 3 OCTOBER 2001. BYCATCH ESTIMATES OF COASTAL BOTTLENOSE DOLPHIN (*TURCIOPS TRUNCATUS*) IN U.S. MID-ATLANTIC GILLNET FISHERIES FOR 1996 TO 2000. NATIONAL MARINE FISHERIES SERVICE.

TASK 3: PARTICIPATE IN A CONFERENCE CALL, TO BE ARRANGED BY CIE, WITH NMFS SCIENTISTS AND MANAGERS TO DISCUSS QUESTIONS EACH REVIEWER MAY HAVE ABOUT THE SCIENCE AND THE TAKE REDUCTION PROCESS.

TASK 4: NO LATER THAN MARCH 1, 2002, EACH REVIEWER SHALL SUBMIT A WRITTEN, NON-CONSENSUS REPORT OF FINDINGS, ANALYSIS, AND CONCLUSIONS BASED OF THEIR REVIEW OF THE DOCUMENTS (TASK 2, ITEMS A-G). THE REPORT SHOULD BE ADDRESSED TO THE UNIVERSITY OF MIAMI INDEPENDENT SYSTEM FOR PEER REVIEWS AND SENT TO DR. DAVID DIE, UNIVERSITY OF MIAMI/RSMAS, 4600 RICKENBACKER CAUSEWAY, MIAMI, FL 33149 (OR VIA EMAIL TO DDIE@RSMAS.MIAMI.EDU).

SIGNED_____ DATE_____

ANNEX I: REPORT GENERATION AND PROCEDURAL ITEMS

3. THE REPORT SHOULD BE PREFACED WITH AN EXECUTIVE SUMMARY OF FINDINGS AND/OR RECOMMENDATIONS.

4. THE MAIN BODY OF THE REPORT SHOULD CONSIST OF A BACKGROUND, DESCRIPTION OF REVIEW ACTIVITIES, SUMMARY OF FINDINGS, AND CONCLUSIONS/RECOMMENDATIONS.

5. THE REPORT SHOULD ALSO INCLUDE AS SEPARATE APPENDICES THE BIBLIOGRAPHY OF MATERIALS PROVIDED BY THE CENTER FOR INDEPENDENT EXPERTS AND NMFS AND A COPY OF THE STATEMENT OF WORK.

ATTACHMENT A
DEFINITIONS

STRATEGIC STOCK IS DEFINED IN SECTION 3(19) OF THE MMPA TO MEAN A MARINE MAMMAL STOCK, "(A) FOR WHICH THE LEVEL OF DIRECT HUMAN-CAUSED MORTALITY EXCEEDS THE POTENTIAL BIOLOGICAL REMOVAL LEVEL; (B) WHICH, BASED ON THE BEST AVAILABLE SCIENTIFIC INFORMATION, IS DECLINING AND IS LIKELY TO BE LISTED AS A THREATENED SPECIES UNDER THE ENDANGERED SPECIES ACT (ESA) OF 1973 WITHIN THE FORESEEABLE FUTURE OR (C) WHICH IS LISTED AS A THREATENED SPECIES OR ENDANGERED SPECIES UNDER THE ENDANGERED SPECIES ACT OF 1973 (16 U.S.C. 1531 ET SEQ.), OR IS DESIGNATED AS DEPLETED UNDER THIS ACT [MMPA]."

POTENTIAL BIOLOGICAL REMOVAL (PBR) LEVEL IS DEFINED IN SECTION 3(20) OF THE MMPA TO MEAN, IN RELEVANT PART, "THE MAXIMUM NUMBER OF ANIMALS, NOT INCLUDING NATURAL MORTALITIES, THAT MAY BE REMOVED FROM A MARINE MAMMAL STOCK WHILE ALLOWING THAT STOCK TO REACH OR MAINTAIN ITS OPTIMUM SUSTAINABLE POPULATION."

DEPLETED IS DEFINED BY SECTION 3(1) OF THE MMPA TO MEAN ANY CASE IN WHICH, "(A) THE SECRETARY, AFTER CONSULTATION WITH THE MARINE MAMMAL COMMISSION AND THE COMMITTEE OF SCIENTIFIC ADVISORS ON MARINE MAMMALS ESTABLISHED UNDER TITLE II OF THIS ACT, DETERMINED THAT A SPECIES OR POPULATION STOCK IS BELOW ITS OPTIMUM SUSTAINABLE POPULATION; (B) A STATE, TO WHICH AUTHORITY FOR THE CONSERVATION AND MANAGEMENT OF A SPECIES OR POPULATION STOCK IS TRANSFERRED UNDER SECTION 109, DETERMINES THAT SUCH SPECIES OR POPULATION STOCK IS BELOW ITS OPTIMUM SUSTAINABLE POPULATION; OR (C) A SPECIES OR POPULATION STOCK IS LISTED AS AN ENDANGERED SPECIES OR A THREATENED SPECIES UNDER THE ENDANGERED SPECIES ACT OF 1973."

OPTIMUM SUSTAINABLE POPULATION IS DEFINED BY SECTION 3(9) OF THE MMPA TO MEAN, "WITH RESPECT TO ANY POPULATION STOCK, THE NUMBER OF ANIMALS WHICH WILL RESULT IN THE MAXIMUM PRODUCTIVITY OF THE POPULATION OR THE SPECIES, KEEPING IN MIND THE CARRYING CAPACITY OF THE HABITAT AND THE HEALTH OF THE ECOSYSTEM OF WHICH THEY FORM A CONSTITUENT ELEMENT."

FISHERY CLASSIFICATION IS DEFINED IN SECTION 118(C) OF THE MMPA AND IMPLEMENTED BY REGULATION IN 50 CFR PART 229. THE FISHERY CLASSIFICATION CRITERIA CONSIST OF A TWO-TIERED, STOCK-SPECIFIC APPROACH THAT FIRST ADDRESSES THE TOTAL IMPACT OF ALL FISHERIES ON EACH MARINE MAMMAL STOCK AND THEN ADDRESSES THE IMPACT OF INDIVIDUAL FISHERIES ON EACH STOCK. THIS APPROACH IS BASED ON CONSIDERATION OF THE RATE, IN NUMBERS OF ANIMALS PER YEAR, OF INCIDENTAL MORTALITIES AND SERIOUS INJURIES OF MARINE MAMMALS DUE TO COMMERCIAL FISHING OPERATIONS RELATIVE TO THE PBR LEVEL FOR EACH MARINE MAMMAL STOCK.

- TIER 1: IF THE TOTAL ANNUAL MORTALITY AND SERIOUS INJURY ACROSS ALL FISHERIES THAT INTERACT WITH A STOCK IS LESS THAN OR EQUAL TO 10 PERCENT OF THE PBR LEVEL OF THIS STOCK, ALL FISHERIES INTERACTING WITH THIS STOCK WOULD BE PLACED IN CATEGORY III. OTHERWISE, THESE FISHERIES ARE SUBJECT TO THE NEXT TIER OF ANALYSIS TO DETERMINE THEIR CLASSIFICATION.
- TIER 2, CATEGORY I: ANNUAL MORTALITY AND SERIOUS INJURY OF A STOCK IN A GIVEN FISHERY IS GREATER THAN OR EQUAL TO 50 PERCENT OF THE PBR LEVEL.
- TIER 2, CATEGORY II: ANNUAL MORTALITY AND SERIOUS INJURY OF A STOCK IN A GIVEN FISHERY IS GREATER THAN 1 PERCENT AND LESS THAN 50 PERCENT OF THE PBR LEVEL.
- TIER 2, CATEGORY III: ANNUAL MORTALITY AND SERIOUS INJURY OF A STOCK IN A GIVEN FISHERY IS LESS THAN OR EQUAL TO 1 PERCENT OF THE PBR LEVEL.

ATTACHMENT B

CHRONOLOGY OF EVENTS RELATED TO THE BOTTLENOSE DOLPHIN TAKE REDUCTION TEAM

1997 NMFS INITIATED TAKE REDUCTION EFFORTS FOR BOTTLENOSE DOLPHINS IN 1997 AS PART OF THE MID-ATLANTIC TAKE REDUCTION TEAM. AT THE TIME, DATA ON BOTTLENOSE DOLPHIN STOCK STRUCTURE, ABUNDANCE, AND MORTALITY WERE LIMITED. IN 1997, THE LACK OF DATA MADE DISCUSSION OF SOLUTIONS TO REDUCE BYCATCH DIFFICULT, AND THEREFORE NMFS DELAYED ADDRESSING BOTTLENOSE DOLPHIN ISSUES UNTIL BETTER DATA WERE AVAILABLE.

NOV. 2000 NMFS PRESENTED INITIAL INFORMATION ON BOTTLENOSE DOLPHIN SCIENCE AT THE ANNUAL MEETING OF THE ATLANTIC MARINE MAMMAL SCIENTIFIC REVIEW GROUP AND RECEIVED RECOMMENDATIONS FOR MODIFYING THE ANALYSIS.

APRIL 2001 INTERIM REVIEW OF BOTTLENOSE DOLPHIN SCIENCE BY THE ATLANTIC MARINE MAMMAL SCIENTIFIC REVIEW GROUP.

MAY 2001 NMFS SPONSORED THE FIRST OF TWO WORKSHOPS WITH THE PUBLIC TO SHARE DATA AND OTHER INFORMATION RELATIVE TO BOTTLENOSE DOLPHIN INTERACTIONS WITH COMMERCIAL FISHERIES. PRESENTATIONS FOCUSED ON STOCK STRUCTURE, ABUNDANCE, AND DISTRIBUTION OF BOTTLENOSE DOLPHINS.

JULY 2001 NMFS SPONSORED THE SECOND OF TWO WORKSHOPS WITH THE PUBLIC TO SHARE DATA AND OTHER INFORMATION RELATIVE TO BOTTLENOSE DOLPHIN INTERACTIONS WITH COMMERCIAL FISHERIES. PRESENTATIONS FOCUSED ON THE BIOLOGY OF BOTTLENOSE DOLPHINS, MORTALITY, BOTTLENOSE DOLPHIN STRANDINGS, AND FISHERIES WITH A HISTORY OF INTERACTIONS WITH BOTTLENOSE DOLPHINS.

OCT 2001 FORMAL REVIEW OF BOTTLENOSE DOLPHIN SCIENCE BY THE ATLANTIC MARINE MAMMAL SCIENTIFIC REVIEW GROUP.

NOV 2001 FIRST MEETING OF THE BOTTLENOSE DOLPHIN TAKE REDUCTION TEAM

JAN 2002 SECOND MEETING OF THE BOTTLENOSE DOLPHIN TAKE REDUCTION TEAM

FEB 2002 THIRD MEETING OF THE BOTTLENOSE DOLPHIN TAKE REDUCTION TEAM
MAR 2002 FOURTH MEETING OF THE BOTTLENOSE DOLPHIN TAKE REDUCTION TEAM

APRIL 2002 FIFTH AND FINAL MEETINGS OF THE BOTTLENOSE DOLPHIN TAKE REDUCTION TEAM

MAY 2002 FINAL REPORT WITH RECOMMENDATIONS FOR REDUCING BOTTLENOSE DOLPHIN BYCATCH DUE FROM THE BOTTLENOSE DOLPHIN TAKE REDUCTION TEAM TO NMFS.

SUMMER 2002 NMFS PUBLISHES PROPOSED REGULATIONS IMPLEMENTING THE BOTTLENOSE DOLPHIN TAKE REDUCTION PLAN AND SOLICITS PUBLIC COMMENT ON THE PROPOSED RULE.

WINTER 2003 NMFS PUBLISHES FINAL REGULATIONS IMPLEMENTING THE BOTTLENOSE DOLPHIN TAKE REDUCTION PLAN.