Review of the Scientific program and information relevant to the International Dolphin Conservation Program Act (IDCPA) held at the Southwest Fisheries Science Centre, La Jolla, California, 8 to 11 March, 1999.

Stephen J. Smith
Head, Molluscan Fisheries Section
Invertebrate Fisheries Division
Department of Fisheries and Oceans
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
Canada

11 March 1999

The terms of reference for this review which operated under the University of Miami's Centre of Independent Experts were as follows:

1. To evaluate if the mail reviews of the scientific program were appropriately taken into account (i.e., comments incorporated or actually rebutted), and

2. To evaluate if the final conclusions in the draft report to congress were supported by the research to date.

Given that my expertise mainly concerns stock assessment models and sampling theory, I will concentrate on those aspects of the program. The other two members of the review team will deal with the habitat and stress issues.

1. Mail Reviews

Reviews by Marine Mammal Commission (MMC)

Comments by the MMC were contained in two letters dated 8 January and 12 February 1999. In the first letter, the following points were raised concerning the stock assessment model or the research survey.

1. What value would be used for the estimated maximum growth rate (r_{max})? Letter noted that values of 3 and 4 percent were discussed by NMFS but no consensus reached.

NMFS responded by stating that the values used in the analyses of 1.5 and 1.7 percent for northeastern spotted and eastern spinner dolphin populations, respectively, were based on calculations from the data for 1975 to 1991. While, the idea of incorporating a default value of 3 or 4 percent would be worth investigating, the current approach of using an estimated value will be retained.

2. NMFS should consider publishing the details of the proposed decision analysis framework.

Document giving details of the framework prepared by Prof. D. Goodman (Montana State) and sent to MMC (received 1 February, 1999; see section on letter from MMC dated 12 February below).

3. Due to the lack of historical information on coastal spotted dolphin, NMFS should consider an alternative approach be used for making a determination for this stock. It was further recommended that NMFS obtain and review information on the frequency at which sets are made on this stock and the numbers of coastal spotted dolphins that are chased and encircled relative to the estimated stock size.

NMFS responded that while this proposal has merit there are difficulties in obtaining or interpreting some of this information. They proposed that at least an estimate of the average annual frequency of sets on spotted dolphins within the coastal stock's range would be provided for the 2002 finding.

The comments contained in the letter of 12 February mainly concerned editorial changes to the three background documents detailing methods and results for the 1998 research survey, the literature review of the potential influence of fisheries induced stress and an preliminary investigation of dolphin habitat variability. In addition, receipt of a document on the decision analysis framework was acknowledged and the MMC reported that it was satisfied with the documentation and the framework itself.

The editorial changes suggested for the document on the 1998 research survey were all taken in account in the revised version dated 26 February 1999.

I conclude that all of the MMC's comments were either incorporated or objectively rebutted by NMFS staff.

Reviews by the external referees

Technical comments on the line transect methodology were submitted via email by Dr. K. Burnham, Dr. S. Buckland and Dr. Jeff Laake. All of these individuals have participated in a large way in developing the methodology and software for the line transect methods used to estimate abundance from the research survey and the kill estimates and population indices from the tuna vessel observer data (TVOD). Given their involvement, their reviews can hardly be construed as independent outside reviews. Overall, their comments mainly dealt with matters that could provide for future improvements to the surveys and the methods. While the comments were pertinent to the line transect methodology and the reviewers are experts in the field, future reviews should at least include outside experts in survey sampling design.

Reviews by the Inter-American Tropical Tuna Commission (IATTC)

Three letters dated 14 January, 3 February and 17 February from the IATTC to NMFS commenting on the scientific program were provided to the reviewers prior to the week of 8-11 March.

14 January 1999:

1. Concerns about the decision framework and the interpretation of the TVOD index.

3 February 1999:

1. Comments on habitat variability and stress paper.

These will be handled by Drs. Olsen and St. Aubin.

17 February 1999:

1. Reiterated comments from 14 January on problems with the decision framework in response to NMFS response to the original comments by the IATTC.
2. IATTC contends that TVOD index does show that population growth rate of northeastern offshore spotted dolphin has been between 2 and 4 percent in the 1993-1998 period.
3. Problems of the comparison between TVOD and MOPS.
4. Comments on small CV's for the TVOD, especially in the most recent years..
5. Questioned usefulness of comparing population growth rates between the two periods (pre and post 1991).
6. Questioned designation of reduction in population growth rate as being due to unreported additional mortality rather than change in carrying capacity or some other answer.
7. Refers to problems observed in a "strategic" analysis currently being done by IATTC (not provided) of TVOD data.

The main issues raised by the IATTC in their letters of 14 January and 17 February concerned the decision rule defined by NMFS and the interpretation of the TVOD data. Overall, the NMFS response was to try to agree to disagree on the decision rule and to point out that the IATTC appeared to use the TVOD as a relative index of abundance so why couldn't NMFS. I discuss both these items in more detail below when discussing the model under the second term of reference. There being no middle ground on these points, it can be concluded that for the most part NMFS stuck to its point of view when drafting the Report to Congress. It was difficult to do otherwise because there was no published or widely disseminated material concerning the problems with the TVOD raised by the IATTC.

A fourth letter from the IATTC to NMFS dated 5 March was submitted to the reviewers on 8 March despite our intent to limit submissions to what could be received by the reviewers as of 2 March so that adequate time would be available to understand and evaluate the contents. NMFS staff did not have time to reply to the 5 March letter and as a result no evaluation of response on my part is possible.

2. Conclusions of Draft Report

The finding of the National Marine Fisheries Service is there is evidence that in the period since 1991, there has been for the northeastern spotted and eastern spinner dolphin populations a failure to grow at the expected rate (designated as r_{max}) of 1.5 and 1.7 percent, respectively.

The population model developed to evaluate the current status of the dolphin stocks is a Leslie matrix population model in which parameters are estimated using empirical Bayesian methods. The parameter estimates themselves are obtained as means or medians from the posterior distributions for the parameters with these distributions being generated from the input data (TVOD, research survey, TVOD estimates of dolphin kills by tuna fishing, Leslie Matrix structure with respect to age composition, selectivity, survival and fecundity) and prior distributions for the parameters of interest using a computer-intensive resampling approach (SIR method). While this model is complex in structure its behaviour with respect to the main data sets used in it can be characterised as one where the TVOD data generally determine the trend in the population estimates while the research survey generally determine the scale (parameter *a*). That is, the research survey data contribute more toward determining the absolute population size of the dolphin populations than trends in population size over time. Trend and scale are modified somewhat by the age composition, selectivity and assumptions in the model concerning the fecundity and age of sexual maturity. However, the main signals for the population model come from the TVOD and research survey data.

Major issues of the model:

Research survey data:

The research survey data used in the population model come from three separate programs: 1) Research vessel surveys, 1979–80, 1982–83; 2) Monitoring of porpoise stocks (MOPS), 1986–1990; 3) Stenella population abundance monitoring (SPAM), 1998 [first of three mandated surveys]. All of the survey data have been analyzed in a similar way for inclusion in the model. The general trends over the three series are highly variable for the northeastern spotted and eastern spinner dolphin populations. Both series exhibit large increases/decreases in population size that would appear to be biologically unrealistic given the expected growth rates for these populations. Additionally, all of the abundance estimates from these surveys have large coefficients of variation (CV) with most being in the range of 22 to 46 percent and 34 to 76 percent for northeastern spotted and eastern spinner dolphin populations, respectively. The 1983 estimates for both species and the 1980 estimate for the eastern spinner dolphin have very large CV's ranging from 161 to 359 percent.

As has been already noted by the reviewers and the authors of the reports, the larger CV's associated with the research survey compared with the TVOD data results in the survey data contributing more to scaling the population size estimates than in estimating the trend of the population. The only way that the research survey estimates will contribute more to the trend aspect of the model is have much lower CV's on the 1999 and 2000 surveys relative to the TVOD. While the CV's for the 1998 estimates are in the lower range of those observed for all three surveys, the 1998 levels of precision were obtained using the total sampling effort of three vessels. It appears that only two vessels will be available for 1999 and 2000 which suggests that the CV's for estimates in those years may be expected to be higher than those for 1998. Therefore, if the CV's of the estimates from the next two year's surveys end up to be no lower than previous surveys then it is unlikely that these survey estimates will in any substantial way influence the trend predicted by the model.

If sampling effort can not be increased, then it may be possible to evaluate the current survey design to see if its efficiency can be improved upon by changes in the allocation or stratification scheme. In sampling theory, stratified random survey designs result in increased precision (lower CV's) for estimates over simple random sampling design only if the design of the stratification scheme and the scheme for allocating sampling effort (days) to strata is optimal. The theory for evaluating the efficiency of a stratified random design is available for estimating means (see Smith and Gavaris 1993 for a fisheries example). The method partitions the difference between the variance for simple random sampling and the variance for a stratified random design into two components — allocation and stratification. The allocation component can be equal to zero, greater than zero or less than zero depending upon whether the number of sampling days were allocated to strata proportional to the area of the strata, proportional to the product of the strata area and strata standard deviation, or in an arbitrary manner. The strata component will always be greater than zero and its magnitude will reflect whether the within strata variances were larger than the between strata variances. If the sum of the strata and allocation terms is greater than zero then the current stratified design has resulted in a gain in precision. It is entirely possible that the strata may be meaningful but that the allocation of sets to strata is so suboptimal with respect to strata variance that the allocation term is large and less than zero resulting in a difference between the simple random sampling variance and the stratified variance that is also less than zero. This implies that the stratified design resulted in an estimate with lower precision (larger CV's) than simple random sampling. Given that allocation schemes are generally under the control of the survey program staff, changing this scheme to be optimal in some manner (as discussed above) may result in a gain of precision without additional cost. If the conditions with respect strata variability are fairly constant over time then it may be possible to use the 1998 survey to redesign the allocation scheme for the 1999 survey.

Often however, the distribution of animals is rarely constant from year to year and last year's survey may be a poor predictor of conditions in the current year. In that case, the method of adaptive allocation may more useful (Thompson and Seber 1996). In this method the total number of days for the survey d is partitioned into two groups d_1 and d_2, where d_1 is usually much larger than d_2. In the first phase of the survey d_1 days are allocated to strata either in proportion to strata area or strata variance from the previous year (if informative) and the survey is conducted. Prior to the first phase a sampling rule is set which prescribes increased sampling in a stratum if it's variance (or mean or CV, etc.) is greater than some threshold level. After completion of the first phase the d_2 days are allocated to those strata for which the threshold level was exceeded in the second phase. Thompson and Seber (1996) provide the appropriate equations to provide unbiased estimates for this kind of two phase sampling.

The situation for the line transect surveys is more complicated than the usual situation of estimating a mean. As a result explicit formulae for assessing the efficiency of the stratified design for line transect surveys do not exist. However, given the number of days observed in the 1998 survey it should be possible to assess the efficiency using sampling/Monte Carlo methods. In addition, the unbiased estimators given in Thompson and Seber (1996) may not be strictly applicable to the line transect situation, although this fact has yet to be determined. The bottom line here is that methods do exist for evaluating and improving the precision of estimates from stratified random designs in the standard situations and modifications could be possible to do the same for the line transect surveys. These approaches offer the means to improve the precision (and hence decrease the CV) for the future research surveys at no extra cost which in turn may mean that these surveys contribute more trend information to the population model than the surveys currently do.

The variances (and hence CV's) for the line transect survey are estimated using the bootstrap resampling method. Recent research in the statistical literature (Rao and Wu 1988, Sitter 1992a,b) has indicated that the application of the standard bootstrap method to situations involving complex survey designs may result in under-estimates of the true variability. Modifications have been provided by the authors cited above and these should be investigated for the line-transect survey data (see Smith 1997 for a fisheries application). These modifications may also be appropriate for the TVOD indices which also use the bootstrap to calculate variances.

TVOD data:

One of the most contentious issues raised by IATTC in its review of the population model is the use of the TVOD data as an abundance index for the dolphin populations. Note that the use of the TVOD estimates of dolphin kills in the model does not appear to be disputed by anyone.

In the most recent IATTC annual report (Anon. 1998) the TVOD indices (smoothed and including error estimates) for the northeastern stock of offshore spotted dolphins and the eastern spinner dolphins are presented as abundance indices in Figures 84 and 87, respectively. The text associated with these figures (pages 79–80) admit that there may be biases in the data and suggest "... so the resulting estimates should be treated as indices of relative abundance of the stocks, rather than estimates of their absolute abundance." The annual report uses these relative indices to comment on population trends for the various dolphin stocks affected by the tuna fishery. The NMFS population model uses the TVOD indices as relative abundance with the conversion to absolute abundance done via the parameter a and contributions from the research data (see above). Appendix 1 of the draft Report to Congress contains a review of the literature mainly contributed by IATTC scientists on modifications of the TVOD data so that they may be used to construct relative abundance indices.

It appears therefore that the reservations expressed by the IATTC are fairly recent and yet to be peer reviewed and published. Are the concerns of the IATTC valid? It is difficult to answer this question without a thorough analysis of the data but there appear to be a number of issues that demand some further study and explanation. The IATTC notes that the TVOD index and the total number of dolphin sets appear to track each other since at least 1981 (comparison provided by IATTC as Appendix D in their letter of 5 March, 1999). Information on trends in the number of dolphin sets is also provided from 1986 to 1996 in Figure 83 of the annual report (Anon. 1998). It should also be pointed out that the reviewers noted that a rough comparison of the TVOD with the searching model and cohort indices of abundance for yellowfin tuna (Fig. 31, Anon. 1998) appears to show coincident trends as well. One explanation for these coincident trends could be that the TVOD is simply reflecting encounter rates with dolphins but a few trends compared by eyeball hardly constitutes a definitive analysis and a more rigourous study should be made of these patterns.

The IATTC contends that their has been a change in the pattern of the data used to estimate the detection function arguing that recent changes in the addition of helicopters and bird radar may result in fewer observations near the trackline than there should be. It is interesting to note that the variability in numbers of dolphin sightings near the trackline noted for the TVOD is also evident in the 1998 survey data (Figure 5, Gerrodette document on Preliminary estimates of 1998 abundance ..., dated 26 February, 1999). The interpretation of abundance indices derived from commercial fisheries data is often complicated by technological changes to the actual process of fishing and the TVOD is probably no different in this regard. While an analysis of the TVOD to determine if technological changes to the fishing process have affected the indices is certainly warranted, the current explanation for the patterns in the data used to calculate the detection function needs more work.

The TVOD indices are derived from commercial fishing data through a process that by its nature lacks the control that one may have over a research survey. On the other hand, the TVOD has the advantage of obtaining more data in time and space than a research survey allowing it to possibly provide a more complete picture of the dynamics of the fishery and the dolphin populations. As noted above, commercial fishery based abundance indices can be influenced by changes in fishing practice and attention must be given to constantly monitoring the fishery for these changes in order to modify the indices appropriately. The use of the TVOD by the NMFS to estimate the growth rate of the dolphin populations seems to have brought the issue of evaluating the usefulness of the TVOD to a head. However, for the moment we do not have any precise answers instead we have a number of analyses that need to be done.

Definition of μ:

Prior to 1992 all fisheries-based mortality of the dolphin populations was accounted for in the population model by the reported kills, M_i from the TVOD estimates in year i. However, from 1992 to 1998 an additional parameter μ was introduced to account for additional mortality above and beyond that represented by the M_i, i.e., total fisheries-based mortality was equal set to $M_i+\mu N_i$, where N_i is the estimated population size in year i. The quantity μ (along with the estimate of $r_{\max}$) is used in the decision rule to assess whether there has been a failure for the population to grow at the expected rate.

This definition of additional mortality may be problematic especially if the TVOD shows an increase in dolphin abundance over the next three years. The parameter μ is defined as a constant rate of mortality over the years 1992 to 1998. In the case of the northeastern offshore spotted dolphin (Figure 2, Draft Report to Congress), 1992 was a high year for the TVOD while the index was lower and flat (no trend) from 1993 to 1998. While all of the years from 1975 to 1991 were used in the population model to estimate the population abundance in 1992, μ may be seen as being driven by one high point and five low points. If the TVOD shows an increase at a rate that may be biologically plausible over the next three years (before the 2002 finding), the five low points may still be influential enough to make the estimated μ relatively insensitive to the growth rate as indicated by the trend in the more recent years. The parameter μ may be much less sensitive if the TVOD increases by a relatively large amount in the next two years but these changes would be questioned because their apparent rate of change exceeds that expected from the dynamics of this population.

Interpretation of μ also appears to be problematic in that it can represent either unseen mortality perhaps defined in terms of deaths which occur some time after the dolphin is caught and then released from the purse seine or retardation of reproduction through either stress-related abortions of fetuses or through some stress-related inhibition of reproduction after exposure to fishing or any combination of these. Given that this mortality or decrease in reproductive output is a function of exposure to fishing activity it is difficult to understand why NMFS chose to define μ as a constant proportion of the dolphin population size rather than some function of either fishing effort or numbers of dolphins recorded as killed by fishing. As it is presently defined it is possible that μ could be estimating some level of mortality even if M_i is equal to zero. The way the model is currently constructed a value of zero for M_i could indicate that no dolphins were killed in dolphin sets or alternatively, no dolphin sets were made but dolphin sightings were still being reported by observers on tuna vessels. The utility of μ appears to be limited as currently defined and an alternative approach should be developed certainly prior to the 2002 finding.

We were provided with a fit to the data which used an exponential model for the series with a break at 1992 and no μ parameter. Indications that there had been a decrease in growth rate still remained, so μ as defined does not appear to be driving the decision but as stated above this parameter has serious limitations in its own right.

Robust to age structure:

The reviewers expressed an interest in knowing if assumptions concerning age structure and the age-specific selectivity curve affected the final results of the model with respect to detecting a failure of the dolphin populations to grow at the expected rate. Paul Wade provided us with a run of his model which included an identical selectivity for all ages and the results were similar to the run provided in the draft Report to Congress — there was still evidence for a depression in the growth rate according to the original criteria chosen. In an analysis for the same data but without age structure Wade (in press) found evidence that the dolphin population was depleted — note that the parameterization to determine depletion was different than that used for the current report but the conclusions about the status of the stock were similar.

Decision analysis Framework:

Arguments forwarded by the IATTC that, while the decision criteria proposed (and used) may be appropriate for resource management issues, they are not appropriate for the question of product labeling, are of more legal interest than of statistical concern. I do not have the expertise to offer opinion on legal manners. However, the decision theoretic framework as a concept appears to be a sensible way of approaching this problem and the only way to deal with decision making when using a Bayesian model. The idea of a decision tree to incorporate a broad range of questions concerning possible sources for the mortality is only outlined in a cursory manner in the supporting documents. I assume that this idea will develop over time but there is not enough information to comment on at this point in time.

In conclusion then, assuming that the TVOD indices tracks dolphin population abundance and the research surveys currently index the range of population size, the population model based on the Leslie matrix with the Bayesian statistical model appears to be a useful model for evaluating growth rate changes. The use of a Bayesian model to estimate parameters leads to using a decision theoretic approach to make inferences. The main model-based weakness noted here is the definition and limitations of the parameter μ for determining if there has been a depression in the growth rate. As noted above different parametrizations for the model of the northeastern offshore spotted dolphin used in Wade (in press) and an exponential model presented to the reviewers indicated that indications are that the stocks are still in a depressed state. The research surveys are extremely variable and suggestions have been given here for ways of evaluating and improving the current design for the next two years. While there appears to be indications that the TVOD indices may not be tracking the dolphin populations in a consistent manner over the whole time series, more investigation needs to be done to establish the magnitude of the problem.

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Review of the National Marine Fisheries Service Report to Congress on 1999 Findings in Response to the International Dolphin Conservation Program Act of 1997

By

Donald B. Olson
Professor Meteorology and Physical Oceanography
University of Miami

Introduction

This review encompasses the 25 February 1999 Draft of the NMFS report,the supporting documents on the programs, review letters solicited by NMFS on the components, and letters of review from the Marine Mammal Commission and Inter-American Tropical Tuna Commission (IATTC). It also involves perspectives gained from presentations to the review committee at meetings held in La Jolla on March 8-11, 1999. This review will begin with an overview of the effort from my perspective. A detailed review of the areas relevant to my expertise; physical oceanographic setting and variations, aspects on the analysis of the dolphin habitat in the Eastern Tropical Pacific, the sampling programs for assessing dolphin abundance, and the population modeling used to arrive at a finding. The portions of the program dealing with stress and cryptic mortality will only be commented on in passing in relationship to the modeling. The technical comments on these issues are preceded by a comment on the structure of the document.

Structure of the Document

The sections in the report seems out of order. In the Act calling for the research there are two points, 1) Abundance and 2) Stress on dolphin. The report starts with abundance then switches to the stress studies. It then returns to the abundance question with an analysis of habitat and finally a model estimate of population trends. It seems logical to move the stress section to the end of the model discussion. This allows the abundance issues to be discussed in sequence. The section on the decision framework adds an interesting approach to the IDCP mandate. The contents should be reorganized to put some of the decision section at appropriate places in the text. A smaller section on the details of the decision work should then come between the model and stress sections (see comments on the decision section below).

The Eastern Tropical Ecosystem

The report attacks the problems posed by Congress in a logically and straightforward manner. The details of the region environment are found in two Primary Research Documents prepared by Drs. Fiedler and Gerrodette covering the habitat variability in the ETP and the 1998 dolphin abundance estimates respectively. The reviewer also received a set of reprints from these authors and others discussing the background behind the work. Here these two elements of the report and science background will be reviewed together. This is because they are fundamentally linked. That is the habitat analysis depends on the abundance surveys and a set of physical data sets. The finding concerning the habitat is crucial for the finding of a lack of evidence for habitat variations that might explain long term shifts in dolphin abundance. Finally, the abundance data are at the heart of the modeling behind the finding here and the future one in 2002. The section 5.4 on the older surveys should also be moved up into Section 3.0 as discussed above.

To begin, this work is of high quality and has been done with a careful choice of sampling and analysis tools. In general, the finding relative to the physical environment is valid within the scope of the data, i.e. there is no evidence that there has been any major change in the habitat regime in the ETP that would suggest a decline in dolphin stocks. However, there are some worries about the data in relationship to abundance over time. This concern also impacts the model results because of its use of this data.

The difficulty involves the large variations in the absolute abundance estimates (App. 2, Figs. 1,2). The interannual swings in the abundance estimates can not be biological as pointed out by the authors in their published work and in App. 1. This is not discussed in the main text. Neither do the figures supplied in the text give the reader much of an appreciation of the abundances in the context of the ETP environment. The following suggestions are made:

- Change the title of section 3.0 to just reflect abundance estimates in general, i.e. the 1998 survey, MOPS and the earlier survey Work, and the Tuna Commission's TVOD indices.

- Include an opening discussion of the MOP and 1998 results and the variations in absolute abundance estimates. Describe the possible reasons for the interannual variations in the estimates. This should carefully define the area used to designate the dolphin stocks. Spotted dolphin, for example, are Trans-Pacific so the stock definition is highly relevant here. This section should include a plot of the absolute abundances over time without the model and TVOD data. In relationship to comments concerning habitat below it would be appropriate to plot the abundance data on the same plot as the dolphin habitat availability (Fig. 5, Fielder white paper).

- Explain the possible reasons for the variations in the estimates, i.e. migration or changes in dolphin availability to the survey. Make it clear here that the interannual changes can not be due to local biological factors in the population such as birth and mortality.

- Point out the importance of carrying out further surveys in order to further clarify the distributions of dolphin abundance and identify possible linkages between variation in the estimates and the ETP habitat (see below).

- Expand Fig. 1 or include another figure showing the survey results. Figs. 2 and 3 from Gerrodette's white paper would be appropriate. This shows the most extensive accomplishment in coming up with the March 1999 findings.

The sections on the TVOD data on mortality and the accompanying index of abundance can be moved up to Section 3.0 and put in a paragraph starting out "Other relevant data on ..." This eliminates the structure in the original draft with its "primary" and "secondary" portions. Also, in an effort to at least acknowledge the concerns of the Tuna Commission, it might be wise to mention some of their problems with the TVOD data. These involve changes in the properties of this data set over time that might reflect variations in the way the data were collected. The data also seems to have the index following the effort rather closely. The report should at least mention these concerns since problems in the TVOD data will significantly impact the model used in the report.

The fisheries derived TVOD data are a major contributor to the results in the model in the report. The TVOD data are the only data set that has the scope to provide a long-term trend in ETP dolphin stocks. The inclusion of the TVOD in the report has created a high degree of angst in the Tuna Commission. They point out the index has changed markedly with the change in fishing practices over the last decade. In particular they suggest that there was a shift in the TVOD after the 1992 La Jolla meeting that lead to differences in how observers worked. This is made worst by the increased use of bird radars that allow the larger boats to spot possible schools and decide to steer towards them by helicopter operations at ranges of up to 20 nm. The timing of this corresponds to a drop in the TVOD indices from 1992 to 1993. Curious also is the fact that the TVOD remains almost constant and has a much reduced variance after 1992. The Commissions search time index for yellowfin tuna, that agrees with their cohort analysis, and the TVOD are also tracking effort. This is typically worrisome in fishery data. In conclusion, the report needs to document the concerns of the Commission concerning the TVOD and its use.

At the end of the abundance section there needs to be a discussion that provides an overall assessment of the problem with providing accurate abundances with which to make a decision. In general, here and elsewhere it is important to make Congress aware of where uncertainties lie and how the effort underway is trying to address these unknowns. This is the place to stress the role of the surveys in providing a better picture of ETP dolphin stocks.

The comments above carry over to the habitat analysis (Sect. 5.3). The reviewer agrees with the validity of the finding at the end of page 10. It would be appropriate to again provide some illustrations depicting the habitat structure in the ETP and its variability. A suggestion for a figure showing abundance and an environmental index is made above(Fig 1). The second paragraph of this section should be revised and expanded slightly. The suggestion is to incorporate Fig. 4b from the Fiedler white paper. The patterns in the spinner dolphin habitat can then be contrasted with the signals in abundance in the previously suggested figure. The reviewer would here speculate that the figures show a coherent pattern in habitat and abundance with low abundance at El Nino peaks and high abundance during La Nina. The MOPS 1988 and SPAM 1998 show a compression of the habitat suitability into the coast of Central America during La Nina phase in at least these two incidences. While one resolution of an ENSO cycle and a few scattered other points are too little data to produce a significant correlation, this suggests that a longer data set may come up with a significant spatial/temporal set of patterns. Understanding these patterns might assist in reducing the high variance within Surveys and large shifts between surveys.

INSERT FIGURE HERE

Fig. 1 Dolphin habitat availability (H) as derived by Fiedler (white paper) and The absolute abundance estimates for offshore spotted and eastern spinner dolphin. The thin lines for H denote monthly values while the thick line is a 13 month runing mean.

The Decision Analysis

The decision framework laid out in the report is interesting and at least to this reviewer novel. If not allowed to become a pedagogical crutch this type of careful attention to the questions one wants to answer with an analysis is

Dolphin Habitat Availability

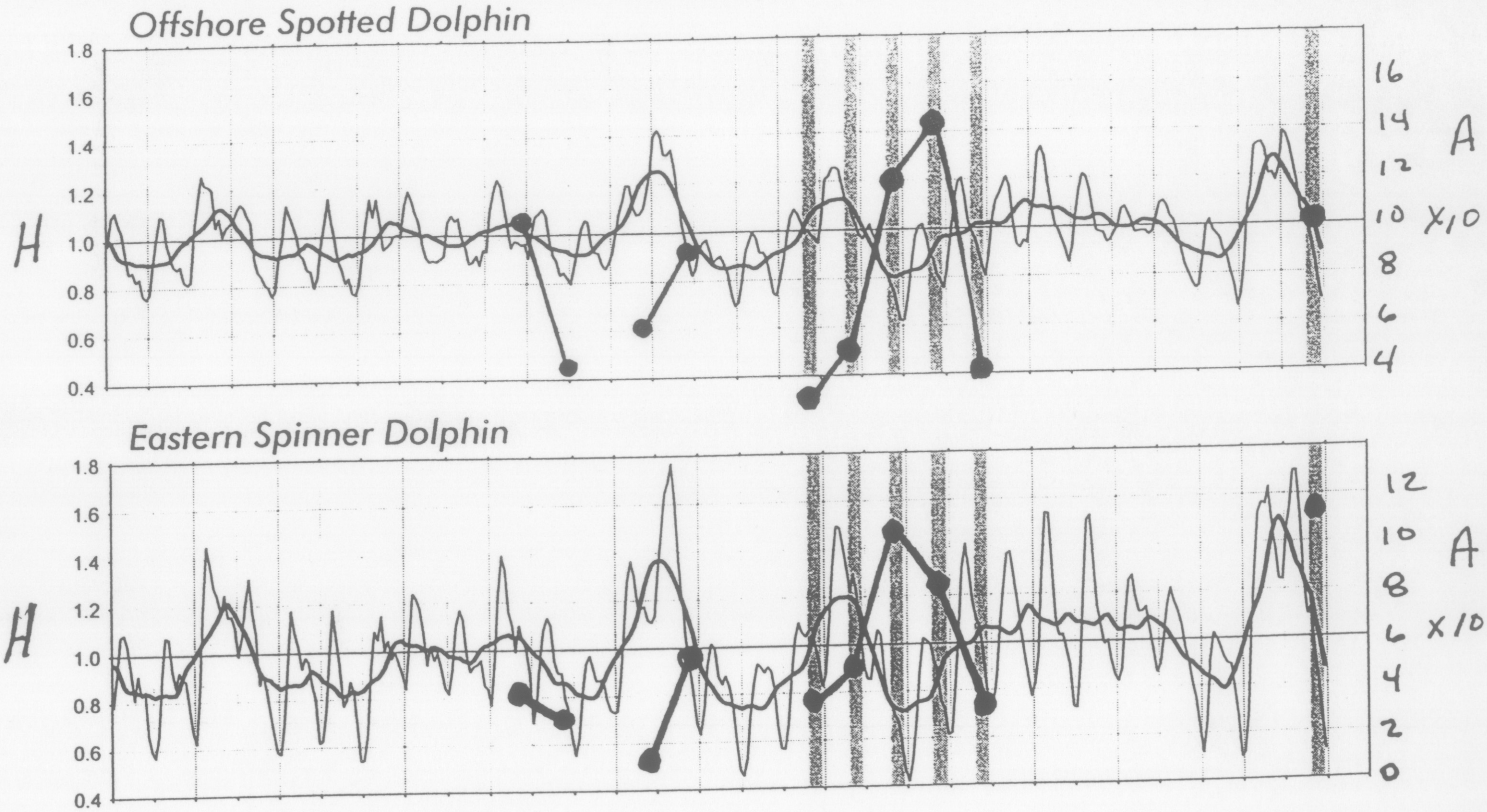

Fig. 1 Dolphin habitat availability (H) as derived by Fiedler (white paper) and The absolute abundance estimates for offshore spotted and eastern spinner dolphin. The thin lines for H denote monthly values while the thick line is a 13 month runing mean.

worthwhile. Some of the early part of Section 6.0 is very introductory to all of the other portions. To sandwich it in between the abundance and habitat material and the stress issues seem out of place. Placing it between the former materials and the model analysis also awkward. The suggestion would be to consider placing some of it prior to the abundance section as part of the discussion of initial strategy. The discussion in 6.1.4, however, suffers from the difficulty that it makes reference to the model and introduces variables that are not defined until the model section. See for example, the introduction of Rmax on the top of page 14. The final criterion in the middle of page 14 is hard to understand. The choice of 1% probability in criterion 1) is consistent with a 99% confidence that the stock will not go extinct. The second to choices, however, are not as obvious. The discussion at the end of these criterion are impossible to follow without a better history of the tuna/dolphin controversies. The Panama agreement and the PBR criteria need to be put in context somewhere in the document. In conclusion the decision analysis needs to be more carefully introduced such that it provides a rationale for the abundance and habitat sections and does not prematurely introduce model variables that have not been discussed yet. These model application portions might be placed at the end of the model section.

The Population Assessment Model.

The population model and the set of estimations made with it are interesting from both a theoretical and an application point of view. Dr. Wade and the others involved with producing it should be highly commended. The model is a Leslie matrix formulation with added mortality outside of the matrix demography. The report says it is the same as Wade (1994), but it applies the fisheries mortality differently. The reviewer would have chosen the 1994 implementation at first look. For the nonmodeler, the Leslie model is a discrete, i.e. one step per year, model that marches a population through age classes with some survival rate between classes and a reproduction term that allows mature animals to repopulate the zero year class. As discussed in detail by Caswell (1989) they are an approximation whose success depends on the time scales of generations relative to the class steps and the manner in which survival and in this case mortality are handled relative to reproduction. Given the long lives and slow growth of the two dolphin populations of interest the model is a natural first choice. Likewise the Bayesian methods used for projection of the model onto the data is one of several state of the art choices. The reviewer can not comment on the latter's implementation other that to say that sensitivity runs that the committee asked Dr. Wade to run during the review suggest that it is reasonably consistent. Clearly the population model is as complicated as one can expect to apply to the data. The techniques to apply it to the data are appropriate. There are several questions to pose, 1) are the assumptions made in the formulation of the model robust versus other choices, and 2) are the data inputs sufficient to allow the model to really produce a result that is usable in the decision framework? The third is the choice of variables to apply to the decision criterion in the report.

To address the first question, there are other approaches that should at least be explored. The model assumes it is following a single stock without migration and that there is no spatial structure. Under the Sustainable Fisheries Act the mandate is to manage fisheries from an environmental perspective. The habitat work of Fiedler's and the spatial surveys suggest that there should be an effort to explore these issues. As discussed above in relation to the abundance and derived habitat analysis there are other factors that need to be addressed. While the current model is age structured it does not take into account the well documented sex bias in the fishery deaths that occurred in the past. For species that take order of 10 years or longer to reach sexual maturity and experience a bias in fishery death to females this issue is still important in the 1991-98 analysis since the first cohort of females without the heavy fishing mortality is just now beginning to reach maturity. As pointed out in the report the maturation of these cohorts may provide a recognizable signal in the upcoming surveys. The investigators should try to model the possible manifestation of the expected increase in population growth potential. Finally, it is probably wise that the effort branch out to include some additional modeling approaches. In particular a spatially explicit set of models to follow up on Fiedler's work and try to understand the large variances that occur in the survey data are suggested. In particular, the issue of migration of the spotted dolphin stocks should be considered.

The next issue is the quality of the data sets for inclusion in the model. The scatter in the data on the plots in the report suggest caution should be taken in trying to use these data. The patterns in the data suggest that there are other ways of using them. The review team requested statistics from the Tuna Commission on effort and catch by set type. For example the 1986-88 change in absolute abundance in the survey data corresponds to patterns in the distribution of effort in the tuna fleet. The patterns, therefore, in both the commercial data and the research surveys suggest that there are significant patterns in the ETP habitat that are responsible for the locations of the dolphin populations in different years (Reilly, 1990; Reilly and Fiedler, 1993, Fiedler, white paper). Similar patterns occur in the fleet effort. In 1986 the heavy effort by class 6 boats extended out to 130 W. This western area of effort diminishes in 1987 and disappears completely 1n 1988. This same period corresponds to the increase in the absolute abundance estimates and to the habitat changes relative to the shift from El Niño to La Nina conditions. It is the reviewers contention that the variances can be reduce substantially if the spatial patterns are taken into consideration; i.e. understanding the spatial patterns and their relationship to the ETP environment should allow a model effort that is not plagued by high variances. The investigators are already well on their way to quantifying the spatial patterns, the next job is to take them into account in the modeling. The final point with the model is the choice of Rmax and mu as the decision variables. The reviewer struggled with both of these. The Rmax is derived from the characteristic equation from the Leslie matrix and is related to the first eigenvalue (Caswell, 1989; Wade 1994). It is a measure of the population entering each fecund age class and their fecundity. In the very crudest approximations (two age classes, immature and mature) Rmax is the natural log of the survivorship of the immature times the fecundity. The additional mortality, mu, is diagnosed as an additive term to the fisheries mortality. It seems to the reviewer that any errors in the data used to start the model off in 1991 becomes part of mu. The introduction of mu to the model is clever, but it is important to understand its sensitivity. For the decision process the ratio of Rmax to mu is considered. This bothers the reviewer from the perspective of units alone. With a week to look at the model, it is possible that this impression is incorrect. Overall, the model is an excellent piece of work and a good start on the route to a final statement in 2002, but the report should reflect some reservations about its findings at this stage. The model description and conclusions should stand where they are, but enough caution should be conveyed here to allow an easy rebuttal given further modeling and data for the 2002 report.

Review of NMFS Initial Finding
Prepared for the
Independent Center of Experts

David J. St. Aubin, Ph.D.
Director of Research and Veterinary Services
Mystic Aquarium, Mystic CT

March 11, 1999

As directed by the International Dolphin Conservation Program Act (IDCPA, 1997), the Southwest Fisheries Science Center of the National Marine Fisheries Service (NMFS) has undertaken a program of research to address the question of whether intentional deployment on or encirclement of dolphins with purse seine nets is having a significant adverse impact on any depleted dolphin stock in the eastern tropical Pacific Ocean. The mandate specifies that an Initial Finding be submitted to Congress by March 31, 1999, and that the document(s) be subjected to peer review. This document constitutes a portion of that review and represents the views of the author based on examination of prepared materials, and presentations and interviews conducted March 8-11, 1999.

The investigation by NMFS has focused on the three dolphin stocks recognized as depleted under the Marine Mammal Protection Act (MMPA): the northeastern offshore spotted dolphin (*Stenella attenuata*), the eastern spinner dolphin (*S. longirostris*), and the coastal spotted dolphin (*S. attenuata graffmani*). The evidence concerning the depleted status of these stocks relative to historical levels is sound and the efforts by NMFS to examine population trends is appropriate.

NMFS has addressed its mandate in this issue by formulating a series of questions, as described in "Decision Framework for Assessing the Status of the Eastern Tropical Pacific Dolphin Stocks" prepared by D. Goodman. The phrasing of these questions, and their application in guiding the investigation prescribed by the IDCPA, is logical and appropriate. They clarify the objectives and specify criteria that would be used to make a determination of whether the growth rates of the populations in question were within acceptable limits. These threshold criteria for acceptable risks of extinction, exceeding potential biological removal (PBR), and delayed recovery, respectively, appear to be a sound basis for evaluating the information derived from stock assessments and population models.

To address the question of whether there has been a failure to recover in any of the identified stocks, NMFS has implemented abundance surveys from dedicated vessels in 1998. The findings were reviewed at a January 21, 1999, meeting and presented in the draft report titled "Preliminary Estimates of the 1998 Abundance of Four Dolphin Stocks in the Eastern Tropical Pacific" by

T. Gerrodette. A critique of the survey and analytical methodologies is beyond the background and expertise of this reviewer.

The next phase of the investigation, following the decision framework established in Goodman's report, was to model the populations to make the determination of rate of recovery (or decline). The approach used is presented in "Description of the Population Analysis" by P. Wade. Data were derived from various sources, including the abundance estimates determined by Gerrodette, fisheries mortality statistics, and the tuna vessel observer program. The finding was made that none of the stocks met the established criteria for acceptable risk relative to recovery rate. Aspects of the modeling approach, data selection and appropriateness, and robustness of the conclusions are addressed by other members of the review panel.

To explain the finding that the stocks in question are not growing at the expected rate, NMFS has considered two possible causes: environmental variability and unobserved mortality resulting from stress or injury associated with fishery activities. Examination of environmental conditions was presented in the report titled "Eastern Tropical Pacific Dolphin Habitat Variability" by P. Fielder. It was concluded that inter-annual variability during recent years has not been anomalous, and that shifts in dolphin distribution that might have biased abundance estimates probably have not occurred. The analytical methodology supporting this conclusion is addressed in more detail by other members of the review panel.

Stress Studies

My comments focus on efforts to address the other possible cause under consideration to account for the failure to grow at the expected rate. It has been hypothesized that encirclement during fishing activities is a stressor that produces insidious physiological changes that compromise dolphin health, fecundity, or fitness. NMFS was directed by the Act to review available literature on the stress response in mammals and to identify plausible mechanisms through which stress might impair the ability of the dolphin population to recover. The findings were presented in the report titled "Stress in Mammals: The Potential Influence of Fishery-Induced Stress on Dolphins in the Eastern Tropical Pacific Ocean" by B. Curry. The stated objective of the report was "… to provide a context for future scientific findings by describing what is known about physiological and behavioral responses to stress in mammals and relating that information to the chase and encirclement of dolphins in the ETP fishery."

The Abstract and Introduction indicate that four general areas of study were reviewed, and that these are outlined in Section 1 of the report. The categories are somewhat ambiguous and arbitrary. For instance, it is unclear what "biomedical laboratory research" means, and how it differs from "research on domestic animals." "Research on free-ranging mammal populations" would more appropriately be termed "research on free-ranging mammals", reflecting an emphasis on physiological consequences on the individual rather than population level effects which are inferred but not directly studied. Nevertheless, the literature reviewed is relevant to the mandated task.

To evaluate whether the review has accomplished its stated objective, I pose a series of questions which I would expect to see addressed in the document. This is done in part because the organization of the manuscript makes it difficult to examine in sequence the events and associated information base from the scientific literature. For example, consideration of the immediate physiological effects related to the chase (Section II.B.3) appears after a discussion of the effects of isolation and restraint following capture (Section II.B.2.b). The following represent, in my view, the questions fundamental to the issue.

Could any activities associated with tuna fishing be considered as potential stressors to dolphins?

The techniques used to encircle dolphins (and associated tuna) are clearly described, and compared with similar activities in terrestrial mammals. For example, the report notes that disturbance caused by helicopter overflights has documented effects on the behavior of bighorn sheep. Pursuit prior to kill produces measurable changes in blood constituents in red deer. Other examples are scattered throughout the review, and together are sufficient to support the conclusion that chase and confinement are recognized stressors in mammals. The review also appropriately considers psychosocial issues that might compound the stress of encirclement, namely crowding, separation, novelty, and isolation. Evidence that such conditions constitute stressors in other mammals is also adequately presented.

Some consideration is given to the possibility that habituation might occur in animals repeatedly encircled in the fishery. Captive dolphins can become accustomed to performing behaviors that allow blood collection (a potentially stressful procedure), yielding samples that are considered to reflect an unstressed state (St. Aubin *et al. 1996*). However, such behaviors typically require lengthy training and consistent positive reinforcement, conditions unlikely to be associated with encirclement in the wild. It is conceivable, however, that experienced dolphins might show a diminished stress response to repeated encounters with nets, such as is presumed to occur with regularly captured bottlenose dolphins in Sarasota Bay. It is not reasonable to expect that the stress response would be eliminated.

Do similar stressors produce physiological changes known as the stress response in other mammals?

Considerable information is presented to demonstrate how stressors comparable to those identified with tuna fishing operations can influence physiological systems in mammals. These physiological and endocrinological perturbations are generically termed the stress response, but as the review acknowledges, there are pitfalls associated with attempts to describe a common response to a wide variety of stressful stimuli. It has also proven difficult to demonstrate direct correlations between either the duration or intensity of a particular stressor and the measured physiological effect. Nevertheless, some gradation of response exists, but the review takes few opportunities to develop this point, often referring generically to stressors such as restraint, isolation, and electric shock with little information on the duration and intensity of the stressor. Such information would be useful to allow evaluation of what degree of stress is necessary to produce specific changes. For instance, it is recognized by veterinary practitioners that the duration of chase can influence the likelihood of developing capture myopathy. This point is particularly relevant to the pursuit of dolphins in the ETP.

The review should recognize apparently fundamental differences in an individual's response to a stressor that it can avoid compared with one that it cannot escape. This point might also be developed to include possible differences between physical restraint (which can be intensely stressful and physically traumatic) and confinement within a relatively broad space (a relatively mild stressor). With respect to the latter, one might expect that confinement of any form would be stressful to animals such as offshore dolphins habituated to an environment without boundary. Still, one might expect that the experience would be qualitatively different from physical constraints on body movements.

What is the nature of the stress response in other mammals?

The recognized physiological and biochemical features of the mammalian stress response are adequately presented. The report relies on a combination of primary works and important reviews. It was not the mission of this undertaking to resolve controversies within the literature regarding aspects of glucocorticoid physiology, for example. Sufficient recognition is given those points in which are apparently conflicting (e.g. lactation may be impaired under stressful conditions yet prolactin, a hormone that promotes lactation, may be elevated as part of the stress response) to demonstrate that the measurement of stress can be problematic. The report does recognize that short-term, adaptive responses are unlikely to have appreciable effects at the population level whereas chronic (sustained or repeated) stress responses could.

As one external reviewer of the first draft of the manuscript suggested, consideration of the role of catecholamines should form a larger part of the report. It is relevant to later discussion of myocardial lesions, and deserves more extensive treatment here than it was given.

What is the nature of the stress response in cetaceans?

The literature on the stress response in cetaceans is sparse compared with other mammals. There are very few studies specifically investigating this issue, and the review includes the most relevant. However, this section (I.C.4.f) might have been expanded to include information that appears later in the document in order to provide a comprehensive account of the cetacean stress response. The reader should not be expected to bring these points together.

Section I.C.4.f.i. suggests that there are several notable aspects of the adrenocortical response to stress in cetaceans but presents only two, which are in fact the only two currently recognized. This leaves the reader wondering what the others might be. Some attention is given to the modest levels of cortisol in comparison with stressed terrestrial animals, and to the participation of aldosterone (though the mention of studies on phocid seals at this point is not particularly relevant). Beyond that, all other evidence which should appear in this section supports the conclusion that mechanisms of stress physiology are fundamentally the same in cetaceans as they are in other mammals.

It was also suggested in an earlier review that, wherever possible, information should be provided about the duration of the perturbations constituting to the stress response. Both from a comparative standpoint and to allow postulation regarding the lingering physiological effects of encirclement, it is important to consider the time course of changes to various physiological systems. Such detail is not consistently presented.

An additional source of information on the stress response of cetaceans not considered in this report is that derived from studies on animals captured for exhibit and from mass stranded whales and dolphins. The former usually exhibit transient changes in blood constituents, reinforcing the information gained from the directed studies. The latter group often typifies the extreme expression of the cetacean stress response, providing insight into the condition of distress. Under such circumstances, constituents such as cortisol may reach excessively high levels due to impaired hepatic function. It is recognized that much of this information does not appear in the primary literature, but some does and would augment the review. However, omission of this information does not detract from the general impression imparted by the studies considered in the report.

What studies have been undertaken to examine the response of cetaceans to stressors such as might be encountered during fishing-related activities?

The report considers literature relating to the effects of activities similar to or otherwise relevant to the stressors encountered by dolphins during tuna fishing operations. Specifically, chase, confinement and physical manipulations have been examined and found to elicit changes in circulating levels of a variety of blood constituents considered to be indicative of a stress response in other mammals. Still, descriptions of the relevant studies typically do not include information on the duration of the stressful event. For example, the first paragraph of Section 2.B.1 describes a suite of changes in bottlenose dolphins following capture. It is not specified, but important to know, how the animal was captured, including how long it was pursued. This is germane to relating the observations to those presented in the ensuing paragraph, which refers to prolonged chase preceding capture, and to the time frame described earlier for the chase and encirclement activities in the tuna fishery.

The literature on cetaceans contains no directed investigations of the physiological response associated specifically with the psychogenic aspects of the stress response. It is therefore difficult to address the question of whether encirclement followed by release might be stressful by itself, notwithstanding the exertion of the chase or other aspects of crowding and sociopsychological factors detailed in the report. However, the cetacean literature does contain some information relevant to this issue. Captive beluga whales showed anticipatory changes in circulating cortisol concentrations in response to lowering water levels in their holding tank, without any other superimposed stressors (St. Aubin and

Geraci 1992). This observation simply confirms that even in the absence of handling, measurable changes in circulating constituents indicative of a stress response are evident.

What evidence is there of stress and/or associated pathologies in cetaceans examined from the tuna fishery?

Previous efforts to identify morphological changes in dolphins killed during tuna fishing operations have failed to produce evidence of capture myopathy or other conditions that might contribute to delayed mortality. Yet the review still suggests that capture myopathy is likely in some proportion of the encircled dolphins. Four variations of the capture myopathy syndrome are recognized, one of which is the delayed-peracute form. Unfortunately, the condition is not sufficiently emphasized in the report as a plausible mechanism through which delayed mortality might occur. Terrestrial mammals exhibiting the delayed-peracute form of capture myopathy may not succumb until a day or more after initial capture, usually in response to a secondary stress. Damage resulting from the first insult may be subtle and not developed within the time frame represented by chase, encirclement and death in the net for those individuals examined in earlier studies. Thus, while at least one form of capture myopathy remains a plausible outcome of dolphin chase and entrapment, the evidence for its possible occurrence is not presented in sufficient detail.

What effects might be expected, including those not yet observed?

In addition to the possibility of delayed capture myopathy previously described, other potential effects on immune function, reproductive physiology, growth and metabolism are identified in the report and adequately supported with appropriate literature. Chronic dysfunction in any of these physiological processes could reasonably account for reduced fitness of the population, and the presumed failure to recover at the expected rate. The key question is whether the stress associated with encirclement is sufficient to impact any or all of these systems. There is sufficient empirical evidence provided from other species to support the conclusion that it can, and therefore deserves continued investigation.

Comments of other reviewers

In the preparation of the document submitted as part of this peer review, NMFS sought comments from a number of experts and agencies. Many of the comments received were incorporated into the current version, or adequately addressed in a document titled "Responses to comments on the draft literature review ..." Some additional observations on points raised by the earlier reviewers, and on the response by NMFS, are warranted here.

The IATTC noted that there may be quantitative differences in the intensity of stress experienced by dolphins in the ETP and those studied in the literature cited in the review. As discussed above, this point would be more effectively addressed in the review if the author were to include more information about the types and duration of stressors imposed during the experiments. The reader would then be able to better judge the comparability of the conditions in the ETP and in the experiments.

The IATTC also felt that the recognition of peculiarities of the cetacean stress response should temper statements about their reaction to encirclement. NMFS responded appropriately by noting that cetaceans exhibit the basic mammalian response to stress. Lower peak levels of circulating cortisol in stressed cetaceans do not signify a fundamental difference in the function of the hypothalamic-pituitary axis, only that cortisol concentrations are not as useful as a diagnostic indicator of stress in dolphins as it is in other mammals. However, several other constituents do show cortisol-induced changes, demonstrating that this hormone does play a role in the cetacean stress response. By making this point more clearly in the literature review as stated above, NMFS will avoid further confusion on this question.

One reviewer introduced the concept of encirclement as a sub-clinical stressor, costing resources that might be needed for a subsequent response to other stressors, such as infection, or for successful reproduction. The reviewer suggests that no clinical signs of such a condition could be detected. The concept is therefore more academic than useful for the present review, and need not be addressed further.

Conclusions

NMFS has provided sufficient scientific information to establish that tuna fishing activities are potentially stressful to dolphins and that the stress response as determined in other cetaceans could compromise fitness and productivity. Nevertheless, the review should be more cautious in how the conclusions are presented. For example, the Abstract states that as "…it seems likely that reproduction for some proportion of female dolphins will be disrupted…" and "(i)t is therefore plausible that stress … is having a population level effect …" Such statements should be rephrased, in the absence of data, to indicate that reproduction <u>could</u> be disrupted and that it is plausible that stress <u>could</u> have a population level effect, based on our understanding of stress and its effects on other mammals. This basic revision would not invalidate the stated objective nor the mandated task, and would relieve concerns that the review is biased towards a finding of negative effect.

The review provides an adequate framework from which to conduct further studies on the stress response in dolphins subjected to tuna fishing operations, as directed by the IDCPA. The proposed studies currently include necropsies of dolphins killed in tuna nets and an experiment involving the capture, release and recapture of dolphins to evaluate the residual effects of stress. These investigations may provide some of the information absent from the literature reviewed in Curry's report. Some comments on the status and objectives of those studies are provided below.

<u>Necropsy Study</u>

The Act stipulates that a 3-year program of necropsies is to be conducted to address the question of stress-associated pathology in dolphins encircled and accidentally killed in seine nets. This effort was to begin in 1998, but it was not until September 1998 that one tuna-fishing nation, Mexico, agreed to work with NMFS on this project. A training session was held by the SWFSC in January, 1999, but as of the time of this review, no fishing vessels have been made available. Consequently, there has been no progress towards collection of the specimens required by the Act. In the time remaining before a final opinion is due, it will be difficult for NMFS to achieve the intensity of sampling without substantially increasing the sampling effort. It remains to be determined whether this element of the program can yield an appropriately large sample size.

The report from the training session was made available for review. It contains sample data sheets and instructions provided to the technicians charged with conducting the sampling program. Two levels of sampling are described, one representing a minimum series of specimens and a more extensive second level sampling to be undertaken as time and conditions at sea allow. Information requirements were minimized to ensure completeness. While the need to maintain a streamlined approach to data collection is recognized, it is unfortunate that certain potentially useful measures were not included in the protocol. Specifically, blubber thickness and body weight are not required. Such information, when combined with body length and girth, can be used to establish condition indices that might be used as an indirect measure of habitat quality, and thereby help to address the question of environmental variability. It was suggested to NMFS that blubber samples collected for toxicological analysis could serve as a proxy source of data. Assuming that samples are collected in the standardized fashion prescribed in the protocol, thickness measurements could be taken by the laboratory charged with conducting the toxicological analyses.

At the time of this review, NMFS had not established a detailed list of the specific analyses that would be performed, other than generic histopathology, immunohistology, toxicology and genetics. Life history data would be derived from teeth and reproductive tracts, presumably analyzed in-house. Potential collaborators were identified for histopathology and immunological studies. It was suggested that if the sampling program was as successful as originally designed (estimated 150 dolphins in each of 3 years) it might be expedient to selectively analyze those cases for which level 2 sampling had been performed, with a view to equalizing the number of specimens in various age/sex classes to allow statistical comparisons. Delays in the initiation of the necropsy program and uncertainties concerning the extent of cooperation that will be received from the tuna fleet may preclude any need to subset the specimens.

Handling Study

NMFS was instructed to develop an experiment involving the repeated capture of spinner or spotted dolphins to chart the physiological responses to such handling. The expectation is that data derived from such a study would allow a more informed assessment of the potential for chronic or cumulative effects resulting from fisheries-related activities, and thereby address the question of whether such activities might reduce individual fitness or reproductive success. A workshop was convened in July, 1997, to develop strategies for the study, while examining whether there was a reasonable expectation that samples could be obtained and analyses performed to answer this question (Curry, 1998). The workshop concluded that this would be possible, and NMFS has proceeded with preliminary discussions of logistic requirements to complete the experiment. No further details of the project or specifics of the analyses had been developed for the purposes of this review. NMFS suggested that it was likely that a panel of experts would be convened to establish a sampling regimen and identify specific analyses that would provide meaningful information on this issue. The study is expected to variably include blood samples, tissue biopsies, imaging techniques (ultrasound, infrared thermography), and physiological sensors (pulse oxymetry, core and muscle thermometry).

One potential limitation of the study design is that it will not be possible to determine recovery for those blood constituents that are expected to be acutely responsive to the stress of recapture. Thus, while it may be important to recognize that cortisol levels remain elevated because of continued secretion or delayed clearance, this effect may be lost within the elevations associated with recapture. In addition, it will be difficult to establish baseline data, unaffected by capture stress, for many of the constituents of interest. Studies on captive animals benefit from the ability to sample animals voluntarily providing access for venipuncture. Such opportunities will not be available to researchers in the proposed studies.