Final Review of the Report of the Scientific Research Program under the International Dolphin Conservation Program Act

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Executive Summary

The draft report on the scientific research conducted by the Southwest Fisheries Science Centre (SWFSC) on dolphins affected by tuna fishing and the status of their populations was reviewed. The main aim of this research was to assess whether the population growth of the dolphin stocks was increasing following the drastic reduction in the mortality associated with tuna fishing, and if not, then why. The report consisted of four main components: Estimates of present abundance from line surveys; studies that included examination of the physical environment, plankton, fish larvae, prey fishes, other marine mammals and seabirds to determine if there had been a change in the marine ecosystem; stress studies to evaluate the effects of continued chase and encirclement procedures during the fishing of tuna; and assessment modelling to determine the population growth rates during the last two decades.

My review primarily focuses upon the ecosystem component as requested, since this is the area of my expertise; I do, however, offer some suggestions on the other component areas.

The draft report is well written and provides a good summary of the findings of the four components.

The ecosystem studies focused upon whether the carrying capacity for the two main species of dolphins (northeastern spotted [*Stenella attenuata*] and eastern spinner [*Stenella longirostris*]) had changed. While many of the physical and biological variables do respond to ENSO events, there did not appear to be any significant difference between the 1980s and 1990s. Where the data allowed, comparisons of this period with earlier times indicated warmer surface waters, weaker trade winds and increased chlorophyll levels in the ETP after the late 1970s; still, the changes were considered small. The ecosystem report concluded that changes in the carrying capacity of the two dolphin species were unlikely to account for their low population growth rates in recent years. I suggest the report clarify what constitutes carrying capacity for an individual species, especially regarding the effects of predators and competitors on the carrying capacity. Also, since the qualitative category of a "low" population growth rate in the 1980s and 1990s is based on the comparison with the expected growth rate, more justification is needed of how this expected growth rate was derived.

The abundance estimates were obtained from line surveys after calibration and adjustment for factors such as distance from the ship, school size, sea-state, sun glare,

etc. The techniques used were state-of-the-art and provided estimates with a precision of around 20%. The draft report should note that the visual observations were calibrated using aircraft.

The stress component consisted of four separate studies: A literature review, a necropsy study; a review of historical data and a field study involving the repeated chasing, and capturing of dolphins. The general conclusions are that the continued chase and encirclement fishing practices have resulted in added stress on the dolphins but such effects appear to be low and are unlikely to play a significant role in maintaining the apparent low population growth levels.

The assessment modelling examined various fits to the measured abundance estimates from the research surveys, and to a lesser extent with the TVOD, as a means of determining the population growth rates. The main conclusion was that the population growth rates were much lower than expected. My major concern is with how the expected population growth rate is derived and suggestions are given for further discussion.

The present conclusion of an unexpected slow lack of recovery is predicated upon the assumption that removal of the fishing mortality from the depleted dolphin stocks would yield an immediate population increase or recovery. However, the apparent lack of recovery of ETP dolphins is strikingly similar to that of most decimated marine stocks. This is mentioned in the report but should, I think, be more prominent in the discussion.

Several recommendations are made for the complete processing and analysis of available field data and to continue stress-related studies and directed ecosystem studies.

1. Background and Terms of Reference

The Southwest Fisheries Science Center (SWFSC) of the National Marine Fisheries Service has assessed the impact of the eastern tropical Pacific yellowfin tuna purse-seine fishery on dolphin stocks. Since yellowfin tuna often swim with dolphin schools, a fact exploited by tuna fishermen since the 1950s, dolphins were chased and encircled by purse seine nets. Upon pulling in the nets, both dolphins and tuna were caught, resulting in many dolphins being killed. Those dolphin stocks associated with yellowfin tuna began to decline. This decline continued through into the 1970s when dolphin protection practices were enacted. Although purse-seiners still chased and encircled the dolphins, the dolphins could escape before the nets were hauled in and the tuna captured. This has drastically reduced the observed fishing mortality of dolphins and today the observed mortality rate of dolphins from tuna fishing is at negligible numbers. In spite of this, reports by the late 1990s showed little evidence of a recovery of the affected dolphin stocks. In the latter years of the 1990s, the National Marine Fisheries Service (NMFS) was charged with undertaking scientific studies to determine if dolphin stocks were or were not recovering, and if they were

not, then why. The four main components of the NMFS SWFSC studies were: Abundance estimations, ecosystem studies, stress and other fishery effects, and stock assessment. The aim of the abundance estimations was to determine the population size of each of the depleted species. Ecosystem studies were carried out to determine whether ecosystem changes contributed to the apparent low growth rates of the dolphin populations during the past two decades. The stress studies focused on whether repeated chase and encirclement negatively impact the dolphins; such as to cause unobserved but increased mortality. Finally, the assessment work addressed whether or not dolphin stocks were recovering and, if so, then at what rate.

As part of its research, the SWFSC undertook extensive field studies to determine abundance levels, measure various components of the ecosystem (including the physical and chemical oceanography, phytoplankton, ichthyoplankton and prey), and conduct stress studies. The results from these field studies were compared to earlier work in the 1980s; however, data for some physical variables enabled long-term comparison. Assessment work was based primarily upon modelling of the observed abundance estimates to determine population trends. Independent scientific panels separately reviewed each of the four components, consisting of a series of draft reports. The reports were to be rewritten based upon the panel's comments and suggestions. A final report summarizing the results from all four components was drafted and it is this final report that is the main subject of the present review.

2. Description of Review Activities and Summary of Findings

We on the final review panel consisted of one to two scientists from each of the four previous panels. We were requested to familiarize ourselves with the draft Science Report and to review the draft Report. Our reviews were to reflect our areas of expertise and to focus on the component (abundance, ecosystem, stress, or assessment) that we had originally reviewed. We were specifically asked to determine whether the comments and recommendations of the review panels were included in the final draft report, and if they were sufficient and acceptable. While my review concentrates upon the ecosystem component, I have also commented on all aspects of the report. We were also provided with a list of technical reports upon which the final report was based. These reports were supposed to be revised versions of the draft reports we had read and commented on during our earlier panel reviews. I have reviewed all 9 of the revised reports from the ecosystem component of the study, in particular noting the requested changes and the responses by the scientists at the SWFSC. Also, I was provided with the reviews of the other 4 panel members from the ecosystem component to ensure that their comments were fully addressed. Finally, I have read several of the reports from components outside of the ecosystem studies, especially those that might have a bearing upon the ecosystem component.

The structure of the draft of the final report consists of an Executive Summary, Introduction, Scientific Findings, and Technical Appendices. The Introduction (pages 11-17) provides the necessary background information about the problem, the history

of the fishery, the associated legislation, the scientific program, and the dolphin stocks. This information was inclusive, well laid out, and very clear, and thus I recommend no changes here. Each of the four components of the study is dealt with in the Executive Summary, the Scientific Findings, and the Appendices. My review is structured in terms of the study components (abundance, ecosystem, stress and assessment), rather than in the format of the report. Since the main focus of my review is on the ecosystem studies, I shall begin with that component.

I found the draft report very readable and I commend the scientists at the SWFSC for a job well done, especially given the short time, extensive sampling and data processing required, not to mention the numerous reviews and reviewers. Having said this, I still have several comments and questions.

2.1 Ecosystem Studies

I begin my review of the ecosystem studies by summarizing the results from the draft report, then making a number of general comments that apply to the findings. I then comment individually on the sections from the executive summary, the scientific findings, and the technical appendix, providing mainly editorial suggestions. This will be followed by whether the reviewer's comments were adequately addressed in the individual reports.

Results

From the 1980s MOPS surveys and the 1990s STAR surveys, along with available historical data, an analysis of ecosystem changes was undertaken by examining physical and chemical oceanography variables, phytoplankton, ichthyoplankton, prey fishes, other higher trophic level animals, and seabirds. No dramatic changes were observed, although it was recognized that most of the data, especially on the biological side, only spanned the period of depleted stocks, i.e. the 1980s and 1990s. For those physical variables whose data extend further back in time, there was evidence of a small shift in the environment to warmer conditions in the ETP, weakening trade winds and, surprisingly, an increase in chlorophyll-a levels in the warm pool of the ETP. However, given that these changes were relatively small and the lack of additional evidence of changes from the higher trophic level species, the authors conclude that such large-scale changes in the environment were unlikely to have affected the dolphin stocks substantially. This was expressed in terms of the carrying capacity for the dolphin species as not having changed significantly and certainly not enough to account for the calculated 3 to 4-fold decrease in the observed annual population growth rate over that which would be expected.

General Comments

I have no major problems with the results and conclusions from the ecosystem section but do have some comments and slight concerns about emphasis and certain impressions that are given in the report. These are outlined below.

In the initial series of reports on the ecosystem, the principal emphasis was on differences between the 1980s (the MOPS surveys) and the 1990s (the STAR surveys) as had been requested. Some longer-term comparisons were provided for temperature and other physical variables, such as wind. While the 1980s to 1990s comparison does provide information, all of the ecosystem panelists in their reviews strongly stressed that the comparison needed to be between the 1980s-1990s when the dolphin abundances were low and the pre-1980s, ideally in those years when the dolphin abundances were known to be high. Examination of temperature data indicates a shift from a cooler period to a warmer period in the late 1970s. Although this timeframe corresponded to an identified North Pacific "regime shift", the changes measured in the ETP were smaller than those for North Pacific. I feel the panelists' comments are adequately reflected in the present draft report.

The ecosystem discussion in the final draft report is largely centered on whether the "carrying capacity" for the dolphin species has changed. A discussion on the carrying capacity did not appear in the reports reviewed earlier by the ecosystem panel but is used in the assessment modeling (Wade, 2002). The definition of carrying capacity is given as the maximum size of the population that can be sustained within a given area or habitat. It was further noted that the time scale of the determination of the carrying capacity should be on decades or longer rather than interannual scales. For this study, the comparison of the carrying capacity should ideally be between the periods of high and low dolphin abundance, i.e. the 1950s-60s versus the 1980s-90s. Unfortunately, most of the data are limited to the period from the late 1970s-early 1980s, the mid-1980s and the late 1990s. Theoretically, the *maximum* number of dolphins that can be sustained within the ETP is difficult to assess. For the earlier pre-exploitation period, it was chosen as the population size for the dolphin stocks in 1958 (Wade 2002). The final report should include this information.

While the population-level carrying capacity is an ecological concept used in the literature, I find the concept applied to individual species or stocks a difficult one. At the ecosystem level, the carrying capacity ultimately depends upon how much primary production occurs and the conversion efficiency up the food chain. Primary production supports a finite amount of biomass at the higher trophic levels, and biomass distribution at any particular trophic level by species depends upon many factors, e.g. predator and prey abundances, competition with other species, temperature (as a determinant of metabolic rates), etc. For example, in an upwelling situation one could envisage the carrying capacity of the ecosystem remaining stable (constant upwelling), but that through competition one might have sardines or anchovies competitively dominant, as indeed is observed. Under these circumstances it is some internal dynamic, and not the physical forcing, controlling changes in a species' carrying capacity Competition and predation, which have not been examined to any detail in the SWFSC studies, could be as important as the physical environment and prey fields in determining the carrying capacity for an individual species. The report should make clear that the conclusions of carrying capacity were based only upon examination of the available, but limited data on the physical forcing

and prey fields. I would be more comfortable with the concept of the individual species carrying capacity, were there an independent measure of it. I think that some further, but brief, discussion or explanation is needed in the report on the factors causing changes in the carrying capacity of a species.

Since changes in predators and competitors can affect the carrying capacity on the species level, what about predators and competitors of dolphins? Can these account for any of the decreases in growth rate? Do we know? Some comment on predators and competitors should be included in the report, even if only to say that these have not been measured.

In spite of my reservations about the carrying capacity for individual species, I am willing to accept it as used for dolphins; however, I suggest that the authors be clearer about why the carrying capacity might change and how to measure it for the present post-exploitation period, including describing the assumptions upon which the concept or conclusions are based.

The comment is made in the draft report that the carrying capacity would have to decrease 3-4 fold to account for the observed population growth rates. This is based upon an expected 4% population growth rate relative to the observed population rate of 0 to < 2%. This 3 to 4-fold change in carrying capacity is strongly dependent upon the value assumed to be the expected growth rate. Because of its importance, its justification needs to be well documented, complete with references. This was not the case in the report, although it is discussed to some extent in Wade (2002). He states that the 0.04 represents a maximum population growth rate, and that this is a theoretical value not based on observation. Why would one expect the depleted population to grow at the maximum growth rate?

The report notes correctly that the largest amplitude variability occurs at ENSO time scales, with generally smaller amplitude changes at decadal (and longer) scales. It also should be noted, however, that the largest or most significant biological response is not necessarily to the highest amplitude variability or at its particular frequency. For example, current variability is often largest at tidal or storm frequencies but the greatest horizontal displacement of a plankton or passively drifting particles often depends upon lower amplitude, long-term residual circulation. This is because the displacement depends not only upon the amplitude but also upon the time scale of the process. Similarly, if the biology responds to a critical value, it might be based on the combination of the variability at the two scales, or on the time periods above or below the critical value. Thus, in the conclusions in Appendix 6, the use of the phrase, "…the magnitude of all of these longer-time scale changes is swamped by that of ENSO-scale perturbations…" gives the impression that the longer-term variability is not important. I argue that this is unlikely to be true.

Executive Summary

The Report must be specific about the response to a particular forcing. For example, in paragraph 2, page 6, it is noted that chlorophyll-a changed at the same time as the temperatures warmed and the trade winds weakened. The report should state that the chlorophyll-a increased in the warm pool of the ETP, rather than just stating that it changed.

The report must be clear when discussing growth rates whether it is the growth of the population (i.e. numbers) or of an individual (i.e. weight/size) or the combined effect (biomass) that is being considered. Throughout the draft report, growth rate is used in terms of population, but most readers, especially those non-scientists, may likely think of growth rate in terms of individuals.

Scientific Findings

The second sentence in the first paragraph on page 19 suggests that a population reduced by fishing, for example, should recover to its former carrying capacity once fishing is removed. This assumes nothing else changes, which is seldom the case. Usually, if a niche opens such as when a stock is depleted, then something else fills that niche and the original occupier will not be able to recover quickly. In the Gulf of St. Lawrence in Canada, such a relationship occurs between herring and mackerel (Skud, 1982). One species tends to dominate at a time. In the 1950s, a disease struck the then dominant herring stock, decimating it such that mackerel became the dominant stock. The herring abundance remained low for many years even after the disease subsided, recovering only when the mackerel declined drastically. This presumably was due to some form of competition. It is analogous to the upwelling example of sardine and anchovy. In terms of carrying capacity, has it changed? Relative to the species it has; but relative to the ecosystem as a whole, it hasn't.

In paragraph 3, page 20, the maximum intrinsic rate of population growth is suggested to be 4% for <u>small</u> dolphins. What is the reference for this? Also, it needs to be indicated as to what is meant by "small" dolphins? I believe this refers to particular species rather than individuals, but it needs clarification.

In paragraph 3, page 20, line 7, eastern spinner (instead of e sp) and northeastern (instead of ne) need to be spelled out.

In paragraph 4, page 20, line 2 should be changed to read "occurs at periods of 2 to 7 years".

Appendix 6

Second paragraph, page 46, line 4 needs a comma between "indicators" and "which".

Third paragraph, page 46, line 1 should have inserted the following: "Based upon the variability in dolphin abundances," before "The most pertinent time scales…".

Second paragraph, page 47, line 4 requires a change from "or" to "and". On the same paragraph, line 8 requires a change from "may" to "should".

In line 10 on page 48, it is stated that generally the SST time series "…show a prevalence of warm and cool periods of 2-3 years associated with ENSO events….". Throughout the rest of the report and in earlier documents it is stated as 2-7 years variability of ENSO. Should this 2-3 really be 2-7? If not, please explain.

In the caption for Fig. 1, NINO3 should be defined.

On the third paragraph, 4th line, page 51, the term "eastern equatorial Pacific" is used to distinguish it from the ETP. It is not clear what constitutes the eastern equatorial Pacific. It needs to be clarified for the reader.

On page 52, in the section on "Oceanography from SWFSC Observations, 1986-2000", the following should be added to the end of the last sentence "… different programs and the lack of species or size-spectra information".

On page 53, first paragraph, state which of the two species were typically found in large schools and which of the species were in small schools or feed separately.

On page 53, second paragraph, could the increase in common dolphins and pilot whales be having a negative impact on the depleted dolphin stocks, at least in those areas where their ranges overlap?

On page 53, third paragraph, "indicators" should be changed to "indicator species".

On page 54, second paragraph, a comment on whether the habitat preferences of the tuna-dependent and tuna-independent species match those of the dolphins would be informative.

On page 56, first paragraph, the use of the term "swamped" is unclear. I would suggest revising the sentence slightly.

Responses to Reviewer's Comments

I have also gone through the reviews by the ecosystem panel and the revised documents to evaluate the extent of the responses to their comments. In general, I feel the SWFSC scientists took the comments seriously and incorporated them where possible, into their revised reports. More of the comments will be considered in future rewritings, especially regarding primary production. The notable exception was the paper by Gerrodette and Forcada (2002); this was not upgraded to an Administrative Report, but assurances during the conference call indicated that this

would be undertaken in the near future. The following is a paper-by-paper assessment of the revised reports. These comments are largely meant to indicate areas that I feel need to be addressed in any future rewriting, if they are to be submitted as primary publications.

Report LJ-02-14: Balance et al.

This introductory paper was changed little from the original but had few comments from the reviewers, as they were generally satisfied with its presentation. The two minor comments (including catch numbers for the tuna and removing the phrase that the studies may prove to be mainly "academic") were followed in the revised report.

Report LJ-02-15: Fiedler and Philbrick

There were a few changes made to the paper on the comparison in the physical and chlorophyll-a properties between the MOPS and STAR years. The minimal amount of data upon which to base conclusions, which was mentioned by all the reviewers, was discussed in the conclusions. Some references and minor clarifications requested were added. Major changes included a paragraph added on the methods discussing the averaging of the gridded data, some comparisons with observed temperature data, and providing a definition of the thermocline depth, all of which were requested by reviewers. In the Appendix, the authors addressed specific concerns not included in the present draft. I disagreed with their conclusion that a quantitative analysis of correlations between SST and thermocline depth is not relevant to the object of the report. Also, the request by the reviewers for more discussion in the conclusion section was not addressed. Given that this report forms the basis of a paper that is to be submitted for publication, these latter two suggestions (especially the latter) should be undertaken before the paper is submitted. Finally, I would like to see the "EASTROPAC" acronym spelled out.

Report LJ-02-16: Fiedler

As with the previous paper, a few changes were made in this revised version. Most of the editorial and corrections suggested by the reviewers were undertaken, except for spelling out acronyms. The question about the change in ENSO frequency and amplitude was added, with the author noting that their effect upon the biology is unknown. Certain suggestions by the reviewers were addressed in the Appendix. I concur with these except the dismissal of the suggestion to cite the Hoppe et al. (2002) paper. Although speculative, I think that this paper should at least have been mentioned, even if only to state reasons for its dismissal. The following are personal suggestions that have not yet been addressed: A clear definition of the difference between the Eastern Equatorial Pacific, the Eastern Pacific and the Eastern Tropical Pacific; and the use of satellite data to observe the ETP.

Report LJ-02- : Gerrodette and Forcada

As previously mentioned, this paper was not revised, but the authors plan to revise it in the near future, taking into account the comments of all of the reviewers.

Report LJ-02-21: Reilly et al.

This paper examined the interannual variability between the MOPS and STAR years of the dolphin habitats using CCA. The authors included three main suggestions from the reviewers in their revised report, including CCA after separating the data between the core area and the outside area, separation of the common dolphins by stock, and including seabirds and the prey (surface fishes and squid) in the analysis. The authors also presented physical explanations of the axis, which were missing in the earlier draft. The Appendix included a list of the major points from all reviewers, which ones were addressed and if they were useful, and those which they felt would be interesting to pursue later. The latter included variability in the degree of clumping, size or frequency of observed foraging of cetacean groups and examining the changes in amplitude and frequency of the variables (not just mean values).

Report LJ-02-17: Ballance et al.

Changes to this report from the earlier draft included the addition of an abstract and a new section containing results from a CCA of the birds, except the phalaropes, which are mainly coastal migrants. These species dominated the CCA analysis when included and since the interest is largely in the oceanic species of dolphins, it was felt that a more appropriate CCA would be without the phalaropes included. Other comments were addressed in the Appendix but not included in the revised document. The most important of these, which the authors indicate that they will address in future rewritings for publication, include the stratification of the data into the core and outer areas, the application of distribution-free statistical methods, and analysis of the temporal patterns of the variability, and not just their mean values.

Report LJ-02-19: Pitman et al.

Aside from the addition of an abstract, no perceived changes were made in this report. However, these authors did address some of the reviewer's comments in Appendix 2. There they remarked on the comments, even if they did not incorporate them into the revised report. Many of the reviewer's comments were similar to comments on the previous report (LJ-02-17) and will be included in any rewriting of the paper for primary publication.

Report LJ-02-18: Moser et al.

Again, except for an abstract inclusion, there was little change in the report from the original draft. However, the panel reviewers had little criticism of the original draft as it stood, given that it only presented the data. Their main comments were directed

towards analysis of the data and the inclusion of the Bongo samples, both of which have not yet been undertaken due to time constraints. Also, the reviewers suggested that the EASTROPAC samples of larval fish be examined and compared with the data from the MOPS and STAR surveys. These samples are being processed, and the analyses of the entire datasets will be undertaken upon completion of these samples.

Report LJ-02-22: Reilly et al.

The brief draft report was substantially revised, with more text and explanations added. The revisions included the use of distribution-free statistical methods to explore relationships, as had been suggested by the reviewers. Also, according to the authors, the suggestion to explore changes in growth rates, size frequency, species composition, and recruitment, as well as abundance, will be pursued in the future, when more time is available.

Finally, one of the main objections of the ecosystem reviewers to the original reports was the emphasis on the difference between the 1980s and 1990s, which was felt to be inadequate to determine if there had been ecosystem changes that could affect dolphin stocks. The reviewers all felt that what was needed was at least a comparison between pre-late 1970s and the recent time. This was reflected in the present draft of the final report.

2.2 Abundance Estimations

Although we were to focus upon our areas of expertise, I am including a few comments on the other components, but in far less detail than for the ecosystem studies. I think that this is appropriate given their importance to the ecosystem component and to provide a scientific, but non-specialist, view. I will not address whether the comments of the panel reviewers were adequately taken into account; instead, my comments for these components are generally restricted to the draft report. The format is similar to that used to assess the ecosystem component.

Results

Visual (line abundance) surveys were conducted between 1998 and 2000. Extensive corrections were made to account for the distance of the dolphins from ship, sea state, swell height, school size, etc., in order to obtain as precise an estimate of the number of dolphins. The population estimates were made and presented along with estimates of the coefficient of variation based on the standard error of the mean. Genetic studies of the coastal spotted dolphins have suggested that they are composed of six separate stocks, and thus the aggregated abundance estimate for this stock is of limited use.

General Comments

The abundance estimates and the techniques used to obtain them were well documented and considered state-of-art. The efforts to take into account the various factors that could affect the estimates were commendable.

Executive Summary

In the first sentence in the section on Abundance Estimation (page 5), I think it is important to state that a visually-based sighting methodology was employed. I suggest the following wording: Delete "..improved analytical methods for abundance estimation" and replace it with "visual (line abundance) techniques corrected for factors that can influence the estimates such as distance of the schools from the ship, the sea state, sun glare, school size, etc."

In footnote 1, the phrase "…mean, the smaller the value…"should be changed to "mean value and is expressed as a percentage. The smaller the CV, …".

Scientific Findings

On page 18, paragraph 2, the information on the meetings of experts is not needed in this section and seems out of place. It also is repeated in Appendix 5. I would delete all but the paragraph's first sentence and join that sentence with the paragraph following it.

Appendix 5

I think it is important to mention that the visual sights have been calibrated using aircraft sightings, as documented in Gerrodette et al. (2002). I feel this is significant and will help to further convince the reader that you have done everything possible to provide the best abundance estimates given the available method. I would even consider mentioning it in the Scientific Findings section.

On page 44, in the first paragraph, line 5, it is stated that a CV of 20% is a "very precise" estimate. I would argue that it is not very precise, but it is as precise as one will get, based on the methods. This should be reworded.

In Table 1, mean estimates of the abundance and CV are provided. The meaning of the latter is unclear. By definition, the CV of the mean abundance is the error of the mean using the three annual estimates. What is estimated is the average of the individual annual mean CVs. It is not the CV of the mean estimate. It should be made clear as to what is meant by "Mean CV", and what it measures.

2.3 Stress and Other Fishery Effects

Results

With the continuation of chase and encirclement of dolphins as part of the tuna fishing technique, questions abound regarding their effect. This includes not only observable fishing mortality but related unobserved effects, such as mortality after release, separation of calf and mother with the potential for calf mortality, stress-related abortion of fetuses, and effects on reproduction. To answer these questions the center performed four separate studies: A literature review, a necropsy study, a review of historical data, and a field study involving the repeated chasing and capturing of dolphins. The necropsy and field studies both involved small numbers of animals, too small to make quantifiable results at the population level. They do suggest, however, a negative impact on the dolphins from the chase and encirclement fishing procedures, but the effect does not appear to be large.

Comments

An enormous effort was undertaken in the stress studies, but there remains a shortage of information to make sound conclusions on population effects. The conclusion of impacts such as some separation of calves and mothers and muscle damage is well documented.

Executive Summary

On the first line of the first paragraph on page 6 in the section on Stress and Other Fishery Effects Studies, “required” should be changed to “requested”.

Mention should be made to the effects of stress from chase and encirclement on reproduction.

Scientific Findings

The last paragraph, last sentence, page 21, should state what the numbers in the brackets refer to, e.g. 275 eastern spinner dolphins and 295 northeastern spotted dolphins.

In Table 1, the CV values are given as a ratio. Elsewhere in the document (earlier and in the Appendices) the CV is given as a percentage. The values should be expressed consistently throughout the document, either as percentage or as a ratio, but not interchangeably.

2.4 Assessment Model

Results

An assessment model was used to explore the population growth rates of the eastern spinner dolphins and the northeast spotted dolphins. The models were based upon the observed abundances from research surveys in 12 years, between 1979 and 2000. The Tuna Vessel Observer Data (TVOD) also were used, but the authors did not place much faith in the results due to problems with the data. Several models were run to explore the effects of time splitting the data, differences in fishing mortality rates, etc. The general conclusion was that the two dolphin species had low population growth rates relative to their expected growth rates.

Comments

The modelling exercise was extensive and, although outside of my field of expertise, it seemed a reasonable approach to the problem. I did have some difficulties, which perhaps could be alleviated by further textual discussion.

The main problem was mentioned briefly under the Ecosystem component and refers to the use of 0.04 as the expected growth rate; however, Wade (2002) notes that this is simply theoretical, representing a maximum growth rate. I think it is imperative to have further discussion on this issue since the conclusion is that the observed dolphin growth rates are below what they should be. The use of 0.04 as a realistic growth rate needs justification in the report.

The discussion on the TVOD appears contradictory, for statements are made following summarization of the results that the data are not reliable. It is unclear to the reader what one should believe.

Executive Summary

Regarding the first line of paragraph 1, page 8, I disagree that the modelling provides a "means to evaluate the joint significance of all research results". The modelling is valuable tool and helps focus the discussion, but I think a growth model falls a bit short of evaluating the significance of all of the research that has been done.

In the second paragraph, page 8, line 4, the statement that the growth rates are very low has to be in reference to some standard. See general comments.

In the third paragraph, page 8, lines 4 and 5, the statement that the populations "…have <u>potentially</u> decreased over the past decade" followed by "These <u>conclusions are robust</u>…" seems inconsistent. Also, I thought that the models suggested that there was not a statistically significant difference between the decades although there are hints that it occurred. While the hint of the decline in the 1990s is suggested in Wade (2002), I am concerned that the 1990s only have three data points, unless one uses the

TVOD, against which the authors caution. If one takes three points in pre-1990s era, one could easily obtain declining growth rates. Since dolphins do respond to ENSO events, one must not conclude too much from the limited data. There is more evidence if one uses the TVOD, but again the authors appear cautious to conclude much from these data.

Scientific Findings

A decrease in population growth rate after 1990 is suggested (see the last paragraph, first sentence, page 25). However, the indications in both Appendix 8 and Wade (2002) are that the 2-slope model was no better statistically than the one slope model for both species of dolphins.

Appendix 8

On page 79, the exponential model for northeastern spotted dolphins is discussed. Using the TVOD, the 2-slope model was found to indicate the likelihood of a change in population growth rates around 1990. The uncertainty of the TVOD data is discussed within the report and has been mentioned previously in this review. The results are derived from Wade (2002) and the data and model fits appear in Figures 12 and 13. What I do not understand is why the TVOD are different in the two plots in Wade (2002). Did this play a role in the differences present in the model results? In Figure 12, the pre-1990s TVOD shows no apparent trend, but there does seem to be an increasing trend in Figure 13.

3. Conclusions/Recommendations

The findings from the four major components (abundance estimates, ecosystem studies, stress studies, and assessment modeling) of the dolphin and tuna purse-seine fishery study have been summarized in the IDCPA draft report. The draft report was very readable and from the perspective of the ecosystem studies, took into account the major review panel suggestions, as time would allow.

The abundance estimates were obtained from line surveys, after calibration and adjustment for factors such as distance from the ship, school size, sea-state, sun glare, etc. The techniques used were state-of-the-art and provided estimates with a precision of around 20%, which are considered reasonably precise for both northeastern spotted dolphins and eastern spinner dolphins. My only comment on this section of the draft report is that it should include the statement that the visual observations were calibrated using aircraft.

The focus of the ecosystem studies was whether the carrying capacity for the two dolphin species (northeastern spotted and eastern spinner) had changed. Physical, chemical and biological variables from the 1980s MOPS and 1990s STAR surveys were examined. The biological data included phytoplankton, ichthyoplankton, prey

fishes, several large mammals and seabirds. While many of the physical and biological variables apparently responded to ENSO events, there did not appear to be any significant difference between the 1980s and 1990s. For some of the physical variables and chlorophyll-a, comparisons could be made with data from earlier time periods. This analysis indicated warmer surface waters, weaker trade winds and increased chlorophyll levels after the late-1970s, a regime shift corresponding in time with a North Pacific regime shift. However, relative to the observed ENSO variability, the changes were considered small in the ETP. Although we do not know the full extent of biological responses to such changes (a point fully acknowledged), the report concluded that changes in the carrying capacity of the two dolphin species were unlikely to account for the suggested low population growth rates observed in both species. While I can agree with this general conclusion based upon the present analysis, I think the emphasis and discussion need to be modified or expanded, to clarify several points. My understanding of "carrying capacity" for an individual species requires that it depends upon among other things, the physical environment, prey field, predator field and competitors; the latter two fields have not been addressed to any extent by the field studies. The report needs to state more clearly what factors contribute to the carrying capacity for the two dolphin species and when the statement is made that the carrying capacity has not changed significantly, what assumptions, if any, have been made. My point is this: If dolphin predators increased, or if another species filled part of the depleted dolphin stocks' niche, then the carrying capacity of the ecosystem for these individual species has changed. The presence or lack of presence of dolphin competitors to fill the void left by the removal of the dolphins through fishing should be discussed in more detail. Some discussion of dolphin predators is needed. Even if nothing is known about predators and competitors, this needs to be acknowledged, as it may affect our understanding about changes in the carrying capacity. Also, since the qualitative category of a "low" population growth rate in the 1980s and 1990s is made based on the comparison with the expected growth rate, more justification is needed of how this expected growth rate was derived (this comment is also in the assessment section).

Four sub-studies comprised the stress component: A literature review, a necropsy study, a review of historical data, and a field study involving the repeated chasing and capturing of dolphins. The continued chase and encirclement fishing practices have resulted in added stress on the dolphins, and they do suggest a negative impact on the dolphins, likely including unobserved mortality through mother-calf separation, muscle damage, possible fetal abortions, interference with reproduction, etc. The necropsy and the chase and encirclement (CHESS) field studies were based on too small a sample size, disallowing quantifiable results at the population level. Overall, in spite of the added stress and expected mortality of dolphins from the chase and encirclement fishing practices, the actual mortality rates from such effects appear to be insignificant factors in maintaining the apparently low population growth levels.

The assessment modelling was limited to examining various fits to the measured abundance estimates from the research surveys, and to a lesser extent with the TVOD (with many reservations provided), as a means of determining the population growth

rates. Primarily, population growth rates were much lower than expected. Also, there were some indication of a decreasing growth rate during the 1990s, especially if the TVOD were included; but this was either not statistically significant (as in the case of one of the dolphin species), or qualified, since the TVOD were not considered totally reliable. One of my major concerns with this section is similar to that raised above, i.e. the expected population growth rate and the standard to which the recent estimates of growth rate are compared. The expected growth rate does not seem to be well founded, so that although the report suggests that the population growth rates are low (and it seems well founded that they are not expanding, at least not perceptibly), perhaps such high growth rates (as the authors expected) are not realistic. The figure of a 4% expected growth rate needs to be justified if it is to appear in the report. Whatever the expected population growth rate is, it needs to be compared to the range of possible growth rates detectable from the available data and methods. This seems to reach a possible maximum population growth rate at present of 1-2%. If 4% is too high, then what difference is there between a possible 2% growth rate and that which is expected? The report also needs to discuss this further. The present conclusion of an unexpected slow lack of recovery, is strikingly similar to that of most decimated marine stocks (Hutchings, 2000), as mentioned in the report. Hutchings (2000) suggests that one should not expect rapid recovery of depleted stocks. This is a very important point, as much of the draft IDCPA report assumes that if you removed the fishing mortality from the depleted dolphin stocks, the population abundance should rise immediately. Why should the dolphin stocks do this when other stocks do not? It was also assumed for many of these other depleted stocks that they would recover at a rapid rate. A case in point is the northern cod off Newfoundland, which the government announced would rebound in an optimistic 2 years, following a moratorium, even though most fisheries biologists felt that recovery would not occur for at least one generation (or 6-8 years). A decade following the fishing moratorium, there are still no signs of a recovery in the fishery. I think that greater discussion or acknowledgment of this point is required, with relation to the dolphin growth rates. Finally, I found the discussion with the TVOD confusing. It was introduced, conclusions were drawn, and then qualifications were given that the data are probably unreliable. I was left not knowing if I should believe anything with these data or not.

In spite of my reservations, I believe the present draft report is an excellent start. I do recommend that some further clarifications and qualifiers are needed to make certain points clearer to the reader, and I have provided above some examples of where I think these are needed.

The work done by the SWFSC in studying the ETP dolphins and their ecosystem is commendable and unprecedented. A large scientific effort has been made, and a tremendous amount of data has been collected. The scientists have done a great job in the short time they have had, in terms of processing and analyzing the data, especially given the imposed time deadlines for the various panel reviews. However, there are some data that have not yet been processed (nutrient data from STAR surveys, and the bongo net samples from the ichthyoplankton surveys), and the earlier EASTROPAC data have not been retrieved. Not all analyses have been carried out

due to time restrictions. For example, the ichthyoplankton have not undergone any extensive analysis owing to the long time required to process the samples. The SWFSC should be provided the necessary resources to continue their work, especially in light of the tentative conclusions from both the ecosystem and stress studies. Such conclusions could be strengthened with additional data and analysis. Given that they are carrying out new and, in some cases, innovative research, and that such studies will provide additional insights into the dolphin populations, I strongly recommend both a completion of the present analysis of the field data and continued measurements in the stress studies and directed ecosystem studies. This was a general theme of all of the ecosystem panelists. Some specific recommendations are listed below.

Data Processing

- Initiate data recovery project on the EASTROPAC surveys as soon as possible to prevent additional loss of any of these invaluable data.
- Proceed with the processing of the nutrient samples from the STAR surveys
- Complete the identification and counting of the ichthyoplankton from the bongo tows.

Data Analysis

- Use non-parametric methods such as rank correlations to obtain a quantitative measure of the trends in the available data sets.
- Examine changes in the amplitude and frequency of the variability in the physical and biological variables, not just their mean values.
- Determine, if possible, the changes in the size and frequency of foraging groups of cetaceans.
- Explore ways to determine the biological response to changes in the magnitude and frequency of physical forcing such as ENSO events.
- Determine whether the size or spatial distribution of seabird flocks in the ETP have changed, in particular those associated with the tuna.

Additional field and lab studies

- The ecosystem measures suffered partly from the fact that they were tagged onto the surveys designed to obtain abundance estimates. These did not represent the optimal design for ecosystem studies. If possible, targeted ecosystem surveys should be undertaken. This would include, among other things, the following: size fractionation of the plankton and small prey; finer mesh nets to collect zooplankton samples for comparison of gut contents in the stomachs of the larvae; collection of otoliths for growth studies of the fish; and measuring growth rates of the flying fish and possibly myctophids using length-base methods.
- Cetacean studies, including necropsy and stress studies should be continued until the necessary number of samples has been obtained to make quantitative statements about effects at the population level. In addition, measurements of the

growth and reproduction of dolphins are needed. Diet studies would be beneficial to determine which of the prey species are most important and whether there have been any observed changes in these species or in the diet of the dolphins.

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Drinkwater, K.F. 2002. Review of Ecosystem Research conducted as part of the studies to assess the impact of the eastern tropical Pacific yellowfin tuna purse seine fishery on dolphin stocks. Review paper prepared for the University of Miami Center of Independent Experts, 25 p.

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Appendix 1: Statement of Work

Consulting Agreement Between The University of Miami and Dr. Ken Drinkwater

Background

The tuna purse seine fishery has used the association between tuna and dolphins to fish in the eastern tropical Pacific Ocean (ETP) for over five decades. Three stocks of dolphins were depleted by high historical levels of dolphin mortality in tuna purse seine nets, with an estimated 4.9 million dolphins killed during the fourteen-year period 1959-1972. After passage of the Marine Mammal Protection Act (MMPA) in 1972 and the increased use of fishing equipment and procedures designed to prevent dolphin deaths, mortality decreased during the late 1970s, 1980s, and 1990s to levels that are generally considered biologically insignificant.

While changes in the fishery have dramatically reduced the observed mortality of dolphins, the MMPA, as amended by the International Dolphin Conservation Program Act (IDCPA), requires that the National Marine Fisheries Service (NMFS) conduct research consisting of three years of population abundance surveys and stress studies to form the basis of a determination by the Secretary of Commerce regarding whether the "intentional deployment on, or encirclement of, dolphins by purse-seine nets is having a significant adverse impact on any depleted dolphin stock". The Secretary must make a final finding in this regard by December 31, 2002. It should be noted that this issue is controversial and particularly relevant to persons involved with NMFS, the US and non-US tuna industry, and environmental groups.

The topic of this review is the IDCPA Science Report that will be presented to the Secretary of Commerce, along with information obtained under the IDCP, and other relevant information to form the basis of the Secretary's final finding. The IDCPA Science Report is comprised of the results of all research activities required under section 304(a) of the MMPA, as amended by the IDCPA. Each major component of this report has been separately considered in a series of independent peer reviews conducted by the Center for Independent Experts (CIE). These consist of: the Abundance Review (October 15-17, 2001) the Stress Review (February 4-6, 2002), the Ecosystem Review (March 6-8, 2002), and the Assessment Model Review (April 3-5, 2002).

<u>Abundance Review</u>

The topic of this review was the abundance of several species of tropical pelagic dolphins that associate with tuna and are killed in the ETP purse seine tuna fishery. Estimates of dolphin abundance based on cruises carried out in 1998-2000 form a central part of these studies. The main task of the consultant was to review the methods used to estimate abundance from line-transect data, including covariate detection models. The fact that these dolphins occur in a wide range of school sizes presents unique problems for the estimation of expected group size, so considerable effort has been devoted to this analysis. Documents supplied to the reviewers included draft manuscripts describing the

covariate analysis, simulations to test the performance of several estimators, calibration of school size estimates, and assignment of partially identified sightings. Background papers included previous relevant publications and reports. The raw data and software used in the analysis were also made available.

Stress Review

The stress studies mandated in the IDCPA include: 1) a review of relevant stress-related research and a three-year series of necropsy samples from dolphins obtained by commercial vessels; 2) a one-year review of relevant historical demographic and biological data related to the dolphins and dolphin stocks; and 3) an experiment involving the repeated chasing and capturing of dolphins by means of intentional encirclement. This review included a suite of studies subsumed under this general topic, and a brief description of these studies follows.

The necropsy program analyzed samples from about 50 dolphins killed incidentally during fishing operations. Historical biological samples and data were analyzed to investigate stress-activated-proteins (SAPs) in the skin in dolphins killed in the fishery and live-sampled via biopsy. Historical data were also examined to assess separation of cows and calves during fishing operations. Chase Encirclement Stress Studies were conducted during a two-month research cruise aboard the NOAA ship McArthur in the ETP. During this project, the team worked in cooperation with a chartered tuna purse seine vessel to study potential effects of chase and encirclement on dolphins involved in tuna purse seine operations. Dolphins groups were found to be much more dynamic than previously recognized, making it extremely difficult to recapture groups of dolphins over the course of several days to weeks, as planned.

In the end, nine different dolphins were tracked for 1-5 days during the course of the study, including two animals outfitted with a thermal tag that recorded heat flux, temperature, and dive data. Individual radio-tagged dolphins and 1-4 associated roto-tagged dolphins were recaptured on several occasions spanning shorter periods of 1-3 days. Six satellite tags were deployed to record movement and dive data on dolphins that were not recaptured. Biological data and samples were collected from as many captured dolphins as possible, and include: 70 blood samples, of which 18 were from repeat captures of marked individuals; 283 skin samples, of which 17 were from previously captured and sampled animals; 449 analyzable thermal images; 52 core temperatures; and 95hrs of heat flux data. Females with calves were noted on several recapture occasions, and one known calf was skin sampled during an initial and subsequent capture.

Ecosystem Review

To complement the three-year abundance studies, population assessments were made for the following years: 1986, 1987, 1988, 1989, 1990, 1998, 1999, and 2000 with a primary goal being to determine if populations that were historically reduced in size are increasing over time. Should the assessments indicate no increase (lack of recovery), three broad categories of factors could be the cause: a) effects from the fishery; b) effects

from the ecosystem; c) an interaction between the proceeding two factors. This need to attribute causality for a potential lack of recovery serves as the primary justification for ecosystem studies. By investigating the physical and biological variability of the ecosystem of which the dolphin stocks are a part, we establish a context, which can be used to better interpret trends in dolphin abundance. A lack of recovery that is not mirrored by some other change in the ecosystem would largely eliminate an ecosystem hypothesis, leaving fishery effects as the most likely cause.

This review included a suite of studies subsumed under the general topic of ecosystem research in the ETP. The basic approach was to compare ecosystem parameters over time with a primary goal being to look for indications of a potential ecosystem shift. The power of these ecosystem studies increased with the number of environmental variables, taxa, and trophic levels included, and with the time period spanned (although most ecosystem data available for these investigations were collected concurrently with dolphin assessment data aboard NOAA research vessels and are restricted to the late 1980s and late 1990s).

The general components of the ecosystem research included: 1) physical and biological oceanography: sea surface temperature, thermocline characteristics, phytoplankton and zooplankton distribution and relative abundance; 2) larval fishes: distribution and relative abundance; 3) flying fishes: distribution, relative abundance, and habitat relationships; 4) seabirds: distribution, absolute abundance, and habitat relationships; and 5) cetaceans: distribution, absolute abundance, and habitat relationships.

Assessment Model Review

As indicated above, NMFS was charged with essentially determining whether or not the depleted dolphin stocks are recovering, and if so, at what rate and at what level of certainty. The topic of this review was the overall framework that will be to estimate the growth rate of two dolphin populations of interest, the northeastern offshore spotted dolphin and the eastern spinner dolphin, using growth rates estimated by fitting a population model to the three-year and other available estimates of abundance. For this review, estimates from research vessel surveys using line transect methods are available for three periods: 1979-83 (four estimates), 1986-90 (five estimates), and 1998-2000 (three estimates), for a total of twelve estimates over twenty-one years. Reviewers were also asked to evaluate the inclusion or exclusion of a set of fishery-dependent indices of abundance, resulting from data collected by tuna vessel observers. Two types of population growth rate will be estimated: (1) exponential rate of change from 1979-2000 and (2) intrinsic rate of increase under the assumption of a density-dependent model where pre-exploitation population size in 1958 is considered carrying capacity. Both an aggregated population model and an age-structured model will be used. Bayesian statistics, using a numerical integration method, were used to estimate a probability distribution for the population growth rate.

Specific Reviewer Responsibilities

For the final IDCPA Science Program Review, expertise is needed to review all components of the research described above, specifically with respect to NMFS' incorporation of comments previously received from the topical reviews also described above. Reviewers will be provided with the draft IDCPA Science Report, as well as comments received as a result of the CIE reviews and explanations of how/why such comments were or were not incorporated into the report.

The reviewer's duties shall not exceed a maximum total of 11 days, including:

- 2-3 days to read the draft IDCPA Science Report (to be provided to the reviewers by no later than August 2, 2002);
- 2-3 days to produce a written report of the reviewer's comments and recommendations on the draft report;
- 1-2 days to discuss via telephone, on August 15-16, 2002, with relevant NMFS staff from the NMFS La Jolla Laboratory, the incorporation of comments and any related questions; and
- 2-3 days to revise the written report based on those discussions.

It is expected that each reviewer will have participated in the earlier CIE reviews of IDCPA research described above and will not require general presentations of research results, but will focus on addressing comments and recommendations included in the reviewers' reports in his/her topic area. Reviewers should particularly consider whether the responses to the original review comments are sufficient and acceptable, in a manner similar to the role filled by a journal editor when considering manuscripts revised in response to referees' comments.

Each reviewer's report shall reflect the reviewer's area of expertise; therefore, no consensus opinion (or report) will be required. Specific tasks and timings are itemized below:

1. Read and become familiar with the draft IDCPA Science Report provided in advance;

2. No later than August 13, 200, submit a written report of findings, analysis, and conclusion in the individual reviewer's topic area to NMFS;

3. Discuss relevant documents with scientists from the NMFS La Jolla Laboratory via telephone on August 15-16, 2002, to facilitate proper incorporation of reviewers' comments;

4. No later than August 23, 2002, submit a revised written report of findings, analysis, and conclusions based on discussions held with relevant NMFS staff

from the NMFS La Jolla Laboratory. The written report[1] (See Annex I) should be addressed to the "University of Miami Independent System for Peer Review," and sent to Dr. David Die, via email to ddie@rsmas.miami.edu, and to Mr. Manoj Shivlani, via email to mshivlani@rsmas.miami.edu.

[1] The written report will undergo an internal CIE review before it is considered final. After completion, the CIE will create a PDF version of the written report that will be submitted to NMFS and the consultant.

APPENDIX 2: Bibliography of Materials Provided by the Center for Independent Experts

Dower, J.F. 2002. A review of ecosystem research in the eastern tropical Pacific during the monitoring of porpoise (MOPS) and *Stenella* Abundance Research (STAR) Programs. Review paper prepared for the University of Miami Center of Independent Experts, 26 p.

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