Second Review of

Transect Sampling Methods to Obtain Population Size Estimates for Northeastern Offshore Spotted and Eastern Spinner Dolphins

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Executive Summary

This report considers recommendations from Center for Independent Experts (CIE) reviewers' comments on estimating abundance using transect surveys conducted by NMFS. The changes made to the methods and reports have been compared with issues raised by reviewers to ensure they have been addressed.

Neither reviewer had significant problems with the methods used. Both felt significant improvements in the analysis of these sorts of transect data had been undertaken, and that the estimates of abundance were more reliable than estimates obtained previously. However, each reviewer suggested a number of clarifications in the document, and the consideration of possible improvements in the methods and recommendations for future work.

The documents are improved over previous versions, particularly the report detailing the estimates of abundance (LJ-02-06). It is much clearer what methods have been applied, and how the estimates have been improved in each case. The authors have reacted appropriately to recommendations of improvements in the models. They have made adequate concessions addressing the issues raised. Any remaining differences in opinion are subjective, minor, and/or would not lead to any significant changes in results. All other recommendations were made with reference to doing further research, and are not relevant to the current study.

The abundance estimates were used in the population model (LJ-02-13), where they were assumed to have a log-normal likelihood. I could not find a statement to the effect that the estimate likelihoods could be approximated by a lognormal. Such a statement with appropriate evidence would link these two studies much more firmly.

Apart from this, my final conclusion is that no other improvements are necessary to these documents beyond small editorial changes (Appendix 3), although there are some recommendations for further research.

Introduction

This review follows a detailed review conducted La Jolla Laboratory 15-17th October 2001 by Robert Mohn (Reviewer 1) and P. Medley (Reviewer 2). The full background to these reviews is described in the Statement of Work (Appendix 1). The reviewers produced independent reports on the methodology and population estimates for the two main dolphin species. Both reviewers found the methods used were sound, but suggested clarifications to the document and some improvements to methods. The aim of this review was to check what the authors had done in response to the comments and discussion of the reviewers, and ensure that the documents had been corrected or improved in line with the independent reviewers' suggestions. The current task would also have included negotiation in any dispute should one arise between the report authors and the reviewers views, but no such dispute has arisen.

Methodology Review

In general, the reviewers found the methodology sound and indicated that they felt it improved the estimates of abundance over previous analyses. There were some minor concerns over the methodology, requirements for clarification and recommendations for future research. While future research may be of interest, it has no implications for the current methods, estimates and conclusions. The following considers each document provided separately, comparing the original review with the author's reply and the modified report.

Responsive movement and g(0) for target species of research vessel surveys in the eastern tropical pacific

John Brandon, Tim Gerrodette, Wayne Perryman and Katie Cramer
LJ_02_02. Previously unreviewed.

While this paper has not been previously reviewed, some evidence of this research had been presented to the reviewers. The paper looks at the available data to test whether there are responsive movements to the research vessel by the dolphins which will bias abundance estimates. In my opinion, the general conclusions which can be drawn from this paper are:

- No consistent response to the transect line could be detected from the available data. While this does not exclude any effect, it does suggest the effect will be small relative to other errors in the analysis.
- Even if an effect is present, it probably has not changed over the years of the survey; so, population trends are not affected.

Probably the best way to estimate this effect would be to increase the use of helicopters in aerial-monitoring of school movements before and after detection by the research vessel. Such information could be continued to be collected on future surveys.

In summary, I believe the authors have demonstrated, as far as possible, that this is not a significant error source for their estimates of abundance.

Estimates of abundance of northeastern offshore spotted, coastal spotted, and eastern spinner dolphins in the eastern tropical Pacific Ocean.

Tim Gerrodette and Jaume Forcada
LJ-02-06

Document Clarity

Reviewer 1 stated "This document was completed, but required substantial clarification as many explanations of details were sometimes too terse to be followed by an external reviewer." Reviewer 2 requested the authors should make it as clear as possible what was used for final results. The paper has a much improved discussion section which addresses both Reviewers' concerns. There is much clearer explanation on what improvements have been carried out and what implication they have for the final results.

Reviewer 1 indicated more simulation results would be useful, together with details on the fit diagnostics. More details have been given in LJ-02-07. The number of fits and the way the method has been conducted would exclude a complete set of diagnostics for model fits from the final document. I would expect much more extensive documentation on the method (such as the software) and results would be kept by the researchers for future reference. These need not be published as I believe it is difficult to assimilate very large amounts of such information, and therefore adequate provision is made in the current documents.

Reviewer 1 suggested that a comprehensive sensitivity analysis should be performed. The discussion section sets out the effect of changes, and most importantly indicates whether such changes might affect the population trends. I also support the view of the authors, who state that "We believe estimates are improved by using more accurate data, regardless of sensitivity." However, sensitivity indicates the covariate modelling in the detection function is probably the most important change; therefore, this should probably be the focus of discussion on the improvements.

Reviewer 1 also raised the issue of using the Tuna Vessel Observer Data. The suggestion is more relevant for the population modelling, where these data were indeed used (see below). However, corrected indices from these data are included as plots in this article, which helps in comparing indices.

Consistency in modelling across years

Reviewer 1 and 2 suggested a more consistent modelling approach across years. That is, they suggested using the same structural model for all years, while allowing parameters to be estimated independently. As far as possible, the same corrections have now been applied across all years, and the authors have decided to apply the half normal consistently across all models as their best detection model. However, the covariates in each year were still chosen independently, based purely on statistical criteria. The authors justify this on the need to maintain the independence of the abundance estimates in each year. They also make the valid point that covariates are correlated; so, to some extent, they share effects. Finally, they have averaged estimates over models weighted by the AIC which will smooth over the transition between models. There is also the issue that genuinely different structural models may apply.

The general argument in favour of a single set of covariates fitted each year can be summarised, as follows:

i. As the parameter values are not of interest, over-fitting producing high parameter estimate correlations are less important.
ii. Parameters which do not meet the AIC (or other statistical) criteria presumably do so, at least partly, because they are small when estimated. Excluding a covariate from the model essentially sets the covariate parameter to zero. Estimating the parameter where it is not significant should not affect results.
iii. Particular covariates either do or do not affect detection, and while the size of the effect may vary, their presence / absence should not. School size, like perpendicular distance, can be justified on theoretical grounds; therefore, it should be included whether judged statistically significant in any individual year or not.
iv. Statistical tests will depend upon sample characteristics, such as sample size. By considering the model structure across years, effects of sample size on the model structure may be eliminated.

I would summarise arguments for the authors' approach as:

i. The most important consistency problems were dealt with by choosing the half-normal model.
ii. The method is objective. In practice, there is no other clear objective way to choose covariates across years while maintaining parameter independence. Perusal of results 1998-2000 (LJ-02-07) suggests that a linear predictor -perpendicular distance + school size (pd+gs) - could be applied, but I cannot see how this could be decided objectively without fitting the model across years.
iii. The authors have chosen to use the model averaging procedure. While more complicated, this does stabilise estimates by smoothing between separate models. It is worth noting, however, that the model averaging means that, strictly speaking, the estimates are no longer maximum likelihood.
iv. Parameters close to zero, where the inclusion is borderline, will mean estimates from the procedure chosen will not greatly differ from alternative approaches. This is supported by the abundance estimates not being very different among models. Likewise, correlations between parameter estimates will minimise differences between models.
v. The approach allows for unknown structural differences between years, while minimising the impact false statistical inference through the model averaging procedure and use of the small sample QAIC.

It is clear that the authors have justified and validated their method, and it is not possible to make a recommendation of a better procedure. In summary, I believe the method:

i. Has met the major concerns of the reviewers.
ii. Would probably not lead to very different results to a reasonable fixed covariate model considering the observation errors. As noted by the reviewers, the model structure did not change wildly from year to year and was, in any case, averaged.
iii. Is fully simulated in the bootstrap. The bootstrap procedure should generate reasonable estimates of the associated error.

Survey Design

Reviewer 1 suggested re-assessing the statistical implications of the survey design. Reviewer 2 suggested modelling the survey in the bootstrap, while recognising the major logistical problems in designing such surveys. Clearly the researchers involved in these surveys have given these issues much thought, which the authors indicate. Survey design should be regularly re-examined to minimise bias and make the survey as efficient as possible, but the points are not directly relevant to the current estimates.

Reviewer 2 suggested that the spatial dependency in the survey design could be used in the bootstrap procedure so that the resampling procedure could simulate the conditional probability of encounter, for example. The authors point out this is more complex than it might seem, because of unknown oceanographic feature on the scales of hundreds of kilometres which contribute to a non-random pattern among daily transects. This is perhaps also an issue for future consideration.

Future Research

Reviewer 2 suggests some additional approaches to modelling the detection function. The suggestion based on radial distance has already been considered and found to give poorer estimates. Some other ideas may or may not be useful for future research, but have no implications for the current estimates.

Multivariate methods for size-dependent detection in conventional line transect sampling

Jaume Forcada
LJ-02-07

The method described contains a number of improvements beyond those suggested by the reviewers. This paper is a summary of a considerable amount of work, but its description of the methods applied is somewhat terse, so that it is difficult to follow what was done without turning to the referenced papers. This is fair enough in a scientific paper, but it does presume significant knowledge of a variety of techniques. The results are clearly stated, however, and the choice of final method (multivariate parametric detection model) appears to have been the best of those tested. This makes sense to me as non-parametric methods, while robust, tend to be worse than methods where some functional form (i.e. extra information) can be imposed on the data.

Reviewer 1 suggested that the simulation study description was too brief and required a more realistic covariate model. Both these issues have been addressed in the new document. In my opinion, the most useful results for observing how the different methods behave is the simulation study. This is still short, although the table gives fairly complete statistics. It is not clear to me what additional simulation work might be presented without simply overloading the paper with results. The author has chosen to present a simulation, indicating the pattern of results and justifying the final choice of model. I believe what is presented is adequate for this purpose.

The software has been tested by comparing results with other software over common analyses. The authors have shown their software produces results that are consistent with those obtained with other software, which tests for coding errors and artefacts in the minimisation routine. The author states that he has extensively tested for errors to ensure the most possible reliable results. Given the complicated data set (for example, the proration algorithm) the author appears to have made every effort to produce reliable results.

A concern was raised by the reviewers over the variability in estimates induced by outliers (large schools at a great distance). This merely repeated the concern of the author himself. The occurrence of such large schools is taken into account in the bootstrap procedure. This would make the procedure robust as long as the full probability distribution is used rather than just point estimates for the abundance.

Reviewer 2 commented on the problems with correlations between covariates. The author pointed out that inclusion of collinear covariates was avoided, based on an ad hoc analysis of covariate effects, and states that "the problem of relationships between covariates was not found to be a source of structural error in models and/or bias, and any potential bias should be small." The issue over the types of covariates to record and their use is, again, more relevant for future research.

Reviewer 2 suggested alternative detection distance as opposed to the perpendicular distance used. The author points out that this approach has been considered before, but that was found unstable. While this approach might still be pursued in future, it has no implications for the current assessment.

Calibrating group size estimates of dolphins in the eastern tropical Pacific Ocean

Tim Gerrodette, Wayne Perryman and Jay Barlow
LJ-02-08

Reviewer 1 was concerned that some observers were not calibrated. The results from this paper have been extended to all observers, which should remove some bias. The bootstrap procedure, and how the calibration is applied to all observers, is now clearly described in LJ-02-06. The method has a weighting procedure based on the calibration error, which was also suggested by reviewer 1.

Reviewer 1 pointed out that if the remaining school estimation error is irreducible, some further types of training might improve estimates. The authors point out that real-time feedback is most desirable, but not currently possible. If this is relevant, it would only be useful for future research and not the current assessment. Clearly, a considerable amount of training is already undertaken, and it may not be possible to improve subjective school size estimation much beyond using the current methods.

A suggestion from Reviewer 2 was to improve the calibration by changing the fitted model. The model fitted, as described by the report, minimises the squared difference between the estimated school size and the expected estimated school size modelled as a function of the true school size. The calibration reverses this equation, modelling the expected true school size as a function of the estimated school size. Reviewer 2 pointed out that the authors could have fitted a calibration model directly. This would have: a) minimised the sample's calibration error, which they did not do; and b) could have made the modelling easier. In reply, the authors pointed out that the estimates are derived from the school size and therefore their model is in the natural (dependent / independent variable) form.

In these circumstances, it is possible that using an arbitrary model, one that is not based upon how estimates are derived, may introduce structural error. This should not affect the MSE within the sample, but may introduce bias when applied to non-sample estimates (those without a photo count). I would argue that they are not trying to model how estimation takes place but correct estimates for which I believe the only relevant criteria is the mean squared error between the corrected estimated and the true (log) school size. The easiest way to test whether the method the authors used could be significantly improved is to run simulations calibrated in three ways: a) using the method described (fitting equation 1, calibrating using equation 2); b) fitting the calibration model directly (fitting equation 2); or c) fitting a simple linear model such as that described by the reviewer. Table 1 presents the results from simulated data that is generated by equation 1. That is, it is assumed the model defined by the authors accurately describes the way in which estimates are derived. Each of the three types of calibration were then carried out on each of the simulated data sets.

Table 1 Results of true school size estimated from observed data on school size as the average values from 1000 simulations. Each simulation consisted of 344 observations, 86 of which have been sampled (photos of schools are available). The data were simulated using equation 1 with a √MSE of 0.25 for the model form and estimates from observer 4 model 6 (see LJ-02-08 Appendix A), and a log-normal school size distribution. The weighting between high, best and low estimates were not used. The MSE and bias from fitting the model again are given in column 1 and the resulting calculated calibrations are given in column 2. This repeats what the authors describe. The direct calibration estimation fits the calibration calculation equation they used. By definition, this will give a sample MSE equal to or lower than the MSE from the authors' calculation, although the improvement might be expected to be small. A similar result is obtained from the simple linear regression, even though this model was not used to simulate the data. Bias was not significant in any case.

		Fitted Equation 1		Direct	Linear
		Model	Calibrated Estimates	Calibration Estimation	Calibration Estimation
MSE	**Sample**	0.0568	0.0685	0.0621	0.0622
	Non-Sample	0.0650	0.0816	0.0769	0.0770
Bias	**Sample**	0.02%	-0.02%	0.00%	0.00%
	Non-Sample	-0.05%	0.09%	0.08%	0.09%

The general results from the simulation suggest that the calibration model fitted directly only slightly reduces the MSE of the calibration estimates, but this improvement remains for the non-sample estimates. However, a simple linear regression correction does just as well. In both cases, the improvement over what was done is trivial, therefore there appears to be no reason to alter the analysis. I do not believe what the authors did is in any way wrong, but in my opinion, in the future a simpler and slightly improved linear regression could be applied. Any alternative approach could be tested more realistically using a non-parametric bootstrap procedure.

This suggestion is probably most relevant to future research. Using a weighted linear regression approach to the calibration would make combining all observers into a single model much easier. Trying to implement the method described by the authors simultaneously across all observers would be difficult and slow.

Accuracy and precision of perpendicular distance measurements in shipboard line-transect sighting surveys

Douglas Kinzey, Tim Gerrodette and Daniel Fink
LJ-02-09

Both reviewers agreed that the methods described should improve the accuracy of perpendicular distances. Reviewer 2 expressed support for corrections based on well-known physical effects over the regression-based adjustments, but expressed some concern that the data used for the refraction corrections were not available for all years. As a result the authors chose a method so that all measurements in all years have been adjusted for the refraction component. This is only small improvement, but it is straightforward and uncontroversial.

Assessment of the population dynamics of the northeastern offshore spotted and the eastern spinner dolphin populations through 2002

Paul R Wade
LJ-02-13

The population models are based on Bayesian techniques. The method used uninformative priors for various population model parameters, by-catch mortality data and the population size estimates from the surveys reviewed above.

Reviewer 1 suggested an age structured population model based on the photographic data. An age structured model was used based on aged sample of the incidental kill. The size frequency data possibly obtained from the photographs may perhaps be considered for future models.

Reviewer 1 also raised the issue of using the TVOD data, a point also raised by the separate reviewers of the population model; these data are now included in the assessment. However, the author believes these data are unreliable. Given the rigorous methodology applied to the surveys, I support the view that the transect survey estimates are much more reliable than the TVOD index, but this should not necessarily exclude the TVOD from the assessment.

The author reports the population size estimates were assumed to have a log-normal likelihood. I was unable to find any statement in the other document on the estimate likelihoods. I assume log-normal is a reasonable approximation. If so, it would help if this is stated in the result section. That is, it is stated that the mean and CV adequately describe the distribution of the bootstraps, and, by extension, approximates the likelihood for these estimates. Therefore, they are sufficient statistics for the population assessment.

Recommendations

Current Assessment

The authors of the document detailing the estimates of dolphin abundance (LJ-02-06) should add additional information and text indicating whether log-normal is truly a reasonable approximation for the likelihood for these statistics. I suppose the bootstrap sample frequency probably provides the best approximation to the likelihood and, for this purpose, can be examined in each year. A smoothed bootstrap (using a smoothing kernel) could be developed if the log-normal is found to poorly describe the tail of the likelihood distribution.

Other suggested small changes to the document text are described in Appendix 3.

Further Research

The current method provides an innovative and sound basis for the estimation of the dolphin populations. Further refinements are not likely to yield great benefits in terms of reducing errors. The largest improvements are most likely to be obtained from improved data collection, rather than refinements in the modelling. Continuing surveys in future years and focusing on key issues such as fishery induced mortality would appear to be higher priority. Obviously, to take account of the new data, the models may need to be adapted or extended.

Improvements in data collection could include a wide variety of measures, such as:

- Collecting size frequency information from photographs. This is perhaps the most important activity as it may allow monitoring population structure. This will not only allow more rapid detection of population increase or decrease, but may also offer some explanations why changes are occurring. For example, increased mortality will produce a different population structure to decreased recruitment. I understand these data are already being compiled from the aerial photographs.
- Varying detection parameters experimentally. The covariates used, such as school size or bird association were, in general, not under control of the researchers. A few are however, and varying these may be much more informative in choosing an appropriate detection function (currently the half-normal). Variables that can be altered include vessel speed, which varies the time an object is at each detection distance, and numbers of observers on deck, which presumably affects chance of detection (probably asymptotically with numbers of observers). It may be difficult or expensive to alter these variables, but just a few trials may indicate whether they are informative enough to be worth pursuing.

The covariate modelling is an excellent extension on this method. In this implementation, I believe further improvements may be possible. Although co-linearity is not important, non-linear relationships among covariates and between covariates and the detection function parameter could be. For example, school size and bird association may not be independent factors, so that bird association may only have a significant effect when school size is small. Such effects may be subtle and difficult to fit without additional information on relationships or other covariates, perhaps obtained from discussions with the observers themselves and GLM analyses. Improving the covariate models may not only increase precision of the estimates, but it could increase stability and allow wider transects, making better use of the available data.

It is likely that a simpler estimation procedure will have to be adopted for the current method to progress. The estimation procedure has become very complex in order that it might provide the best current scientific information for the assessment. However, for improvements to continue a simpler standard procedure will probably be required. For example, if the next aim on improving the methodology is to model the abundance estimate marginal likelihood probability distribution for use in the Bayesian assessment (as opposed to approximating it using bootstrap frequency), the current approach (model averaging) will prove too complicated to apply. This will lead to backtracking to a simpler single likelihood model which will have to be selected.

An improvement to the estimates (and elimination of the arguments for several models) may be obtained from releasing the constraint of independent population estimates. This makes the fitting problem larger, but only one model need be fit, and some reductions in the Hessian matrix might be possible and parameter precision will improve. Fits of 200+ parameters are routine in AD Modeller. In terms of transferring the information to the stock assessment, the CVs can be simply passed in a covariance matrix, rather than one CV for each estimate.

Appendix 1 STATEMENT OF WORK

Consulting Agreement Between The University of Miami and Dr. Paul Medley

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).

Abundance Review

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

[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.

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.

ANNEX I: REPORT GENERATION AND PROCEDURAL ITEMS

1. The report should be prefaced with an executive summary of comments and/or recommendations.

2. The main body of the report should consist of a background, description of review activities, summary of comments, and conclusions/recommendations.

3. The report should also include as separate appendices the bibliography of materials provided by the Center for Independent Experts and a copy of the statement of work.

4. Individuals shall be provided with an electronic version of a bibliography of background materials sent to all reviewers. Other material provided directly by the center must be added to the bibliography that can be returned as an appendix to the final report.

Appendix 2 Bibliography

Brandon, J., T. Gerrodette, W. Perryman and K. Cramer. 2002. Responsive movements and g(0) for target species of research vessel surveys in the eastern tropical Pacific Ocean. Administrative Report No. LJ-02-02, NMFS, Southwest Fisheries Science Center, 8604 La Jolla Shores Drive, La Jolla, CA 92037.

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Appendix 3 Editorial Suggestions

The following are changes suggested changes to the documents provided to improve readability.

REPORT OF THE SCIENTIFIC RESEARCH PROGRAM UNDER THE INTERNATIONAL DOLPHIN CONSERVATION PROGRAM ACT
FS_Master_11

Executive Summary

"In the 1950s, a fishery method was developed whereby fishermen looked for the surface-schooling dolphins to locate the tuna and use speed boats to herd the these dolphins into large purse seine nets."

could be corrected to

"In the 1950s, a fishery method was developed whereby fishermen looked for the surface-schooling dolphins to locate the tuna and then used speed boats to herd the dolphins into large purse seine nets."

Pg 4 Para 2 For "Current abundance estimates were derived from research vessel surveys conducted in the ETP during 1998, 1999, and 2000, using improved analytical methods for abundance estimation." Remove final "for abundance estimation".

Pg 4 Para 4 "… carrying capacity[2] is more likely to be affected by long-term (over decades) changes rather than those occurring short-term (interannual or seasonal)." Insert "over the" before "short-term".

Pg 8 Footnote 3 "Ninety-five percent of the time, the "true" value will fall within the stated confidence interval" might be better defined in Bayesian terms as "Ninety-five percent or 19:1 chance that the "true" value will fall within the stated interval."

APPENDIX 5 ABUNDANCE ESTIMATES FOR DEPLETED STOCKS

Pg 42 Para 1 (also Pg 17 Para 3)

"This method is the most widely used method for estimating the abundance of biological populations and belongs to a suite of techniques collectively referred to as "distance sampling," which has been used on various organisms, including trees, plants, insects, amphibians, reptiles, birds, fish, marine and land animals. In line-transect sampling, observers perform standardized visual surveys along a series of lines, searching for objects of interest (i.e. groups of dolphins) and recording each animal or group detected and its distance from the observer. From this information, the abundance of the animals in the survey area can be estimated, including the proportion of animals missed by the observers..…"

- could be simplified to -

"This is the most widely used method for estimating the abundance of biological populations and has been used for the assessment of many organisms, including trees, plants, insects, amphibians, reptiles, birds, fish, marine and land **mammals**. In line-transect sampling, observers move along a series of lines (i.e. transects), looking for the objects of interest and recording each animal or group detected and its distance from the observer. From this information, the abundance of the animals in the survey area can be estimated. The method automatically accounts for the proportion of animals missed by the observers.”.

ESTIMATES OF ABUNDANCE OF NORTHEASTERN OFFSHORE SPOTTED, COASTAL SPOTTED, AND EASTERN SPINNER DOLPHINS IN THE EASTERN TROPICAL PACIFIC OCEAN LJ_02_06

I would suggest replacing “/year” by “$year^{-1}$” or “per year” to make the text more readable.

Pg 4 Para 2 “Within each stratum, transect lines were randomly placed to achieve uniform spatial coverage.” doesn’t make sense. Perhaps should read “The vessels’ tracks were random within each stratum, with the constraint that transect coverage would be as even as possible.”

Pg 7 “For consistency we used the half-normal model in each year, with sightings truncated at 5.5km.” should perhaps read “For consistency we used only the half-normal model, which was the best overall detection model, with sightings truncated at 5.5km.

Pg 11 Para 3. “Weighted first-order linear regressions indicated…” would perhaps be clearer as “Linear weighted least-squares regressions indicated…” . “Second-order (quadratic) regressions indicated a concave-upward curve for northeastern offshore spotted dolphins,…” would perhaps be clearer with “Quadratic (curved) regressions indicated a concave-upward curve for northeastern offshore spotted dolphins,…”.

Appendix 4 Background material provided by the CIE

Medley, P. 2001. Review of Transect Sampling Methods to Obtain Population Size Estimates for Northeastern Offshore Spotted and Eastern Spinner Dolphins. CIE. Miami.

Mohn, R. 2001. Review of methods to estimate dolphin abundance from line transect surveys and presentation at Southwest Fisheries Science Center (SWFC), La Jolla, California 15-17 Oct. 2001. CIE. Miami.

1. IDCPA research program:

SWFSC. 2002. Report of the overall IDCPA research program and results.

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Brandon, J., T. Gerrodette, W. Perryman and K. Cramer. 2002. Responsive movements and g(0) for target species of research vessel surveys in the eastern tropical Pacific Ocean. Administrative Report No. LJ-02-02, NMFS, Southwest Fisheries Science Center, 8604 La Jolla Shores Drive, La Jolla, CA 92037.

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Escorza-Trevino, S., A. Lang and A. E. Dizon. 2002. Genetic differentiation and intraspecific structure of eastern tropical Pacific spotted dolphins, *Stenella attenuata*, revealed by mitochondrial and microsatellite DNA analyses. Administrative Report No. LJ-02-38, NMFS, Southwest Fisheries Science Center, 8604 La Jolla Shores Drive, La Jolla, CA 92037.