INTER-AMERICAN TROPICAL TUNA COMMISSION
COMISION INTERAMERICANA DEL ATUN TROPICAL
Scripps Institution of Oceanography, 8604 La Jolla Shores Drive, La Jolla, CA 92037-1508
Tel: (619) 546-7100 – Fax: (619) 546-7133 - Director: James Joseph, Ph.D.

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Dr. Michael F. Tillman
Science Director
Southwest Fisheries Science Center
P.O. Box 271
La Jolla, California 92038-0271

Dear Mike:

I am writing in reference to the meeting held on 16-18 December to provide the Commission's view on the topics which were discussed, including the survey, its analysis, and the associated framework for decision analysis. I take the meeting to be part of the consultation with the IATTC concerning the study of the effect of intentional encirclement, which addresses the question as to whether encirclement of dolphins is having a significant adverse impact on any depleted dolphin stock in the eastern tropical Pacific Ocean, as required by section 304 of the Marine Mammal Protection Act. The decision analysis is an integral part of the study, as it influences the data analysis and, in particular, specifies the propositions to be tested.

First, I have views on the propositions to be answered by the abundance survey. Dr. Goodman's notes of 14 December from the meetings of September and November 1998, proposed two questions to be answered at unspecified levels of probability.
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1. Does the evidence indicate with a probability X% or greater that the recent tuna fishery practice of setting on dolphin and encircling dolphin schools, as done during the period since 1991, caused a reduction in survival and reproduction in any depleted dolphin stock greater than or equivalent to an annual removal of a number of individuals corresponding to 0.4% of the estimate of N_{min} for that stock?
2. Does the evidence indicate, with a probability Y% or greater, that the recent tuna fishery practice of setting on dolphin and encircling dolphin schools, as done during the period since 1991, caused a reduction in survival and reproduction of great magnitude enough to prevent the recovery of any depleted dolphin stock?"

Dr. Goodman noted that the levels X and Y are policy choices, and that those who attended a previous meeting suggested 50% and 10% respectively.

At the meeting on 16-18 December three similar propositions with proposed probability levels were suggested.

I. Probability ($M+U > r_{max}$) exceeds 1% to 5%, depending on stock. M = reported kill and U = shadow kill, both as fractions of stock size; r_{max} is maximum intrinsic rate of growth.
II. Probability ($M+U > 0.1*r_{max}*N_{min}/N$) exceeds 50%.
III. An intermediate condition between I and II.

The statements in proposition 2 of Dr. Goodman's report (is there a reduction in survival and reproduction great enough to prevent stock recovery?), and the first proposition discussed at the meeting of the 16-18 December (does M+U exceed r_{max}?) are reasonable descriptions of adverse impact, though, of course, not necessarily caused by the encirclement of dolphins.

The finding that the Secretary of Commerce has to make is whether there is a significant adverse impact on any depleted dolphin stock. It is, of course, up to the Secretary to determine what burden of proof he requires for such a statement. If the decision analysis is intended to be a guide for that, I believe it should be at least on the balance of probabilities, that, is with probability more than 50%. Statistical hypotheses are normally tested with probabilities of 95% or 99% to minimize the possibility of drawing false conclusions. Thus Dr. Goodman's proposition 2 with a conventional probability level Y, rather than the 10% suggested at the previous meeting, would be a useful test.

The probability levels proposed for the first proposition from the meeting of 16-18 December are not appropriate in terms of the question posed in the MMPA. Using a probability of the statement being true of 1% or 5% translates in plain English or statistical convention to "can we be reasonably certain that there is no adverse impact?" If that were the intent of Congress it would have been written that way. Given the lack of precision of the survey data, the answer to such a question would almost certainly have to be no. I believe this proposition would be consistent with the standard in the Act with a higher conventional probability level.

I am not convinced that Proposition 2 in Dr. Goodman's report provides a reasonable test of significant adverse impact. The statement is that there is "a reduction in survival and reproduction greater than or equivalent to an annual removal of a number of individuals corresponding to 0.4% of the estimate of N_{min} for that stock". N_{min} is a quantity designed for conservative management of a stock, not the estimate of a stock size. The net reproduction of the stock is likely to be of the order of a few percent of the actual stock size, which, in turn, is likely to be greater than N_{min}. Thus $0.4\%N_{min}$ is likely to be considerably lower than the net reproduction. In my view, a reduction in survival and reproduction of that extent would not constitute a significant adverse impact on any depleted stock, so this proposition should not be used.

It is more difficult to comment on whether the statements in the second or third propositions discussed in the meeting are reasonable statements of adverse impact. The second statement addresses the possibility of there being a slight decrease in survival and/or reproduction, but it is not clear that this would correspond to a significant adverse impact. For that reason, I suggest it not be used. The third statement is a hybrid between the first and second, and I also suggest you do not use that one.

I also wish to comment on the likely difficulties of attributing any estimates from the analysis to the effect of intentional encirclement of dolphin schools. There are many plausible alternatives, including:

a) other species, such as tunas and billfishes, may have occupied niches vacated by dolphins, constraining any potential recovery;
b) lack of precision and high variance normally associated with fishery survey data, particularly for analysis of an effect postulated to occur over just 5 years and for a slow-growing, long-lived species;
c) natural variation in survival and reproduction;
d) lack of definitive cause-effect from planned stress and genetic studies;
e) ecosystem effects of large changes in environment, and abundance of tunas and other species over the period of data collection and over the recent period of interest;
f) bias resulting from literal interpretation the fishery-based tuna vessel observer data;
g) bias due to model assumptions, particularly reliance on simple deterministic models with assumed parameterizations for density dependence;
h) assumptions about the prior distributions of model quantities;
i) possible bias because the survey in 1998 followed a major El Nino event, after which the population distribution and group structure may have been atypical;
j) possible bias after 1992 in the fishery-based tuna vessel observer data due to a shift of the tuna fleet toward more dolphin-safe trips.

The significance of some of these points will be easier to evaluate when the results of the survey and the precision of the estimates are available. However, at this stage it is possible, with very simple calculations, to demonstrate the difficulty I expect in drawing conclusions. The coefficients of variation of the estimates of dolphin abundance are quite high. For example, estimates of abundance for the NE spotted dolphin population measured in the 1986-91 surveys had coefficients of variation of about 32% annually or 14% for the mean over the five years. If the actual growth rate of one of the depleted stocks was, say, 30% over ten years and the 1998 survey had a coefficient of variation of 30%, then the 95% confidence intervals for the estimated growth obtained by comparing the 1998 and 1986-1991 surveys would be about (-30%, 150%).

Dr Allen's letter of 15 July 1998 referred to some of the expected difficulties in estimating the impact on population growth rates. It is still our view that the analysis should include a comprehensive evaluation of alternatives to the model used. I have extracted the part of that letter relevant to this issue below.

> *In practice the analysis is likely to be sensitive to the details of any assumptions concerning the nature of parameters (for example if r-max is treated as a constant or allowed to vary over time) and their prior distributions. I believe it is of utmost importance to examine the effects of a wide range of plausible alternatives to ensure that the effects of initial assumptions on the results are well understood. To facilitate this it would be useful to have a discussion of the details of the method prior to as well as after the analysis.*

I think that there should be particular care in looking at the effect of environmental variation between the years of the earlier surveys and 1998. At the 16-18 December meeting it was stated that the 1998 conditions were within the range of those observed during 1986-91. However, you will recall that the estimates of abundance for each of the surveys in those years varied greatly. I think conclusions could be drawn only after looking at the environmental conditions and estimates of abundance in a detailed way, year by year.

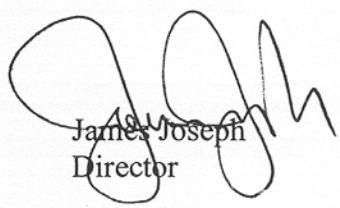
Finally, I would like to comment about two issues concerning the use of the relative estimates of abundance from the observer data.

We believe the observations from tuna vessels miss a large portion of the juvenile population. NMFS first saw a noticeable dearth of spotted dolphins in age classes 5 to 15 among the dolphins killed in the fishery during the 1973-81period. This gap persisted over time indicating it was not likely to be due to a change in recruitment. It appears that individuals in these age groups are less vulnerable to the fishery, and less likely to be sighted by tuna vessel observers. Consequently tuna vessel observations may miss the part of the population where the growth is occurring. Thus, if the tuna vessel observer data are used in an estimation procedure, selectivity should be taken into account.

We have started an examination of the estimation of relative abundance estimates based on observer data. We are concerned about some trends in these data, particularly during the last five years, which appear to indicate some changes in the sampling conditions. Given this, we believe that the estimates should not be used to make comparisons between early years and those after 1992 until these trends are understood.

I trust these comments are useful, and look forward to any responses you might have.

Yours truly,



James Joseph
Director

CC: Commissioners