# A Review of a Proposal for Long-term Research on Small Cetaceans in the Eastern Tropical Pacific

Prepared for the Center for Independent Experts, University of Miami

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Executive summary

In general, the proposal is very highly graded science both for its focus upon the practical problem of tuna/dolphin/fisheries interactions in the ETP and for its contribution to the field in general. The proposal is ambitious overall and proposes to use state-of-the-art methods. Although the individuals involved and their skills are not even mentioned, I am aware that this comes from a world-leading team. It will probably be the globally leading program of research on cetacean ecology.

Overall, I thought the proposal was underpinned by some excellent science and some excellent people but it was difficult to be convinced by this in the presentation. One had to use a lot of background knowledge to appreciate the academic background of the proposal as this was not made clear within the proposal. Several major parts of the proposal were missing. For example, there was no linkage between planned research and the current skills base at the SWFSC and there was no serious attempt at suggesting how this program might be implemented. If the purpose of the proposal was to provide a flavour of the science to be undertaken over the next 5-10 years then it fulfilled its purpose but, in its current form, it is not a document that could be used to provide a foundation for funding with the purpose of delivery towards milestones and strategic targets.

Specific criticisms and recommendations include:

1. The proposal itself makes no clear statements about the ultimate objectives of the research and I recommend that this should be rectified by ensuring that the strategic context of the proposal is articulated properly. It is also very vague about time scales.

2. To this end, a mapping exercise could be undertaken to link the proposed research effort onto the research needs and that this should be used to show how research effort is prioritised and where there are research gaps.

3. The program is composed of two types of project, those measuring the state of the system and how it is changing and those involved in understanding the mechanisms that are governing the dynamics of the system states. In general, the rationale for the former is more mature, and better justified, than for the latter type of study.

4. The most appropriate rationale for the proposed studies of the mechanisms governing the state of the dolphin/tuna system is to adequately address uncertainty but with the caveat that more knowledge of this type may actually increase uncertainty as past assumptions are shown to be false, e.g. about population definitions.

5. Research to understand mechanisms is necessarily of a more speculative nature and I recommend that, to achieve highest quality, it should be funded under an open Call for Proposals

6. I recommend that a project should be included that examines bycaught dolphins for signs of long-term pathologies that could be associated with capture events.

7. In some areas, particularly ecosystem biology, there appears to be a need for some thinking to be done "out of the box" to start making genuine progress. The current proposal is slightly unimaginative in this respect.

7. Insufficient information was provided to assess whether the budget was appropriate. Under certain assumption, the budget would appear to be completely inappropriate to the task.

8. It was difficult to judge whether this program was achievable as an integrated set of projects. Some information was provided with most projects about the time scale but it is impossible to judge from the proposal if the time scales from different components fit together and whether the project is feasible in terms of the resource allocation and, importantly, the key skills required for delivery.

Basis of the review

This review is a broad-brush approach. The reviewer has related experience in each of the main research themes but has particular experience of organising research at a strategic level. Therefore, the review will not focus on one particular part of the proposal but will deal with the proposal as a whole. As part of this review I have taken into consideration background papers detailed in Appendix I and one of which was considered in more detail within Appendix II. The Statement of Work for this review is provided in Appendix III.

Background to the review

The tuna-dolphin complex is a well-known ecological interaction in the Eastern Tropical Pacific that has been exploited over several decades by commercial tuna fisheries. The presence of dolphins provides a guide to the location of tuna and consequently when the fishery sets its nets on the dolphins large numbers of dolphins can be captured as a result. In the distant past, this led to large numbers of dolphins being killed and substantial reductions in the population size of dolphins. Technological innovations and improved methodologies and experience has resulted in very large reductions in the number of dolphins killed as a result of these fishing

activities. However, these reductions in bycatch have not led apparently to a recovery of the dolphin populations as might have been expected within the current time scales.

The Southwest Fisheries Science Center of NMFS has made the study of the tuna-dolphin-fisheries complex a central part of its research. It has used this successfully to build much of its very strong reputation in cetacean biology with a particular emphasis upon abundance estimation methods but also including ecological studies. The present proposal is a description of the way that the Center wishes to pursue its research on a 5-10 year time horizon.

Specific points addressed

(a) Is the scope of the proposal adequate and appropriate?

There is a clear set of challenges arising from the previous round of research reported in Reilly et al. (2005). In broad terms, these relate to the apparent lack of recovery of dolphin populations in the ETP following mitigation of direct mortality caused by fisheries targeting yellowfin tuna setting nets directly on dolphins. The main emerging issues can be summarised as:

(i) Uncertainty about the detectability, or expectation, of a recovery in abundance given the current variance in population estimates (see comments in Appendix II relating to interpretations of this in Reilly et al. 2005) and the range of assumptions about population dynamics. In particular, this could include:
   a. Whether there is sufficient statistic power in the data to detect the kinds of trends one might expect in the recovering populations;
   b. The expectation that there could be time-lags in recovery because of the way in which the social/community structure has been altered as a result of the extraction of individuals by the tuna fishery.

(ii) Uncertainty about whether there is mortality additional to that observed and recorded. In particular, this could include:
   a. Some form of traumatic injury which may not be evenly distributed amongst individuals of the population and could be cumulative in its impact on individuals;
   b. Separation of nursing calves from their mothers;
   c. Disruption to social structure and/or multi-species aggregations either through direct effects caused by interference or because of indirect effects of the fishery on the dynamics of these multi-species aggregations;
   d. Lack of information from some parts of the fishery, particularly Class 6 vessels.

(iii) Uncertainty about the identity of some population segments, particularly the near-shore populations of spotted dolphins. This could include:
   a. The extent to which continued studies of the genetics of these populations will reveal a highly complex set of populations some of which may show range overlap.
   b. The extent to which some of the current “stocks” could, or should, be considered as metapopulations.

Notably, the previous work made a strong case that large-scale ecosystem effects were unlikely to be the cause of the lack of recovery (Dower 2002; Drinkwater 2002). Although, of course, it is impossible to eliminate such a factor completely, any broad categorization of priorities for future research would probably indicate that ecosystem change is at the low priority end of a spectrum of research defined by the set of uncertainties given above. Drinkwater (2002) suggested that there should be some clarification of the definition of carrying capacity, presumably through some form of measurement, for example of the prey field.

This assessment is based upon the principle that the ultimate objectives of the long-term program of research will be to

- advise on the development and implementation of policy with respect to the management of the yellowfin tuna fishery and the dolphin population in the ETP;
- maintain a long term time-series of observation in the ETP that has general scientific value beyond that required by policy;
- enhance our understanding of the ecology of the upper trophic levels of the ETP complex.

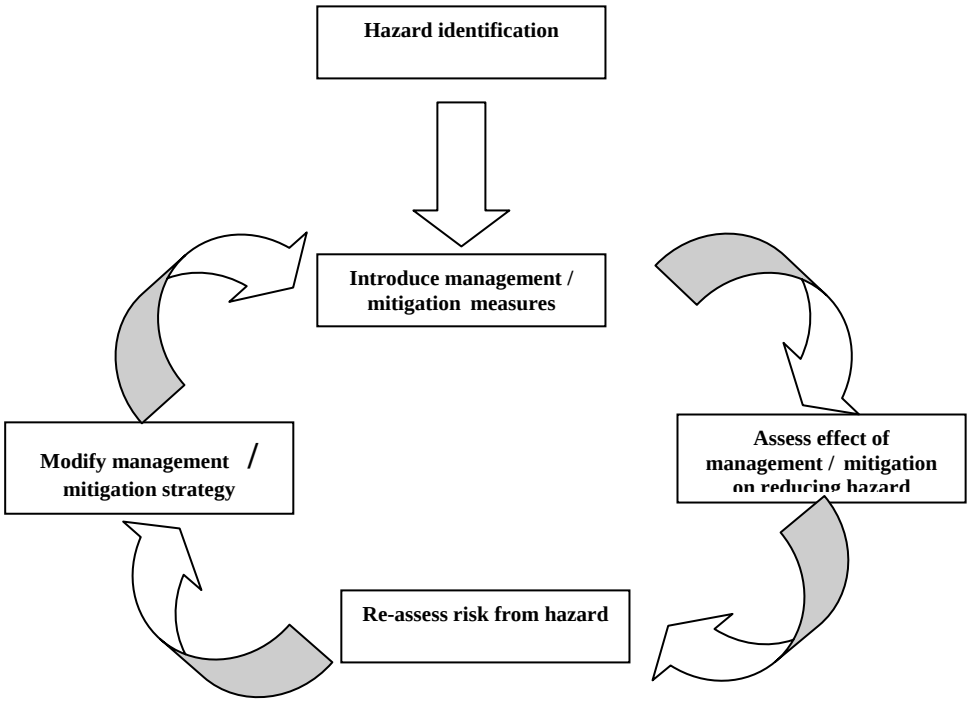
To this end, this assessment assumes that the second and third bullets above are consequential on the first and that, with respect to the issues of dolphin/tuna management, a risk framework is being applied[1]. Such a framework is often applied in these circumstances but, as in the case of the present proposal, it is implicit rather than explicit. There is some evidence that a risk framework is being used here but the general lack of a structured, strategic rationale for the research within the proposal leaves the reader without a strong set of reference points from which to understand how the various segments of the research converge towards a common outcome.

**The proposal itself makes no clear statements about the ultimate objectives of the research and I recommend that this should be rectified.** In particular, it is important to provide guidance about the interaction of the objectives in the three bullet points given above. To my mind the second and third bullet points clearly underpin the first but the way in which priorities are set for research in the second and third bullets depends on their relevance to the first.

The research programme as it is currently written makes no effort to place the research in this type of context. Only by so doing is it possible to map research effort onto research needs. **I recommend that a mapping exercise should be undertaken to link the proposed research effort onto the research needs and that this should be used to show how research effort is prioritised and where there are research gaps, if any.**

In summary, I am assuming that the strategic process being followed is summarised I the following diagram:

[1] See "Science and Judgment in Risk Assessment" (1994, Commission on Life Sciences, National Academy Press). Normally this involves 5 steps: (1) Hazard identification; (2) Dose-response assessment; (3) Exposure assessment; (4) Risk characterisation; (5) Mitigation. Normally this is undertaken as an iterative process, otherwise known as adaptive management, in which the success of mitigation actions is examined by assessing the extent to which the mitigation has modified the risk.




The hazards in this case have been defined as of two types:

- those associated with the yellowfin tuna fishery
- those associated with long-term ecosystem change
- those associated with scientific uncertainty

The previous research (Reilly et al 2005) has suggested the second of these, long-term ecosystem change, has a comparatively low risk of being a driver of the lack of recovery. However, it did highlight substantial areas in which there is scientific uncertainty, for example the time it might take unknown effects involving disruption to the social structure or Allee effects to dissipate. It also emphasised that even apparently low levels of mortality caused by the fishery could result in a lack of recovery and that there were difficulties associated with detecting these effects.

The 2002 program of research (Reilly et al. 2005), as well as previous activities relating to the dolphin/tuna problem has already taken us around the loop shown in the diagram above on at least two occasions. I suggest that a major part of the rationale for the current proposal should be to be to carry out a further assessment of the effectiveness of management/mitigation and a re-assessment of risk in the light of new knowledge and through targeted research.

I am aware that I may be placing a slant on the proposal that the authors had not intended but, in the absence of a rationale of this type, I am seeking to find a framework within which to make a judgement. Moreover, it is not even clear over what time period this program will be active. 5-10 years appears to be about as precise as it gets. Presumably funds will not be made available on an open-ended basis?

Returning to the original question, the scope of the proposal is very broad indeed but, without the type of mapping exercise suggested here, it is difficult to assess whether it

is *adequate*. However, the proposal is generally ambitious and proposes to use state-of-the-art methods. It will probably be the globally leading program of research on cetacean ecology. Notwithstanding the caveats given above about presentation, the program seems to be *appropriate* to the extent that it is addressing an important management and policy issue (although whether it is *adequate* for this purpose is not so clear) and it is likely to have important spin-offs in terms of our general understanding of marine ecology. However, see later comments about the appropriateness of the resources being applied to the project.

(b) Are any areas addressed in less, or more, depth than needed?

Until the strategic context is fleshed out in more detail (as described above) it is difficult to judge whether more or less depth is required in different areas. However, there would appear to be a natural ranking of research priorities. These are built around two different approaches to addressing the problem of dolphin/tuna interactions:

(i) Measuring the state of the system and how it is changing
(ii) Understanding the mechanisms that are governing the dynamics of the system states

The first of these is an absolute necessity and it includes such information as the 3-yearly population assessments, the identification and quantification of population segmentation and the mortality imposed by the fishery. It is principally this information that policy judgements will be built upon.

The second is the part of the work that contains the more interesting science but it is necessarily much more speculative and, in some cases, there are not the methods to properly address some of the questions. I thought, for example, that the "Process cruise to elucidate mechanisms underlying cetacean-habitat relationships" (Page 118) struggled with a very difficult topic and came across as unfocussed and likely very expensive for what will probably turn out to be a number of fairly inconclusive relationships. This is less of a criticism and more of an observation because I appreciate that science has to tackle difficult problems, but this type of project carries a high risk of not succeeding. On page 118 it is stated that "Despite conducting more than twenty dolphin abundance surveys in the ETP over the last thirty years, of which all since 1986 have included ecosystem studies …, we still know very little about how dolphins exploit prey." One can read this in two ways, either that there is still a grand challenge there or that we have failed repeatedly to make progress in this field. My view of the remainder of this section was that it failed to convince me that there had been an advance in technology or theory sufficient to assure me that the proposed work would not end up once again in failure. Perhaps the approach needs to be re-thought because simply throwing more resources at the problem without a significant change in approach may not be wise.

The definition of carrying capacity (Drinkwater 2002) is central to the ecosystem studies and, while this is extremely difficult (see current and past research on Steller sea lions as an example which has failed in spite of massive effort), the proposal needs to be realistic about its prospects of making genuine progress in this field. I have a feeling that that simply applying more of the same methodology and collecting

more and more data to feed unrealistic models of non-linear complex systems is not the answer. I'd really have liked to see something more imaginative here. Even something very speculative would be acceptable so long as it was genuinely attempting to break a mold that really is taking us nowhere in terms of understanding and predicting ecosystem processes.

Underlying the second challenge above is an implicit belief that dissecting out detailed mechanisms can lead to an enlightenment that leads to predictive power. I am not so sure. Again, while I do feel that we need to go on doing this kind of research, it has to be measured carefully against the gains and objectives. Understanding mechanisms probably has greatest utility in helping properly to address uncertainty. Certainly, if it could be properly quantified, the bounds of uncertainty in advice given in the policy arena might well get larger through time, particularly as we develop a better understanding of the complexities of the system. **Therefore, I suggest the most appropriate rationale for the proposed studies of the mechanisms governing the state of the dolphin/tuna system is to adequately address uncertainty but with the caveat that more knowledge of this type may actually increase uncertainty as past assumptions are shown to be false.** For example, I suspect that studies of population structure will simply add another layer of complexity (and, with it, uncertainty) to population models where, in our historical ignorance, we simply assumed we were dealing with panmictic populations. Much the same could be said for studies that provide individual-based information. These will run a coach and horses through our historical assumptions in population models that all individuals are the same.

The proposal mixes up the two different types of science detailed above. Although there are advantages to doing this to help create an integrated approach, I suggest that there are disadvantages because different components of the science have highly variable risk-reward trade-offs and there needs to be some moderation between these so that if one high risk piece of science fails to materialise this does not jeopardise essential studies measuring the current state of the system.

Overall priority gradings for each of the major components of the proposal based upon this rationale might be:

| **Assessment of each project. H = high; M = moderate; L = low.**<br><br>**Project title** | **Practicality** | **Risk** | **Reward** | **Priority** |
|---|---|---|---|---|
| **Assessments of Status and Trends** | **H** | **L** | **M** | **H** |
| **Abundance and monitoring of dolphin populations** | **H** | **L** | **H** | **H** |
| **Cetacean Stock Structure in eastern tropical Pacific (ETP) pelagic waters** | **M** | **M** | **H** | **H** |
| **Monitoring and assessment of coastal stocks** | **M** | **M** | **M** | **H** |
| **Ecosystem Studies** | **L** | **H** | **L** | **M** |
| **Fishery Effects** | | | | |
| **Fishery Exposure** | | | | |
| *Exposure index* | **M** | **L** | **H** | **H** |

| | | | | |
|---|---|---|---|---|
| *Chase and set frequency* | **H** | **L** | **H** | **H** |
| *PIT tagging of ETP dolphins* | **L** | **H** | **H** | **M** |
| **Life History** | | | | |
| *Updating parameter estimates* | **M** | **L** | **M** | **M** |
| *Evaluating interpretation of age distributions* | **L** | **L** | **L** | **L** |
| *Estimating fetal mortality* | **M** | **L** | **M** | **M** |
| *Estimating vital rates from aerial photogrammetry* | **M** | **M** | **H** | **H** |
| *Estimating vital rates via blubber steroid composition* | **M** | **M** | **M** | **M** |
| *Evaluating relative stress level via blubber steroid concentration* | **L** | **H** | **L** | **L** |
| **Physiology** | | | | |
| *Drafting hydrodynamics and kinematics* | **M** | **M** | **L** | **L** |
| *Analysis of data collected from ETP dolphins* | **M** | **L** | **H** | **H** |
| **Behavior** | | | | |
| *Behavioral responses to chase* | **L** | **H** | **H** | **H** |
| *Sociality and social disruption* | **L** | **H** | **H** | **H** |
| Mating behaviour | **L** | **H** | **L** | **L** |
| **Purse-seine fishing on dolphins by Class 5 vessels** | **H** | **L** | **H** | **H** |
| **ETP Purse-Seine Bycatch Reduction** | **M** | **L** | **H** | **H** |
| **Data Management** | **H** | **L** | **M** | **H** |

The following analysis expands upon the scores provided in the table.

## Assessments of Status and Trends

- Use state-of-the-art modelling approaches
- Focus on uncertainty – but see the discussion above about the problems attached to quantifying uncertainty and bias – models are only as good as the estimates of uncertainty they portray
- Tests the relative weight of competing hypotheses and combines varied data towards testing hyptheses
- Focussed on management and likely to be a major tool for providing management advice in future
- Model error is a concern: good that different independent model structures are to be considered
- Model selection will be an issue as will search routines for the parameter space if many parameters are involved as seems likely (see McAllister 2002 for an indication that there are no absolute ways to set up these models and, despite methodologies to help guide interpretation, the complexities involved in interpretation and the small pool of expertise available to provide appropriate interpretations, is a significant problem)
- Authors should be prepared to be disappointed about the capacity of the data to provide anything other than very broad predictions

## Abundance and monitoring of dolphin populations

- Essential source of basic information about population
- Survey design is probably the best there is

- Testing and potentially improving methodology is important although modelling different distributions will be necessary before taking a “process” cruise into the field
- Studies of echosounder effects may have particularly broad relevance
- Detection of food web changes using archived tissue and new sampling seem important but must be aware of changing sample bias
- Section on analysis of cetacean life history (P137) seems out of place within this section

**Cetacean Stock Structure in eastern tropical Pacific (ETP) pelagic waters**

- Strong justification for this work but rationale based upon ecological segregation is weak because this is not addressed and would be very difficult to study
- Not clear how these results will be introduced into population assessment models

**Monitoring and assessment of coastal stocks**

- Not clear why this is a separate project from the above
- Objectives probably too broad and nebulous; need to prioritise
- Ecological interactions studies of higher risk because of inherent difficulties with sampling the prey field
- Otherwise an important project

**Ecosystem Studies**

- Already well researched in terms of large scale effects and little supporting evidence forthcoming
- Main justification is that data can be collected alongside other uses of logistics in high priority projects keeping costs down
- Objectives seem nebulous and along the lines of measure everything that time and technology will allow in the hope of finding relationships
- Process cruise needs greater focus on specific questions
- Modelling – multi-species minimum realistic approaches and ECOSIM approaches are very experimental (although not usually recognised as such) and should probably be classified as an academic exercise.
- The logic underlying the choice of the set of questions on P122 is unclear
- Models need to be tested against data through a fitting process – not clear how this can be done

**Fishery Effects**

Fishery Exposure

*Exposure index*

- Important to quantify exposure as part of the overall risk assessment

*Chase and set frequency*

- This would seem to be important information as part of the process to characterise the hazard

*PIT tagging of ETP dolphins*

- Its difficult to be convinced that PIT tags will work in these circumstances – PIT tag performance is always well below spec when applied in the field
- PIT tags are only useful if there are other methods of detecting marked individuals because of false negatives
- Rewards will be high if this can be made to work

Life History

*Updating parameter estimates*

- Important work and important to include populations/samples not yet analysed
- Must be mindful of the practical limits to refinement of parameter estimates because of inherent biases in the sampling process
- Therefore there is probably a diminishing return, in terms of narrowing model uncertainty, for effort invested in this area
- A measured approach is required to investment in this – there is a tendency to want more data on this but how useful will it be if effort can be applied to more tractable but equally pressing problems?

*Evaluating interpretation of age distributions*

- As with the previous project, returns on investment may not lead to significantly reduced model uncertainty unless some form of new method is available to significantly advance the field

*Estimating fetal mortality*

- Not a large investment for a reasonable return and important if pregnancy data are being used in estimation of vital rates

*Estimating vital rates from aerial photogrammetry*

- Good to see an alternative method for determining vital rates from analysis of bycatch
- Non-invasive so likely to be less biased
- May be challenging to make it work but certainly worth trying

*Estimating vital rates via blubber steroid composition*

- Group has done much to develop methods but invasive nature may lead to biases in the population sample
- Need to ensure appropriate assessment of false positives/negatives

*Evaluating relative stress level via blubber steroid concentration*

- More problematic than using sex steroids because stress is a relative effect
- Validation and calibration may be a problem
- Are these steroids really reliable for measuring stress
- The only unstressed animal is a dead one – definition of stress is very subjective. Not sure this is going to help much
- High risk because the method may not work

Physiology

*Drafting hydrodynamics and kinematics*

- Study of fundamental principles that may provide insights
- Already well-known field
- Doubtful if more detailed studies will lead to strong conclusions about effects – mother-calf separation is separation whatever the cause

*Analysis of data collected from ETP dolphins*

- Fairly peripheral to the main objectives of the study
- Not clear how this builds into useful management advice

Behavior

*Behavioral responses to chase*

- Important area for understanding the potential effects
- Making well controlled behavioral measurements will be difficult using classical behaviour methodologies

*Sociality and social disruption*

- Very difficult to make progress in this important area
- Proposal seems a little short of ideas

*Mating behavior*

- Testis and sperm morphology studies seem very peripheral to the objectives, even bizarre in this context

- Mating behavior needs behavioral not morphological study
- This is a subset of *sociality and social disruption*

Purse-seine fishing on dolphins by Class 5 vessels

- It would seem to be essential to plug this gap in knowledge

**ETP Purse-Seine Bycatch Reduction**

- Would appear to be essential

**Data Management**

- Always a high priority for any integrated research program
- Should be a cross-cutting background activity in every project

**Summary budget**

This seems to be a pretty unrealistic budget for the work described in this research program in that it is a small fraction of what would be required to complete the program.

Perhaps I am misinterpreting the basis of the budget? Are these simply the marginal costs or are they the full economic costs? If the latter then I cannot see how the programme can be completed for this amount. Even if these are the marginal costs then it would still be a stretch.

Or, is this the annual budget? On this basis, the costings are realistic but its still not clear if these are the marginal costs or FEC. I suspect they are most realistic if they are and annualised FEC, but this is a strange way to present a costing for a program of research.

(c) Are the approaches proposed unbiased?

This question can be approached from two angles. One “are the approaches unbiased statistically” and “are the approaches unbiased towards different stakeholder interests”.

Are the approaches unbiased statistically?

Almost certainly not, although the extent of bias will depend upon the specific piece of work being carried out. It is nearly impossible to carry out unbiased population sampling of the type described in many of the projects being suggested in this proposal. Quantification of biases is often a major component of the research and perhaps there could be both a greater awareness of these biases throughout the proposed research and, having raised awareness, concentration upon the quantification of these.

Are the approaches unbiased towards different stakeholder interests?

This is difficult to assess without knowing those interests. However, the science suggested within this program is generally what one would expect given the range of problems to be solved. I have no evidence to suggest that the program is anything other than an honest attempt to undertake science that will provide the basis for advice concerning policy development and for public consumption.

(d) Does the proposal represent Best Available Science? If not, what specifically would be required to meet that designation, in your opinion?

In general, the proposal is very highly graded science both for its focus upon the practical problem of tuna/dolphin/fisheries interactions in the ETP and for its contribution to the field in general.

However, the proposal has strengths and weaknesses and these are tabulated above. In general, I found the rationale for some of the process-related science (mainly ecosystem science and some of the physiology) to be weak. This does not undermine the case for conducting science of this type but it does not seem appropriate for this to be funded from a budget for applied science. **If this type of speculative science is to be funded, I recommend that it should be funded under an open Call for Proposals**. Although this may already be the intention (there is no indication in the proposal of how this program is to be implemented), this is likely to be the only way of ensuring that the science contributing to these parts of this strategic program is the "best available".

(e) Comment on the proposal's strengths and weaknesses, and suggest any additional lines of research that appear promising.

Strengths and weaknesses have already been highlighted in the tabulation above.

One gap in the proposed science appears to be concerned with the pathology associated with repeated dolphin captures in seine nets. This was discussed by Reilly et al (2005) and has been heavily researched in the past through the CHESS studies (Martineau 2002; Ortiz 2002). Although the process of the development of pathology is interesting, it is its capacity to inform about possible long-term effects that is mainly of interest. I suggest that this is of more immediate importance than some of the physiology that has been proposed. **I recommend that a project should be included that examines bycaught dolphins for signs of long-term pathologies that could be associated with capture events.** For example, could life history analysis of tooth growth be used to examine these effects? Are there different susceptibilities amongst individuals? Whatever, the reasons are for not including continuation of pathology studies they should be articulated because this would appear to involve a change in strategic direction. However, it would seem unsatisfactory not to follow them up in some way even in an attempt to quantitatively link the past effort in this field into the management models being proposed.

(f) Overall, are the individual sections well integrated into the proposal as a whole? If not, what could be done to improve integration?

Notwithstanding the general lack of an overall strategic rationale showing how the different components of this program are integrated, the program does seem to hang together reasonably well. However, it is left to the reader, and his/her own experience, to extract this integration from the proposal, i.e. it was implicit rather than explicit. This may be the reason that I have come to the conclusion that some sections of the proposal are of lower priority.

Overall, I found it difficult to judge whether this program was achievable as an integrated set of projects. Some information was provided with most projects about the time scale but it is impossible to judge from the proposal if the time scales from different components fit together and whether the project is feasible in terms of the resource allocation and, importantly, the key skills required for delivery.

## Conclusions

The proposal presents a case for a substantial body of research. The work is generally very strong and is built on a formidable track record but there are also weaknesses. In general it shows a strongly applied rationale concerned with providing information for improvements in fisheries management. In areas where a large investment has been made in the past the rationale has not been developed to show why further research should, or should not, be carried out. Some suggestions are made about how that rationale could be developed. I recommend that there needs to be an overarching strategic assessment of research requirements against needs and that the current proposal should be revised to provide this assessment as a new introduction. The assessment should be written in a way that provides confidence that the research proposed is properly prioritised. This includes discussing areas that are not going to attract investment and articulating the rationale for this decision. The proposal then needs to conclude with a section showing how the research it is going to be resourced (both financial and human) and implemented. In its current form the proposal provides insufficient information as to whether the research is actually practical and, therefore, if the science outcomes can actually be delivered.

Appendix I

**Bibliography of material provided for this review**

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Appendix II

**Issues raised concerning the 2002 report (Reilly et al. 2005)**

Para 2, second sentence. Sentence structure could be improved. It is not the research effort that either aids of constrains recovery.

It would have been useful to have defined the geographical extent of the ETP at an early stage.

P10, summary. The quotation given is the percentage depletion. I might be worth showing the level of confidence around these percentage values.

P26. The number of additional mortalities per year is given. Does this relate to any age class and how does this compare with the lactation rate amongst adult females? With the current probability of a lactating female being involved in a chase, is the level of additional mortality plausibly explained by calf separation?

P27. Population growth rate. The ability to detect the kind of growth rates expected, which are low even at the best of times, is small. The case is made that the annual growth rate would need to exceed 2% to be detectable, but this presumably assumes that the CV on the population estimates is correct. What level of confidence is there in the CV? CVs are commonly misunderstood, even by those who create them and use them. Are these the CVs of the method or the CVs of the actual estimate? If they are the CVs of the estimate, what assumptions need to be upheld to make them robust and what is the sensitivity to assumption violation?

P29, para 2. It would be worth expanding briefly upon the “substantial problems” with the TVOD data. I can imagine what this might be but its worth saying a little more.

P29, para 3. There is an assumption that all individuals have the same probability of being set upon. It would be unusual if the vulnerabilities of this did not differ amongst individuals. A wide variety of factors could cause this. The point being made her may be particularly important. Non-linearities of effects could result is quite different behaviour from that predicted by the model. The authors are saying this but perhaps with insufficient strength or conviction.

P29, last para. The assessment model may have been equivocal but this may mean that the assumptions underlying the model are wrong, not that there is a possibility that substantial mortality in addition to that reported does not exist.

P30, para 2. Lags are possible, especially in socially-structured populations, because of Allee-type effects. It may take some time for effective social groups to re-form after key individuals have been removed in large numbers. This is mentioned on P32 but might also be mentioned here.

Appendix III

**Consulting agreement between the University of Miami and Ian Boyd**
**STATEMENT OF WORK**
**Eastern Tropical Pacific Dolphin Research Plan**

**Background**

The topic of the review is the evaluation of a long-term research plan to monitor the abundance and environment of several species of tropical pelagic dolphins that are killed in the purse seine tuna fishery of the eastern tropical Pacific (ETP), and the evaluation of reasons for the apparent lack of recovery of depleted stocks. The Southwest Fisheries Science Center (SWC) has been conducting research in the ETP since the 1960's. Research topics through the 1980's ranged from assessing direct dolphin mortality in the fishery to an examination of fundamental aspects of biology and life history, monitoring the numbers and types of dolphins being taken, conducting sighting surveys of dolphin abundance from ships to estimate abundance and trends over time, and collecting data and samples on a broad range of attributes of the physical and biological environment.

In a 1997 amendment to the Marine Mammal Protection Act, Congress directed the National Marine Fisheries Service to undertake a research program to determine, by the end of 2002, whether the fishery was having a "significant adverse impact" on depleted dolphin stocks in the ETP. The research program that the SWC designed included four components: abundance estimation, ecosystem studies, stress and other fishery effect studies, and stock assessment. This research culminated in a Final Science Report (FSR) in 2002 and thirty-four separate science papers to provide information for answering the question posed by Congress. The FSR contained the following primary conclusions: (1) northeastern offshore spotted dolphins were at 20% and eastern spinner dolphins at 35% of their pre-fishery levels of the late 1950's, levels largely unchanged since the 1970s; and (2) neither population is recovering at a rate consistent with these levels of depletion and very substantial reductions in reported kills. Data on the possible causes for the continued depletions were too sparse to be conclusive on possible ecosystem effects, but existing information did not support the occurrence of the 70% reduction in effective carrying capacity that would be required to cause the dolphin stocks to remain stable at such low levels. Data and results on possible indirect fishery effects also were inconclusive, but did disclose a common pattern of separation of cows and nursing calves. More data and studies are needed to bring closure to questions surrounding the lack of substantial progress toward recovery by these severely depleted dolphin stocks. The long-term ETP research proposal describes a program of action directed at this closure.

## Reviewer Responsibilities

The Center of Independent Experts (CIE) shall provide four expert reviewers. Each reviewer's duties shall require a maximum of seven days of effort, including time to read relevant documents and to produce an individual written report consisting of their comments and recommendations. No travel is required, so each reviewer shall

work from their home location. Each reviewer's report shall reflect his/her area(s) of expertise, and no consensus opinion (or report) will be required. Further, each reviewer shall only comment on sections within his/her area of expertise.

Expertise needed to review the proposed long-term research plan, including its methods, scope and priorities, includes the following: (1) cetacean biology, (2) line transect-based abundance estimation and stock assessment modeling, (3) biological oceanography and pelagic marine ecology, and (4) population identity – stock structure.

Documents supplied to the reviewers shall consist of the (1) Long-Term Research Proposal in the ETP, (2) 2002 Final Science Report, and (3) CIE reviews of the Final Science Report. The reviewers shall become familiar with the research plan and the background documents.

## Specific Reviewer Tasks and Schedule

1. Read and consider the 2002 Final Science Report and CIE reviews of the Final Science Report that provide context and background on research in the eastern tropical Pacific Ocean.

2. Read and analyze the Long-Term Research Proposal for the ETP that describes the SWC's approach to resolve the cause(s) of the apparent lack of recovery by depleted dolphin stocks in the ETP.

3. Specific points to be addressed (at minimum) for sections within each reviewer's area of expertise:
   (a) Is the scope of the proposal adequate and appropriate?
   (b) Are any areas addressed in less, or more, depth than needed?
   (c) Are the approaches proposed unbiased?
   (d) Does the proposal represent Best Available Science? If not, what specifically would be required to meet that designation, in your opinion?
   (e) Comment on the proposal's strengths and weaknesses, and suggest any additional lines of research that appear promising.

4. Specific points to be addressed (at minimum) for all sections:
   (a) Overall, are the individual sections well integrated into the proposal as a whole? If not, what could be done to improve integration?

5. No later than August 1, 2006, submit a written report[2] to the CIE that addresses the points in items 3 and 4 above. See Annex I for additional details on the report outline. Each report shall be sent to Dr. David Die, via email at ddie@rsmas.miami.edu, and to Mr. Manoj Shivlani, via email at mshivlani@rsmas.miami.edu.

[2] Each written report will undergo an internal CIE review before it is considered final.

## 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.

Please refer to the following website for additional information on report generation: http://www.rsmas.miami.edu/groups/cimas/Report_Standard_Format.html