Panel Scientific and Technical Review Form
(Note: Review comments will be anonymous, but public.)

Proposal number: 2001-B200 **Short Proposal Title: Mitten crab (Culver)**

This is a research project to characterize the reproductive life history of the invasive Asian mitten crab in the Bay-Delta region, primarily the identification of breeding sites and environmental conditions favoring breeding via a combination of larval settlement sampling and documentation of collections of brooding females. Age at maturity will be evaluated using field collected specimens, while the potential for, and magnitude of, repeat brooding by females will also be assessed under laboratory conditions. The goal is to provide life history information that will facilitate the use of control measures at the time and place that will provide maximal levels of population control, although the research is primarily basic in nature.

1a) Are the objectives and hypotheses clearly stated?

Summary of Reviewers comments:
Clearly stated, although links between objectives and removal strategy (management) was tenuous, and application may be inconclusive.

Panel Summary:
Reasonably well stated problem and general approach to solutions, using life history info. to understand vulnerable stages or periods in the population cycle. Crabs may be more concentrated in breeding sites, or in transit there, and during this period there is best chance for predicting vulnerability windows. They clearly identified what info. is missing and needed, esp. the lack of understanding of environmental factors related to breeding and larval settlement – surprisingly little is known about these.

Hypotheses were adequate, but not inspired, consisting primarily of ideas that are generally already known or fairly obvious: settlement greatest in late spring to early summer; peak reproduction is at low flow; females aggregate in breeding habitat; only the single vs. multiple brood periods is a novel hypothesis, but its importance to understanding life histories and also to control potential is not strong. Needed was a stronger set of hypotheses related to mechanisms, physiological and/or environmental, that determine reproductive behavior and phenologies.

1b1) Does the conceptual model clearly explain the underlying basis for the proposed work?

Summary of Reviewers comments:
Conceptual model seems appropriate, but with possible shortcomings. Reproductive period is short, and fecundity high, so potential for removing significantly large proportion of the population seems unlikely.

Insufficient data on population structure at this point to base management decisions, and so this information will be valuable.

Panel Summary:
The case was fairly laid out, although our understanding would have been improved with more discussion of what is know about this crab already, esp. in Asia; and, since this is a freshwater species, more evaluation of what is know about other freshwater decapods would have been useful. Applying 'fishery-type' models to crab populations was interesting, but could have been more detailed in terms of identifying and quantitatively modeling critical junctures in the life history. How DOES one incorporate life history info. into predictions of time and place of reproduction, esp. when these are presumably variable from year to year? The conceptual model could have been more focused on identifying mechanisms, as pointed out above, rather than relying on the concept of using life history info. for making management decisions.

1b2) Is the approach well designed and appropriate for meeting the objectives of the project?

Summary of Reviewers comments:
All the data will be useful in the long run, but only portions will be directly applicable to management decisions. Recruitment dynamics are important, and approach is reasonable, but relationships with hydrology and stochastic weather patterns should be more fully considered.

Panel Summary:
Again, if we assume objectives are to characterize reproductive biology, then approach is basically good; the stated objective of using information for making management decisions could possibly be de-emphasized. The researchers are generally going in the right direction, although there may be some practical issues that should be taken more fully into account: are there other indicators of age, such as exoskeleton 'layering', use of tetracycline markers, etc.? are there other tools for identifying breeding sites, like tags, labels or transponders that could be tracked? what if breeding and settlement occur in vegetation, rather than open water – then, might the settlement traps be ineffective in the chosen sites? Are there enough settlement collection sites to provide comprehensive view of breeding ecology? Maybe the focus should be more on just figuring out precisely where breeding occurs, and then develop intensive study of why once this aspect is better known so that methods could be chosen based on findings.

There are many assumptions that the proposed methods will be successful, and easy. What will the back-up approach be if the proposed design is ineffective? What are possible reasons for failure (which is possible in any research)? More explanation for confidence in information from laboratory culturing would be helpful to overcome skepticism – growth rates and other traits are often subject to artifacts when moved to the lab.

1c1) Has the applicant justified the selection of research, pilot or demonstration project, or a full-scale implementation project?

Summary of Reviewers comments:
Yes

Panel Summary:
Generally identified as 'research', and it is very useful research, yet their emphasis on linkage of results with practical applications is not strong.

1c2) Is the project likely to generate information that can be used to inform future decision making?

Summary of Reviewers comments:
Probably more useful for improving general understanding of biology. Somewhat weak on capacity to evaluate causes of variation in the population.

Panel Summary:
There is a strong need for more information on basic biology of this species, but as stated above the linkage between reproductive events and short-term management decisions in not particularly strong.

2a) Are the monitoring and information assessment plans adequate to assess the outcome of the project?

Summary of Reviewers comments:
Research good and study monitoring is likely to be sound, although stated goals may not be met.

Panel Summary:
Probably reasonable, but seems that much is left unstated. As stated above, there is not a strong evaluation of contingency plans should the proposed design be problematic. We feel there should be better evaluation of possible problems, contingencies, etc.

2b) Are data collection, data management, data analysis, and reporting plans well-described, scientifically sound and adequate to meet the proposed objectives?

Summary of Reviewers comments:
Seems adequate.

Panel Summary:
Probably OK but data management and analysis was not discussed in any detail (e.g. proposal states that data will go into a 'MS Access database', which doesn't say much about how data will be handled and analyzed). Also, the use of growth curves to identify life history features was mentioned but not discussed at a level where we could assess its utility.

3) Is the proposed work likely to be technically feasible?

Summary of Reviewers comments:
Technically feasible, but questions about the appropriateness of the original approach and research questions.

Panel Summary:
Same as above – proposers assume these crabs will act like others ("we do not anticipate any problems' was a common refrain, yet these statements of confidence were not well-justified), but panel not convinces of lack of uncertainly. Why should we assume that freshwater crabs act like marine ones? Maybe so, but more justification needed. Could crayfish or other freshwater decapods provide useful models for predicting how these crabs will work? Again, their methods probably will be suitable, but it would be helpful to have more reason for confidence in that.

4) Is the proposed project team qualified to efficiently and effectively implement the proposed project?

Summary of Reviewers comments:
Certainly qualified, and have experience with mitten crab, but distance from study site may reduce efficiency.

Panel Summary:
The team seems scientifically competent, although the limited experience with freshwater or brackish water organisms may be a minor issue. Other potential collaborators were mentioned, yet the means for integrating with these other scientists was not clear. If others are doing related work, esp. since it is stated in the proposal that other funded projects get at 'distribution and population dynamics' questions, why aren't they more fully integrated into the 'team'? The publication record for the proposers is not strong (no citations in 'Lit Cited'), which makes cooperation with others more important in providing sense of confidence.

5)Other comments

Variation in the Bay-Delta system and hydrology should be more fully evaluated for their possible influence on population dynamics and reproductive behavior – these elements are recognized, yet not adequately addressed in the research approach.

Didn't fully explore other options for control (e.g. biological control, inhibiting upstream migration, etc.) for comparison, or really provide a comprehensive background on what WAS known about mitten crabs here or in Asia or Europe, nor coverage of crab biology in general that might be relevant. In particular, the introduced populations in Europe have apparently experienced dramatic population increases and declines, and the mechanisms behind this could be useful in developing control – this may not be directly relevant to this proposal, but should have been addressed.

The role of predation is considered insignificant, but not necessarily based on strong field information – predation has been seen in the field, so its potential role should not be so easily rejected.

The implications of life history info. for assessing approaches to control were reasonable, but in some cases not strongly supported – e.g., why are number of brood periods, age at reproduction, and number of year classes contributing to the population critical bits of info. for better control? The project could be justified simply for providing a more comprehensive picture of how this very problematic creature work. By justifying research primarily for figuring out better control procedures, one was left hanging trying to figure out just how the resulting info. would be integrated into control efforts – and mechanisms for integration of results into management plans were little discussed.

Is semelparity really known?

How does multiple breeding offer opportunity for control?

Reviewers Evaluations:

Three outside reviews were provided, and evaluations were Fair, Good and Excellent. The 2 reviewers with the Fair and Good evaluations are personally experienced with estuarine crab biology.

Overall Evaluation
PANEL SUMMARY COMMENTS

The panel feels that this proposal was GOOD, and would have been rated more highly if some of the expressed concerns had been addressed. The lack of information on this species clearly need to be addressed, and this research proposal is a good step in that direction. Some of the ideas here are good, and should be presented as basic ecological studies rather than attempting to tie them directly into management decisions. Application will certainly result from this type of research in the long run, but the evaluation of environmental relationships with basic reproductive biology is an adequate goal of the research. Re-submission with a clearer evaluation of the research approaches, and incorporation of suggestions provided in the reviews, could lead to an excellent-ranked proposal.

Summary Rating

- ☐ Excellent
- ☐ Very Good
- ☐ Good XXX
- ☐ Fair
- ☐ Poor