



United States Department of the Interior

U.S. GEOLOGICAL SURVEY
Office of the Director
Reston, Virginia 20192

In Reply Refer To:
Mail Stop 300
#20040150

MEMORANDUM

JAN 12 2004

To: Assistant Secretary - Water and Science
Assistant Secretary - Fish, Wildlife and Parks

From: Charles G. Groat, Director, U.S. Geological Survey
Steven A. Williams, Jr., Director, U.S. Fish and Wildlife Service

Subject: Results of the Peer Review of the San Luis and Delta--Mendota Water Authority's
White Paper titled "The Delta Smelt and the State of the Science"

As per your request of October 21, 2002, the U.S. Geological Survey (USGS), in collaboration with the Fish and Wildlife Service (FWS) completed a peer review of the subject white-paper on delta smelt.

We applied the rigorous peer review process that the USGS uses for all science and other documents. This process requires reviewer anonymity and no involvement in the subject research effort. It further requires the 'editor' to manage the process with the highest standards of professionalism and integrity. Two reviewers were from the USGS, one from the FWS, and one was from academia. All reviewers have exceptional fisheries science expertise and credibility. Please find attached the individual reviews completed by four independent reviewers as well as a synopsis of the collective results compiled by the editor.

In summary, the White Paper recognizes the need for an updated status review for delta smelt and actually presents a partial analysis to assist that process. However, the paper lacks sufficient scope and scientific rigor to present a compelling argument that a designation of threatened status is no longer warranted for the smelt.

If you have additional questions regarding this report, please contact Dr. Susan Haseltine, of the USGS, at 703-648-4050, or Gary Frazer, of the FWS, at 202-208-4646.

Attachments



United States Department of the Interior

U. S. GEOLOGICAL SURVEY
Biological Resources Division
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November 6, 2003

Memorandum

To: Susan D. Haseltine
Associate Director for Biology

From: Ronald E. Kirby
Senior Advisory Biologist, Western Region

Subject: Editor's Comments—Peer Review of the 2002 San Luis and Delta-Mendota
Water Authority White Paper "The Delta Smelt and the State of the Science"

Following are my synthesis comments on the peer review of the delta smelt document. I first describe the material under review. Then I summarize the reviewers' overall conclusions. Finally, I collate the comments from the reviewers in a point-by-point analysis of the conclusions claimed by the authors of the white paper.

No attempt is made to reiterate the sum of the arguments presented by the reviewers. The original comments should be consulted for such details. Nor do I develop an additional set of review comments. Instead, I present through summary the main points germane to assessment of the science quality of this document. This synthesis text should be read in conjunction with a thorough reading of the individual peer review texts.

The Subject Document

The full citation of the paper reviewed is:

San Luis & Delta-Mendota Water Authority. 2002. The delta smelt and the state of the science: a white paper reviewing the 1993 listing of delta smelt as threatened and a review of recent science concerning the delta smelt. 5 pp + Appendices A, B, and C.

The text consists of a two-paragraph *Introduction*, two paragraphs of *Conclusions* containing three major points, and two paragraphs of *Background* (words in italics are section titles within the document). Twelve paragraphs support the following statements (which are the conclusions) in a section entitled *The state of the science*:

- “Since the mid 1980s—well before the fish was listed as threatened—the population of delta smelt has been increasing.”
- “Recent analyses indicate that the number of sub-adult (i.e. fall) delta smelt present in the Bay-Delta ecosystem in the late 1990s was at least 1 million and ranged up to 12 million.”
- “Using these population estimates, the probability of extinction of delta smelt can be estimated. If the population of sub-adults is in the range of 12 million (the 1999 estimate), the probability of extinction by the year 2050 is about one to two tenths of one percent.”

The paper then presents two paragraphs of *Conclusion(s)* which include 1) the statement that there is “a compelling argument for removing the delta smelt from both California’s and the U.S. government’s threatened species list” and 2) a request that water allocation decisions that favor the delta smelt be revisited.

The text contains no literature citations but internally and indirectly refers to the attached appendices. The text, in effect, reiterates the conclusions of these appendices:

Appendix A (Manley, Miller, and Hillman 2002. *Trends in the fall midwinter trawl abundance index for delta smelt*) analyzes recent delta smelt abundance data and concludes, “The fall midwinter trawl index has shown an upward trend for approximately the last 15 years.” This appendix cites a draft report to CALFED and two mathematical references.

Appendix B (Miller and Britton 2002. *Estimating the population of sub-adult smelt*) explains data manipulation methods, defines assumptions, and provides a table of abundance indices. “Conservatively low” estimates of the sub-adult population are calculated to range from 1 to 18 million from 1996-1999. Calculation sheets are provided. This appendix cites a personal communication, a memorandum, and the USFWS 1996 recovery plan for Sacramento/San Joaquin Delta Native Fishes.

Appendix C (Miller 2002. *Converting sub-adult delta smelt population estimates to estimates of adult population*) consists of text explaining assumptions and the named estimates. Estimates of the adult population range from a low of 200,000 to a high of 300,000 to 1,600,000. This appendix cites Appendices A and B, the aforementioned draft paper to CALFED, and Moyle et al. on the life history of the delta smelt.

Peer Review Comments

Overall Assessment—There was unanimity of opinion among the four peer reviewers that the conclusions reached in the paper were not supported by either the data or the arguments presented. The reviewers criticized both the selective use of data and application of questionable analytical techniques. The reviewers viewed the assumptions accepted in the analyses (which the authors termed “conservative”) as unsupportable with the evidence provided. The authors were viewed as having ignored the results and implications of their work that were contrary to their conclusions. Most importantly, in their focus on delta smelt abundance indices, the authors were viewed as having ignored the important aspects of the life history of this organism which include their 1) severely restricted geographic range, 2) one year life cycle, and 3) semelparous life history.

There was unanimity that the authors selected data specifically to support only certain points and that the overall effort was extremely weak scientifically. The data presented were not viewed as “new” by the reviewers as claimed by the authors and commonly accepted methods for analyzing data of the sort in this paper were not used. All reviewers questioned various portions of the analyses, which they viewed as inconsistent with the generally accepted meaning of the term and lacking in rigor.

Detailed comment—The conclusions of the white paper depend entirely upon the analyses of the three appendices. Specific comments supporting the summary paragraphs above are provided first for the appendices and then for the paper proper.

Appendix A (Manley, Miller, and Hillman 2002. *Trends in the fall midwinter trawl abundance index for delta smelt*)

The reviewers' overall conclusion was that the authors failed to demonstrate a positive trend in delta smelt abundance. The statistical techniques were found questionable in that the authors only visually fitted different lines to a single subset of the data (limited time series) without any analysis that would permit evaluation of supposed trends. Specifically, there were no statistical tests to determine positive or negative trends and there was no discussion of either significance or power of the results. Reviewer 4 supported this criticism by applying modern techniques to the data and found the opposite trend from that claimed by the authors. Given the availability of many statistical packages appropriate to analyses of this sort, their common and accepted use in the scientific literature, and the great amount of data available to the authors, the authors' choice of visual line fitting was deemed inexplicable and not in keeping with standard scientific practice.

Especially troublesome to the reviewers was the focus of this analysis on only one of the life stages available for delta smelt. Reviewer 4 pointed out that the text provided no rationale from the ecological perspective that justified the authors' use of only the autumn mid-water trawl, when the literature emphasizes the value of summer tow-net abundance indices. The lack of discussion of causal factors that would put any trend identified in context was found to be a fatal shortcoming. Even reviewer 3, who allowed

that the analysis showed a turnaround in the "trend" based upon the assumptions used, did not accept that possibility as removing risk from the population as claimed by the authors. Reviewer 2's concern about bias in the analysis is well taken and is supported by comments from the other reviewers. Risks to the population from both anthropogenic and climatic factors were listed by the reviewers as major concerns in evaluating these abundance indices, regardless of analytical outcome.

Appendix B (Miller and Britton 2002. *Estimating the population of sub-adult smelt*)

The reviewers rejected the assumptions, methodology, and conclusions of this appendix. A statistically significant positive trend, as claimed, is not presented. Knowledge of the life history of the fish, especially its preferred depth and areal distribution, was not incorporated in analysis. The comparison of catches with different gear and scaling of density data to obtain population estimates was found unsupportable as were the estimated correction factors used to offset trawl "inefficiency." Importantly, and fatal to the argument in this appendix, the use of a correction factor for the Kodiak trawl was questioned by the reviewers from several viewpoints. Scaling up of zero values is found unacceptable given the assumptions listed, life history of the fish, and limitations of the gear. Reviewer 2 provided some suggestions on how this analysis might have been conducted including deletion of outlier data and recognition of the "noise" inherent in data of this type. Reviewer 4 provided calculations showing how removal of one outlier point dramatically changes the entire analysis presented by the authors.

Appendix C (Miller 2002. *Converting sub-adult delta smelt population estimates to estimates of adult population*)

The reviewers found the uncertainties in the calculations in Appendix B, which led to rejection of Appendix B's conclusions, dramatically increased by the assumptions accepted in this appendix. Therefore, the estimates of the numbers of adults as calculated in this appendix were judged invalid as well.

Further summarization is best organized within the categories defined by the authors of the document, i.e., the three conclusions, and also the concluding remarks in the final section of the document:

- "Since the mid 1980s—well before the fish was listed as threatened—the population of delta smelt has been increasing."

The reviewers found that the data presented do not support this conclusion. Instead, it is clear that there is large inter-annual variability as expected from a species with this life cycle.

- "Recent analyses indicate that the number of sub-adult (i.e. fall) delta smelt present in the Bay-Delta ecosystem in the late 1990s was at least 1 million and ranged up to 12 million."

The reviewers deemed the procedures used to reach this conclusion unacceptable. At least four steps in the calculations have been found in either error or dependent upon critical assumptions that were omitted in analysis. The numbers obtained are therefore unsupportable as estimates of population size.

- “Using these population estimates, the probability of extinction of delta smelt can be estimated. If the population of sub-adults is in the range of 12 million (the 1999 estimate), the probability of extinction by the year 2050 is about one to two tenths of one percent.”

The reviewers found the first two conclusions unrealistically optimistic and based on questionable science. They therefore found the use of these conclusions to reach this last conclusion a severe underestimate of the probability of extinction risk. Importantly, this statement of extinction risk is based upon an unpublished model that has no associated risk factors, there is no estimation of likelihood of such risks, nor is this conclusion based upon commonly accepted methods of Population Viability Analysis.

The reviewers' comments on the final section of the paper (*Conclusion*) provide a final assessment of the white paper:

The reviewers found this section contained statements not supported by either the paper proper or the appendices. The reviewers did not agree that this paper proved that the population was increasing, nor did they agree with the estimation of probability of extinction. They concluded that even if the population were increasing, that in and of itself would not be sufficient to conclude that the threat to the population was significantly less than previously thought. This last argument was made specific by repeated comment by the reviewers that the life history of the fish was not properly considered in these analyses and that the consequences of a within-year catastrophe for the species were unappreciated by the authors. Thus, contrary to assertions by the authors, the reviewers did not find this paper “a compelling argument for removing the delta smelt from both California’s and the U.S. Government’s threatened species list.”

The paper’s final argument was that, based upon the conclusions of this white paper, water allocation decisions within the Central Valley Project and the State Water Project should be revisited. This issue was beyond the scope of the analysis of the science of the document and was not commented upon by the reviewers.

The peer reviewers provided detailed discussion on every point of the paper and its appendices, made specific suggestions regarding alternative analytical techniques (and in some cases reanalyzed the authors' data to illustrate their comments), specifically mentioned ancillary data that should be brought to bear on the topic, and throughout suggested organizational rearrangements that would make this document an acceptably scientific analysis and a presentation in conformance with standard and accepted methods of reporting science. The reviewers have provided literature citations they believe pertinent and have suggested numerous ways in which this overall analysis could be improved. We hope these are useful to the authors. The peer reviewers and I appreciated the opportunity to address the issues raised in the San Luis & Delta-Mendota Water Authority's 2002 assessment, *The delta smelt and the state of the science: a white paper reviewing the 1993 listing of delta smelt as threatened and a review of recent science concerning the delta smelt.*

(signed) Ronald E. Kirby

Attachments: Peer review comments from Reviewers 1-4

Review of The Delta Smelt and the State of the Science

This "paper" is actually a reiteration of the three documents that I review below.

Introduction

The paper gets off to a bad start by showing a misunderstanding over what happened in the Klamath Basin. The regional economy was not threatened with "sudden and catastrophic collapse" because of cut-off of the water (farming is a small percentage of the economy) during a period of severe drought. The NRC report did not say there was "insufficient science" but that the agencies failed to adequately consider alternative explanations for the results of *some* of their studies. There has been no general "re-examination of ... the scientific foundations of Endangered Species Act determinations" by reputable groups because there is good evidence the process has generally worked well, all things considered (as expressed in other NRC reports).

The author's state their intention is to examine "the state of the science in connection with the status of the delta smelt...". However, this paper and its attached appendices address only data collected by one survey, the Fall Midwater Trawl Survey (FMWT). The authors ignore other important data sources such as the Suisun Marsh Survey and numerous analyses done by other researchers studying this species and the larger biotic community of the San Francisco Estuary. This paper is not, in any way, a comprehensive review of what is known about delta smelt in particular or the San Francisco Estuary more generally, nor is it an evaluation of the "state of the science." It is a seriously flawed analysis of a limited set of selectively chosen data, designed to support a predetermined conclusion.

Conclusions I find the location of this section unusual and suggestive of their approach. At this point in the paper, the authors have not presented a context or rationale for these conclusions, hypotheses which are to be tested, nor any methodology used to assess these hypotheses. Regardless, each of these conclusions is based on erroneous assumptions and/or heuristic impressions of the data.

(1) The assertion that the delta smelt population "has been increasing" since the mid-1980's is demonstrably false. Based on the data presented in this paper (FMWT indices), it would be correct to say that the delta smelt population has displayed the high inter-annual variability typical of this and all other semelparous, annual species. In 2002, the Suisun Marsh Survey (which began in 1980) recorded very low catches of delta smelt – among the lowest seen in the data set as a whole.

(2) The authors claim that the delta smelt population has recently ranged between 1 million and 12 million individuals. As I explain below, the procedure used to generate this estimate is completely without merit. It is extremely unlikely that delta smelt populations have reached these levels in recent years. Even if the population had been as high as the authors estimate, the species would still be at grave risk of extinction because many of the threats faced by this species (e.g., entrainment on the Delta pumps, widespread collapse of Delta levees by earthquake or flood, major chemical spills, invasion of non-native predators, competitors, or pathogens, etc.) are completely unrelated to population size or density.

(3) Based on the previous two assertions, the authors suggest that the extinction risk for delta smelt is <0.2% over the next 48 years. Any effort to model extinction risk based on #1 and #2 above severely underestimates extinction risk because the inputs are unrealistically optimistic. Also, the extinction-risk estimate does not (and probably cannot) account for the risk of catastrophic events that threaten delta smelt in its limited range.

I am sympathetic to the author's point in the final paragraph of page 1. It would be nice if we had adequate historical abundance data from sampling gear that caught delta smelt. It would also be nice if we understood delta smelt ecology well enough to interpret survey results in terms of population size. We are making tremendous strides on the latter front; but unfortunately, the best data long-term data we have for

delta smelt comes from **sampling gear** that is not well-designed for catching delta smelt. The existing data are very useful for assessing population *trends*, but they are not much good for estimating the absolute abundance of delta smelt in any one year. Fortunately, knowing the exact number of delta smelt in the Estuary at any point in time is not necessary to protect and manage the delta smelt population.

Background

This is a rather meager summary of the biology of delta smelt considering this paper purports to be determining "the state of the science." It is interesting that even the tiny amount of life history information presented here is largely ignored in the analyses. In the second paragraph, the authors assert, with no documentation, that the listing of the delta smelt has "resulted in a substantial loss of water for agriculture, municipal, and industrial uses." There is no doubt that water project operations have been changed as the result of the smelt (and other species) but whether there has been substantial loss due *solely* to smelt is open to question. A strong and bald assertion like this requires reference to real analyses of water budgets and economics.

The state of the science

The authors claim that the Service's analysis of the Delta smelt's population status was "misleading". However, the authors then present two other comparisons that they believe demonstrate a population increase *since* listing. It is not clear how this supports their point that the Service's initial evaluation *leading to the listing* was misleading.

The authors then refer to a paper by Manly, Hillman, and Miller (*unpublished; attached as an appendix*) to support their claim that the population has increased over the past 15 years. I discuss this paper in detail below. The authors' claim that Manly et al. (*unpublished*) present a "thorough statistical analysis" is false. The referenced paper presents no statistical analyses whatsoever. Thus, their claim that Manly et al. (*unpublished*) "shows a significant increasing trend in the abundance of delta smelt" is also false.

Next the authors restate their claim that delta smelt populations ranged between 1-12 million in the years 1996-1999. This claim is based on another paper (Miller and Britton *unpublished, also attached as an appendix*). Miller and Britton (analyzed in more detail below) is based on a non-significant correlation between the FMWT and another sampling gear (the Kodiak Trawl) which is much more effective at catching delta smelt. The authors of both papers seem to be unaware that failure to detect a significant correlation between two data sets means that one cannot use results of data set "A" to predict results of data set "B". Even if there were a statistically significant correlation between delta smelt catches by the two gear types, Miller and Britton's procedure for estimating Kodiak Trawl catches using historical catches by the FMWT is hopelessly optimistic.

The authors then reiterate their third point, that extinction of delta smelt in the near future is unlikely based on a current, estimated spawning population of 1 million individuals. The authors base this claim on a mathematical model created by Manly (*unpublished*). No descriptions of the risk factors or estimation of their likelihood or impacts are included. No statistical analyses of the results are presented. Because the results of this model are based on optimistic speculations about the current population size of delta smelt (*see above and below*) and because I have no way of evaluating whether the model parameters are comprehensive or realistic (but strongly suspect that they are neither), the resultant estimate of population risk is nothing more than wishful thinking.

The rest of this section presents a legal rationale for the Service to conduct a status review. I am not qualified to evaluate the argument, nor is it germane to the Service's effort to gather "the best available scientific information" on the delta smelt.

Conclusion

The authors argue that the delta smelt should be removed from federal and state Endangered Species lists. To support this argument they reiterate their claims that (1) the population has been increasing for the past 15 years, (2) the population is larger than previously thought, and (3) given #1 and #2, the risk of extinction is small. There is no new information here and the reasoning is weak as well. As I discuss below the population has not been "increasing for 15 years" but even if it had been, an increase would not necessarily mean that the population is out of danger. Similarly, if the population were larger than previously thought, it would not mean that the threat to the species was *substantially* less than previously thought. As I have already written, the threats facing the Delta smelt population are numerous, large and, in many cases, not at all related to the species' population size. Finally, the evaluation of risk to the species cannot be accurate since (a) the inputs are not accurate, and (b) in numerous one-year periods, the delta smelt population declined perilously close to extinction (for example, the 1981, 1994, and 1996 abundance estimates) from levels that are much higher than current estimates. Given the delta smelt's one year life cycle, a similar decline in abundance is not only possible, it is likely under conditions that are common during a drought. A decline of similar magnitude to that which occurred between 1980 and 1981 or 1993 and 1994 or 1995 and 1996 could extirpate the delta smelt and, if not, would certainly place it in imminent danger of extinction.

Review of Trends in the fall midwater trawl abundance index for delta smelt By B. Manly, W. Miller, and T. Hillman

This paper claims to "examine the trend in the fall midwater trawl index based on data collected since the listing decision was made". This "examination" consists of fitting the data with several different "trend lines". No statistical analyses are presented making evaluation of "trends" impossible. Simply, the authors have drawn different lines through the data (using increasingly complex line-drawing algorithms) and then they have asserted that the delta smelt index has "trended" up for the past 15 years. Curiously, even their own lines do not support this conclusion.

Background

The first three paragraphs seem reasonably accurate but the information is not new. In the fourth paragraph, the authors state that the FMWT index is the "most important" measure of delta smelt abundance. There are several sampling programs that regularly detect delta smelt. Each of these provides valuable information. Although the relative merits of any of these sampling programs may be debated (and none of them are optimally designed to measure delta smelt abundance), the authors examine only the FMWT index. At the very least, they should acknowledge that the Suisun Marsh Survey's catches of delta smelt corroborate patterns detected by the FMWT in most years.

Conclusion

Again, I find it disconcerting that the authors place their conclusions before methods, results, or discussion. Worse though, their conclusion is incorrect and unsubstantiated – the FMWT delta smelt abundance index has both increased and decreased over the past 15 years. Abundance indices in recent years have been low by historical standards and the most recent "trend" (if there is one) has arguably been a decline.

Trends

The authors state "We used four generally accepted methods [for estimating trends in the FMWT index] to see if all four would produce the same results". This statement signals a serious problem with the authors approach. Why have the authors employed four different methods to establish a trend? One method (two

at most) ought to be most appropriate for these data and suffice for further analysis. Different line-drawing algorithms ought to show similar results when they are applied to the *exact same data*. Performing the line-drawing operation *on the same data* creates the false impression that there are several trends pointing in the same direction. In fact, *there is only one data set here*. Rather than draw several lines through these data, the authors should have attempted to *analyze the strength and significance* of any "trends" produced by any *one* line. This basic statistical analysis is completely absent. Instead, it appears that the authors have (1) drawn several different lines through the data and then (2) looked at an arbitrary starting point (the mid-1980's lows) and the most recent three years of data and then (3) drawn a straight line between the 1980's lows and the current abundance estimates. Why bother using "linear splines smoothing" or a complex polynomial if you are not going to (a) analyze the strength of the relationship statistically, or even (b) look at the actual lines these methods actually produce?

Even if the authors had actually analyzed the results of their line-drawing exercises, I would question the techniques they employed. For example, the 5-year moving average of delta smelt abundance would not reveal that delta smelt had been completely extirpated even four years after the extirpation occurred. By "averaging" data from different years together, the five-year moving average (and, to differing extents, the Lowess and spline analyses) builds in temporal dependence into the data set that did not exist previously. Generally speaking, statisticians should seek to limit dependence among data points, not increase it. Indeed, when the authors employ line-drawing algorithms that minimize the number of years which contribute to "the trend line", their graphs show a declining "trend" over the last three years. Conversely, only the stiffest or most rigid lines (those where the influence of a single year registers over a very long section of the line) show a continuous increase in the abundance index. Figure 2, four of six panels in figure 3, and 3 of 4 panels in figure 4 (all of which use the same data) show that, over the past 15 years, the FMWT has both increased and decreased at different times. This directly contradicts the authors' assertion that populations have "shown an upward trend for approximately the last 15 years".

I must reiterate here that the authors have *not established any trend* because they have failed to *analyze the data* in any valid way that would establish a positive or negative population trend. Even if they had established a statistically, significant positive trend in the FMWT index, the rationale for, or estimate of, a population increase that would constitute a significant recovery of the delta smelt population is needed but absent.

Review of Estimating the population of sub-adult delta smelt by W. Miller and A. Britton

In this paper, the authors attempt to estimate current and historical delta smelt populations by correcting for the fall mid-water trawl's inefficiency at catching delta smelt. Throughout the paper, the authors repeat (like a mantra) that they "employ assumptions designed to produce conservatively low estimates" of delta smelt abundance. Actually, all of their assumptions are unsupported by the evidence and would tend to produce a high estimate of delta smelt population size. For example, they employ a putative relationship between the FMWT and another piece of gear, the Kodiak Trawl. However, *there is no statistically significant correlation* between the catches made by the FMWT and those made by the Kodiak trawl. In other words, any perceived relationship between the two gear types is not significantly different from *no* relationship. Thus, one cannot use historical FMWT catches to validly estimate what would have been caught had the Kodiak Trawl gear been used instead. The authors have ignored this fundamental principle of statistics and instead multiply historical FMWT catches by a large correction factor. This is completely and totally inappropriate. What is the point of using statistics if they can be thrown out willy-nilly for "computational simplicity?" The only reason they can even draw a "regression line" with an upward slope is because of a single point created by high catches with both gear types.

The authors then use their bogus estimates of what the Kodiak trawl would have caught (had it been used) and multiply these numbers by two constants to estimate the total population of delta smelt in the

Estuary. First, they assume that delta smelt were evenly distributed throughout the top 15 feet of the water column and multiply the estimated average Kodiak trawl catch-per-unit-effort by the volume of water in the top 15 feet of the Delta, Suisun Bay, and San Pablo Bay. There is absolutely no evidence to support this expansion of the, already highly speculative, estimated Kodiak trawl catches. Delta smelt are very surface oriented and they may not occur in large numbers below the top few feet of the water column. Indeed, the authors ignore the fact that the Kodiak trawl catches more fish than the FMWT because the former gear samples only the uppermost layer of the water column where delta smelt aggregate. The Kodiak trawl samples only the top 6 feet of the water column so there is *absolutely no data with which to estimate smelt abundance in waters deeper than 6 feet*. Furthermore, all of the fish caught by the Kodiak trawl might have occurred in much shallower water than the deepest water (ca. 6 feet) sampled by the net. If, for example, delta smelt are primarily found only in the top 6-12 inches of the water column, then the authors' estimate of delta smelt abundance would be reduced by a factor of 15-30x. The larger point is that, without additional study of the delta smelt's depth distribution and a more extensive Kodiak trawl data set, estimation of delta smelt populations based on estimated Kodiak trawl catches is speculative in the extreme.

The authors also assumed that the Kodiak trawl would have caught fish when the FMWT failed to catch fish. This speculation is based on the finding that, on 3 occasions in a side-by-side study, the Kodiak trawl caught fish when the FMWT did not catch fish. A sample size of three is far too small to justify the assumption that some number of fish existed in historical samples where none were detected by the FMWT. Also, as stated before, the regression equation the authors employ to convert FMWT catches to Kodiak trawl catches has *absolutely no statistical relevance*. But, even if it were statistically significant, the relationship between catches of the two nets is far too variable to allow accurate prediction of catches with one net based on catches by the other net. The authors have not even bothered to calculate confidence intervals on their (already too liberal) estimates. Had they incorporated the data variance into the estimates, the resultant error bars would undoubtedly have been huge.

The problems with their approach are also apparent when the spread sheets provided are examined closely. For example, in September, 1996 only one smelt was caught by the FMWT in Carquinez Strait and only one was caught in Suisun Bay. Yet the expanded estimate of smelt numbers is 684,000 for Carquinez Strait and 337,400 for Suisun Bay! Thus 37% of the 2.7 million estimated smelt are the result of a catch of 2 fish.

I firmly believe that the actual number of delta smelt in the San Francisco Estuary is far less than the number estimated by these authors. Despite the limitations of the FMWT index, these data are still quite valuable for assessing long-term trends in delta smelt abundance. First of all, the FMWT data set is largely corroborated by results of the Suisun Marsh Survey and CDFG's Bay Study—each of these monitoring programs reveal relatively low current abundance compared to historical values. Also, though the FMWT data will not allow precise calculation of delta smelt population size, comparison of index values from year-to-year is likely to be well correlated with delta smelt abundance. Finally, while the abundance of delta smelt is a valuable piece of information, the delta smelt will remain in severe jeopardy of extinction as long as the habitat in its very small geographic range is heavily modified by human activities and introduced species.

Review of **Converting sub-adult delta smelt population estimates to estimates of adult population** by B. J. Miller.

The basis of this paper is that the estimates of smelt population abundance made in the previous paper were estimates of subadult populations and that this necessitates converting those numbers into the number of potentially spawning adults. Because the initial numbers used are not valid, as indicated above, the numbers of adults estimated in this paper are also not valid. In any case the conversion model used requires a number of simplified assumptions of delta smelt life history that increase the likelihood of error to the point where it is likely that even the model has little validity.

Review of "The Delta smelt and the state of the science," sponsored by San Luis and Delta-Mendota Water Authority and dated July 2002.

This paper suggests a strategy to improve one of the abundance indices for delta smelt, analyzes adjusted abundance data for trend, and presents an unsubstantiated conclusion from a separate, undocumented study (presumably quantitative modeling) on the risk of extinction for delta smelt. The authors then conclude that "The ... scientific knowledge base concerning delta smelt and the impacts of water exports on the Bay-Delta ecosystem present a compelling argument for removing the delta smelt from both California's and the U.S. government's threatened species list." This paper, however, does not compel this conclusion. The paper provides no description of the study of extinction risk (mentioned above) and no analyses of impacts from water exports on the ecosystem so the reader is left to take the associated results/conclusions on faith alone. Because of these and other critical flaws mentioned below, the paper is an exhortation, not a compelling argument.

The paper suggests a reasonable approach for analyzing midwater trawl data that should provide better insight into population status; however, it provides an inadequate argument for de-listing because it considers only a subset of the relevant information (e.g., only one of several indices [App. A, page 1, par. 3, lines 3-5] are mentioned or analyzed; and factors beyond abundance indices also must be evaluated in listing/de-listing deliberations). Furthermore, the paper does not describe or document the population viability analysis (PVA) which is critical to the final conclusion (page 1). A wide range of methods are available for population viability analysis, and a detailed description of the methodology is essential because suitability and reliability vary widely among methods.

The paper also suffers from various omissions. For example, it should compare the various abundance indicators for consistency, and should evaluate or document the impacts of water exports. The authors provide no evidence or discussion of how (or whether) it has been shown that "the routine curtailments of exports ... have little measurable benefits to delta smelt populations." Indeed, no evidence is presented to exclude the hypothesis that curtailments caused the recent increases in the delta smelt population.

Other deficiencies in the paper also are important. Although the abundance estimates used in the analysis (Page 3, par. 4 & 5, and App. B) are conservative relative to more extravagant estimates, the current estimates are not sufficiently conservative to dispel concerns about bias in the analysis. One wonders whether somewhat more conservative estimates, which would evoke less suspicion of bias, might have substantially altered the results. The analysis should have included additional scenarios to reflect the apparent uncertainty in the vertical distribution of smelt, uncertainty (high variance) in the relation of Kodiak trawl and midwater trawl catches, and uncertainty in the estimates of smelt density for tows where no smelt were captured. Various permutations of the analysis should explore the consequences from a range of alternative assumptions such as (i) all smelt occur within the upper six (or 10) feet of the water column; (ii) the relation between Kodiak and midwater trawl catches might be any one of the relations within the 90 or 95% confidence interval for the maximum likelihood relation; and (iii) the point (77.6,

0) on the surface for Kodiak- midwater catch (App. 2, Figure 2) is an outlier and should be omitted from the analysis.

The current analysis also suffers from inappropriate acknowledgement and response to the large "error" or "noise" associated with trawl catches. Recognition of the noise should have led to discussion of (iii) above, and geometric-mean (or comparable) regression to describe the relation between catches by Kodiak and mid-water trawls. The impact of such reasonable changes in the analysis must be known for one to reasonably judge the rigor or reliability of the conclusions. No doubt the authors agree that the regression analysis also suffers (though not critically) from insufficient data. Side-by-side comparisons should be conducted for more days, with additional explanatory variables included in the trials such as flow, temperature, total depth, salinity profile, or turbidity.

Another critical flaw in the paper is that no analytical or even conceptual attention is given to natural or anthropogenic variation in the system. For example, decadal-scale climatic cycling (vividly demonstrated in California by recent drought and wet periods with major effects on the Sacramento-San Joaquin delta) is widely recognized as a major ecological driver along the West Coast of North America and world wide, and is critically important for rigorous PVA. The paper provides no evidence that the PVA considered decadal-scale variation. Nor was there evidence that the biological consequences of remedial water export schedules or amounts were investigated. No mention of some key biological issues further leads me to question the adequacy of the viability analysis. Such issues include the large interannual variability in abundance expected for a nearly annual species in a dynamic environment, the possibility of severe density limitation during some periods (as suggested by alternating high and low populations during 1990-1997; Appendix A, Figure 1), and the certainty that drought will return.

In summary, this paper suggests the need for an updated status review for delta smelt, and presents a partial analysis to assist that process; however, the paper lacks sufficient scope and rigor to present a compelling argument that a designation of threatened is no longer warranted for the smelt.

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San Luis & Delta-Mendota Water Authority. 2002. The delta smelt and the state of the science: a white paper reviewing the 1993 listing of delta smelt as threatened and a review of recent science concerning the delta smelt. 5 pp + Appendices A, B, and C.

General Comments:

The white paper on delta smelt is succinct and clearly states the author's analytical approach and conclusions. I understand that such a white paper needs to be short and concise for managers and policy makers. The authors have chosen to place great weight in the estimated population levels. However, it is difficult to interpret the significance of these numbers without considering the life history characteristics of delta smelt or the trends in biological and environmental factors that determine delta smelt populations. The white paper has a fairly narrow scope and does not address determinants of delta smelt populations. The citation of scientific literature and technical reports to support the approach is generally lacking. Therefore, the document appears to be somewhat incomplete.

The fall midwater trawl surveys have been conducted for over 30 years and represent a valuable information resource. The trawling surveys were not originally designed as an index for delta smelt. The trawling was started to serve as an index for striped bass. I believe the authors should have acknowledged that their use of the trawl survey data is not the original intended use. Furthermore, the use of the trawl survey data for an index is likely to be widely accepted while use of the data to calculate population estimates has many shortcomings and may not be accepted by the fisheries managers or researchers (Herbold 1996).

The "conclusion" section of the white paper contains conclusions not supported in the text of the paper or the attached appendices. For example, "The increase in the scientific knowledge base concerning delta smelt and the impacts of water exports on the Bay-Delta ecosystem present a compelling argument for removing delta smelt from both California's and the U.S. government's threatened species list." No analysis of the impacts of water exports on the ecosystem or delta smelt populations are cited or presented in the paper or appendices. Low delta smelt index values occurred in the early 1980's and relatively high entrainment rates occurred in 1981 concurrently with the El Nino conditions. A considerable database exists for pump sites where mortality and salvage records (Central Valley Project, State Water Project) are recorded for delta smelt, but an analysis of these data is not presented.

Comments on Appendix A:

This reviewer appreciated the author's efforts to provide information, figures, and data in the appendices describing trends, estimating the population of sub-adults, and converting estimates of sub-adult to adult populations. The analyses by Dr. B. Manly in Appendix A

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are convincing that the "trend" has turned around. The authors notably omit any discussion of causal factors that might have caused the strong downward trend and the more recent increase in the delta smelt indices. Furthermore, The details on the estimated probability of extinction by year 2025 and 2050 are not adequately documented. Therefore, the conclusion "These estimates indicate there is essentially no chance that delta smelt would become extinct in the next half century" is not supported by this white paper. The modeling efforts of William A. Bennett, William J. Miller, and Wim Kimmer provide alternative modeling approaches, but no mention was made of these currently ongoing modeling efforts. Inasmuch as risk of extinction is central to this paper, the dismissive treatment is unfortunate.

During the past 30 years, when the trawl surveys were conducted to determine the delta smelt index, numerous factors such as introduction of exotics, water use, and development of the delta may have contributed to changes in the index. However, the white paper provides no insight to future trends in water use, human population growth, climate change, or other factors. For example, improvements in the management of the Environmental Water Account and improved fish screening technology (e.g., Tracy Test Facility) at water project pumps may improve survival at some locations in the delta. On the other hand, increased water exports or a decadal shift in regional temperatures could turn the trend downward in the future (see Hare and Francis 1994, Bennett and Moyle 1996). Given that we accept the trend in the index has reversed and has recently increased, the next question is, during the next decline in the delta smelt index how far will the index decline and how long will those conditions persist?

The appropriateness of considering causal factors when interpreting trends in the index, particularly low index values that might indicate risk of extinction, is borne out in the Recovery Plan (USFWS 1995) as well as the listing (USFWS 1993). The reasons for decline in order of importance were identified in the Recovery Plan as: 1) reduction in outflows of the Sacramento and San Joaquin rivers, 2) entrainment losses to water diversions, 3) high outflows, 4) changes in food organisms, 5) toxic substances, 6) disease, competition, and predation, and 7) loss of genetic integrity. Note that any of the first four reasons for decline of delta smelt could be worsened by decadal changes in weather patterns. The USFWS (1995) suggested two consecutive years of extreme flow in the Sacramento River could result in serious risk of extinction of delta smelt. This position may be reasonable given the life history characteristics of delta smelt. The short life span, approximately one year, and relatively low fecundity contributes to the risk of extinction if several years of extremely adverse environmental conditions occur.

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Comments on Appendix B:

This reviewer found four steps in the calculations in Appendix B: "Estimating the population of sub-adult delta smelt" where I believe the authors may have erred or may have omitted some

important assumptions. This may cast doubt on the population estimates. The issues are: 1) assumptions for expansion of density to population estimates, 2) regression of Kodiak trawl on fall midwater trawl density estimates, 3) scaling up zero values, and 4) correction factor of 0.25 for Kodiak trawl catch per unit volume. We thank the authors for providing adequate information to compute the total smelt in areas for the population estimates.

The first concern is the assumptions for the expansion of density data to population estimates are not stated explicitly. To make such expansions I believe the sampling design should have random station locations within areas (strata). The authors did not explicitly state these overall sampling design attributes. However, this reviewer believes that non-random station distribution (Sweetnam and Stevens 1993) is a concern for the approach used in this white paper.

The second concern is the units of the predicted values for the Kodiak trawl catch per unit volume from fall midwater trawl catch per unit volume. I did understand the application of the regression and agree that it is computationally easy to use the regression to make the correction. The sample size of the regression is only 12 paired tows so this correction factor is based on a very small sample size providing little confidence in this correction factor. The units in Appendix B Figure 2 for the Kodiak trawl and midwater trawl were "catch per 1000 m³". For delta smelt population estimates, table of adjustable parameters the volume thru the net is in "acre feet". The units are not equivalent as 1 acre ft = 1,234 m³.

The third concern is the scaling up of zero midwater trawl catches. The approach was adequately described and I was able to identify stations scaled up and stations not scaled up. Stations with zero catch that were adjacent to stations with non-zero catches were scaled up using the regression adjustment. The authors reasoned that for some of the zero catches delta smelt were present, but were not caught because of the inefficiency of the fall midwater trawling method. A biologically reasonable alternative is that the fish were contagiously distributed, that is unevenly distributed, and zero catches really represent zero delta smelt present. The contagious distribution is often apparent in schooling fish that are pelagic and feed on zooplankton. Furthermore, delta smelt are most abundant in low-salinity water associated with the mixing zone in the estuary, and if that zone is located in Suisun Bay, then most frequently in shallow water (Moyle et al. 1992).

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The fourth concern is the use of the correction factor of 0.25 applied to the Kodiak trawl catch per unit volume. The need for this correction factor is either not adequately explained so this reviewer could understand or the correction factor is redundant. The fall midwater trawl catch in fish per 1,000 m³ was used to predict the catch by the Kodiak trawl. The fall midwater trawl catch per 1,000 m³ was based on oblique tows from bottom to surface, and therefore, the catch per unit volume is representative of all depths. The regression equation shown in Appendix B, Figure 2 is used to correct the catch per unit volume of the fall midwater trawl directly to estimated catch per unit volume of the Kodiak trawl. If the correction factor of 0.25 is used in addition to the regression correction I believe it is redundant.

References

Bennett, W.A., and P.B. Moyle. 1996. Where have all the fishes gone? Interactive factors producing fish declines in the Sacramento-San Joaquin estuary. Pages 519-542 In J.T. Hollibaugh (ed.) San Francisco Bay: the ecosystem. Pacific Division, American Association for the Advancement of Science, San Francisco, California.

Hare, S.R. and R.C. Francis. 1994. Climate change and salmon production in the Northeast Pacific Ocean, Pages 357-372. In R.J. Beamish (ed.) Climate change and northern fish populations. Can Spec. Publ. Fish. Aquat. Sci. 121.

Herbold, B. 1996. Why we don't do population estimates for delta smelt. Interagency Ecological Studies Program for the Sacramento-San Joaquin Estuary Newsletter, Spring 1996.

Moyle, P.B., B. Herbold, D.E. Stevens, and L.W. Miller. 1992. Life History and status of delta smelt in the Sacramento-San Joaquin Estuary, California. Transactions of the American Fisheries Society 121:67-77.

Sweetnam, D.A. and D.E. Stevens. 1993. Report to the Fish and Game Commission: A status review of delta smelt (*Hypomesus transpacificus*) in California. California Department of Fish and Game. Candidate species status report 93-DS.

USFWS (U.S. Fish and Wildlife Service). 1995. Sacramento-San-Joaquin Delta Native Fishes Recovery Plan. U.S. Fish and Wildlife Service, Portland, Oregon.

USFWS (U.S. Fish and Wildlife Service). 1993. Endangered and threatened wildlife and plants: Determination of threatened status for the delta smelt. Federal register 58 (42): 12854-12864.

General comments:

The authors conducted an analysis to examine the state of the science in connection with the status of delta smelt, which is a listed species under the Endangered Species Act. The review of this paper is made difficult, because in my opinion the authors do not follow any standard scientific protocol used in peer reviewed journals. This paper lacks solid organization, which should identify the specific problems being addressed, clearly identify the authors' hypotheses, explicitly describe the methods they are using to test hypotheses, report and discuss results in a manner that can be repeated by other scientists, and lastly synthesize their findings with a thorough review of the literature on smelt biology, population dynamics, and conservation biology. I believe, from an organizational standpoint, this paper could be greatly improved if it was consolidated and followed the categories of topics I outlined in the previous sentence. I realize the purpose of this paper was not for publication; however the standard for good science should still be applied to this situation. In my opinion the objectives need to be clearly stated, the data and the methods clearly identified, the results repeatable, and the discussion placed in full context of the issues with thorough references to the prevailing literature on the subject. I will describe the specifics of these short comings below.

I feel another significant short coming of this paper was that the authors evaluated the trend of an abundance index for only one of the life stages available for delta smelt. There was no context provided from an ecological perspective that rationalized the author's use of only the autumn mid-water trawl. Moyle et al. (1992) were concerned that the autumn abundance indices may reflect the fact that the population has been confined to a restricted area where fish are concentrated. The summer tow net abundance index is believed to be one of the more representative indices, since the data has been collected over a wide geographic area for a long period of time (Federal Register Vol. 8, No.52, March 5, 1993). The authors concluded that the data show an increasing trend in smelt abundance since the mid 1980s, using a limited time series and only one life-stage abundance index.

I would recommend that the authors evaluate the population trend for a number of the available abundance indices and for the full time series of data. I believe comparison of the trends from the various indices and evaluating them over differing time series would increase the confidence in detecting a change in the population trajectory. For example, evaluating the trend in autumn index smelt abundance by fitting a simple linear model over the entire time series of data, yields a declining trend that is significant at $p=0.05$ level. The authors characterization of the population trend increasing (from autumn indices) appears misleading, because one could alternatively look at the 60% drop from 1970-1973 average to the present (1999-2002 average) or the 33% drop from the 1970-1980 average to the present. I would recommend an evaluation that looks at the trend over the longest time series, similar to estimating the population growth rate (λ) (Gotelli 1998). In fact, my estimate of λ for this population (using the autumn abundance indices supplied in the spread sheet we were given) is 0.975, a negative population growth rate. In order to capture a wide range of environmental conditions and provide a robust analysis, I believe the authors should use the longest time series of data that is available.

In the main paper the authors have two conclusion sections, which is highly irregular and confusing to me. The first set of conclusions address recent studies that posit an increasing trend

in smelt abundance, estimate total abundance for sub-adult smelt, and estimate a probability of extinction for delta smelt. The authors then state that the US Fish and Wildlife Service has not conducted these specific analyses. However, the authors at this point do not conclude why this is a problem relative to evaluating the status of delta smelt. The second set of conclusions claim that *the increase in scientific knowledge concerning delta smelt and impacts of water exports on the ecosystem present compelling arguments for removing delta smelt from the threatened species list.*

I found that in addition to some of the technical problems with the authors methodologies (see details below), two major components were missing. First, they did not present analyses or provide any reference or references that evaluated the impacts of water exports on the Bay-Delta ecosystem as it relates to delta smelt status. Therefore, I feel the authors' do not supply any supporting material for their claim: '*that since 1996 significant new information concerning... the impacts of water exports on fish populations have been developed*' (page 4, paragraph 4)'. There are a number of publications that establish the impacts of water exports on fish (Moyle et al. 1992, Meng et al. 1994, Stevens 1977, and Stevens et al. 1983). The authors provide no analyses, references for new information, or rationale to counter these previous findings. Secondly, the authors present results on estimating the extinction probabilities for delta smelt without presenting the methodology. I could not find a description of the methods used for estimating extinction probabilities in the main report or in any of the appendices. In fact, there were no references to any methods applied to estimate extinction probabilities. In order for the estimates of extinction probabilities to be evaluated I recommend the authors present the model or models used, describe how the population growth rates are estimated, the initial population size, the quasi extinction level used, and the variance about those parameters. In addition, in order to evaluate the robustness if the extinction probabilities a sensitivity analysis of extinction probability estimates to the various assumptions concerning model selection, model parameters, and the variance about those parameters would be required.

Appendix A:

The authors use four methods to analyze the trend in trawl abundance. However, they never state the hypotheses they are evaluating. It is unclear what time series they are trying to assess the trend over. There are no statistical tests used to determine a positive or negative trend and no explanation of the significance or power of the authors' results. The author's draw conclusions based on a visual examination of graphs for the various transformed trawl indices. The authors' did not provide a rationale for why they chose visual inspection over the application of a statistical test. There is no explanation given for why they selected to visually inspect a limited time series of the data. I feel this is an arbitrary approach to assess population trends. I would recommend a simple approach of evaluating the linear trend of the full time series of trawl surveys with regression techniques or the log transformed survey indices.

Appendix B:

The authors' attempt to estimate total abundance of the delta smelt population for 1996-1999. The estimates for abundance from trawl indices appear to me to be highly uncertain. I believe the authors would need to look at the sensitivity of abundance estimates for a number of key

assumptions. For example, applying the point estimates from the regression of mid-water trawl to Kodiak trawl estimates would greatly underestimate the confidence bounds for the population estimates. The regression analysis presented in figure 2 appears to be strongly influenced by a single value (0.6 mid-water trawl value –without this point r^2 drops to 0.0038). In addition, the estimates need to be made for the entire time series to understand the changes in population abundance. Lastly, the adjustment of the zero values for mid-water trawl samples appears arbitrary. I believe before attempting an adjustment to cells having zero values an assessment needs to be made to test for a contagious distribution of trawl values among sampling cells.

Appendix C:

The author attempts to enhance the estimates of abundance in appendix B to account for only the sexually mature portion of the delta smelt population. The uncertainty in these estimates increases, because of the fact that there are additional assumptions applied to the estimates of appendix B. Again, I believe the authors would need to look at the sensitivity of total abundance estimates to a number of key assumptions and also the addition of assumptions used to estimate the mature portion of the population. In particular, I am concerned about assuming all delta smelt reach adult life stage precisely on March 31 given the range of spawning time for this population.

In summary, I believe this report does not layout clear objectives and is poorly organized. The techniques used to analyze the data are ambiguous and lacking in rigor. The authors' do not rationalize their findings with either biological or ecological explanation. I feel that there was little review of the existing literature on either delta smelt ecology or analytical techniques for determining the status of threatened species. I did not find a single reference to any conservation biology literature. I would not characterize any of the techniques used to be cutting edge science. The authors did not discuss or directly challenge any of the alternative views existing in the literature regarding the relationship of delta smelt status to the ecology and threats imposed upon this population. The authors presented limited and questionable information on the population trend of delta smelt, population abundance, and extinction probabilities, which they believe warrants a change in status of the delta smelt population. However, they do not present any information on how the threats to the population has been removed or alleviated, which was key component of the five factor analysis used for listing this population (Federal Register Vol. 8, No.52, March 5, 1993). In short, I believe the authors would need to completely revamp their manuscript in order for these findings to be acceptable in a professional fisheries journal.