

USBR MLR Model and determined that it represented the best available scientific and commercial information. We retain our conclusions regarding our analysis and meta-analysis of the model's results, regardless of its developers' current desire to secondarily qualify its application.

*Peer Reviewer Comment 11:* A peer reviewer commented that it was unclear whether we had independently re-derived the CDFG/USBR MLR Model results submitted jointly by CDFG and USBR.

*Our Response:* We did not independently re-derive those results. We accepted the results presented in CDFG (2001) and USBR (2001) at face value, as they were developed by subject area experts within CDFG and USBR during a peer review and public comment process.

*Peer Reviewer Comment 12:* A peer reviewer believed that our that our statement, “\* \* \* [the] traditional [alpha-value] criteria assume a much higher standard of statistical power than the splittail data are able to meet \* \* \*” in our March 21, 2002, notice (67 FR 13095) is erroneous.

*Our Response:* We agree with the peer reviewer that in a strictly literal sense, the choice of an alpha-value criterion can be made without any regard for statistical power. However, in practice, researchers are concerned with both type I error (determined by the choice of an alpha value) and type II error (directly related to the statistical power of a study). When conducting our analysis, we made a conscious choice to use the more conservative nontraditional approach of using an alpha value of 0.20.

*Peer Reviewer Comment 13:* A peer reviewer asserted that the purpose of statistical hypothesis testing in the case of the MLR Model is to decide whether trends do or do not exist, not to evaluate gradients of reliability in evaluating trends.

*Our Response:* The CDFG/USBR MLR Model is a probabilistic approach to examining time trends, it is not a categorical “either/or” approach (as the respondent appears to assert). We chose to evaluate the probabilities associated with competing hypotheses concerning the abundance status of splittail. It is for this reason that we stated that all trends, not just trends meeting an arbitrary traditional confidence criterion (95 percent confidence, or alpha-value of 0.05) were evaluated.

*Peer Reviewer Comment 14:* A peer reviewer believes that a trawl's declining catch efficiency for adult splittail as compared to juvenile and YOY rendered trawl surveys less likely

to reflect trends and stated that adult and juvenile indices should not be combined. The peer reviewer also suggested that Bay Study OT and Fall MWT were more representative measures of abundance.

*Our Response:* While we concur that declining catch efficiency may be a characteristic of trawls, we do not agree that it should be used to exclude a trawl survey's data. Declining catch efficiency within a given trawl survey is expected to be uniform from year to year, thus rendering inter-annual analysis valid. Although an age bias will make data series for older age-class fish less sensitive for detecting change, it will not produce a long-term directional bias (*i.e.*, we have no reason to believe that the capture efficiency for older age class splittail is becoming progressively worse over time). Thus, any trends in the older age class data series with a substantive p-value can be viewed to be roughly as accurate and reliable as for the Age-0 class of splittail showing trends at comparable p-values.

We also concur that trawls' declining catch efficiency does preclude the combination of age class data. We report each index separately herein and do not combine adult and juvenile indices other than for meta-analytical purposes. We also acknowledge that, in certain situations, adult abundance for different age classes (of adults) is combined and reported because the data are collected in that manner, *i.e.*, salvage data are reported as Age-1 and as Age-2 and greater with no differentiation made for individuals greater-than Age-2 classes. Situations such as this represent a relic of the sampling methodology but remain the best available information. We continue to believe that as long as the degree of age-based capture bias is constant over a survey period, all age classes should show approximately the same trends, and that combining age classes for meta-level statistical analyses is not problematic.

We reiterate that the Suisun Marsh OT, which combines an efficient, bottom trawling technique with focused surveys in a small habitat at the core of the splittail's range, is the most likely to detect a trend and likely suffers from less sampling inefficiency than the Bay Study OT (low detection of splittail at periphery of range) and Fall MWT (unlikely to detect benthic fish and does not sample shallow water or near-shore areas).

*Peer Reviewer Comment 15:* A peer reviewer asserted that the peer review process for scientific publications doesn't necessarily ensure that published papers are unbiased, scientifically sound, and without errors.

The *Transactions of the American Fisheries Society* does not use the double-blind method for peer review. This issue was raised in regards to our past use of the Meng and Moyle (1995) methodology to determine splittail abundance.

*Our Response:* We agree with this assertion. Each piece of scientific work, whether a peer reviewed published paper or an unpublished, unreviewed, draft report, must be objectively evaluated for the scientific merit of its content alone. Peer reviewed publication provides no guarantee of scientific merit. The test of time, following publication, provides the ultimate measure of scientific merit. Indeed, subsequent iterative examination of the splittail's status has resulted in our abandonment of Meng and Moyle (1995), Sommer *et al.*, (1997) and our permutation-based exact calculations of p-values for stratified (as opposed to unstratified) Mann-Whitney U-tests (66 FR 43145).

*Peer Reviewer Comment 16:* A peer reviewer claimed we ignored the draft “White Paper” published by Moyle *et al.* (2001, in prep.)

*Our Response:* We use the various findings and hypotheses found in the draft and revised White Paper (Moyle *et al.* 2001 in prep.) extensively throughout this document.

*Peer Reviewer Comment 17:* A peer reviewer stated that the range of the splittail is wider than was previously thought.

*Our Response:* The greater range of the splittail was acknowledged in the January 12, 2001, notice (66 FR 2828). The above Background section of this final document contains a discussion of the range of the splittail.

*Peer Reviewer Comment 18:* Several peer reviewers felt that we should not classify the Yolo and Sutter bypasses as a threat to the splittail, as we did in the January 12, 2001, reopening of comment period (66 FR 2828), based primarily upon the data found in Sommer *et al.* (1997) and Sommer (2001a). The bypasses have demonstrated the capability of producing large numbers of splittail when inundated. One peer reviewer also felt that the bypasses cannot be considered a threat simply because the conditions could be better. Another peer reviewer claimed that current operations in the bypasses do not harm splittail or their habitat. Another peer reviewer felt that the bypasses are not to be considered a threat because even though their splittail habitat conditions are not optimal, they are still sufficient to provide substantial benefits to the species. Finally, another peer reviewer