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Peer Reviewed

Title:
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Journal Issue:
San Francisco Estuary and Watershed Science, 9(1)

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Publication Date:
2011

Publication Info:
San Francisco Estuary and Watershed Science, John Muir Institute of the Environment, UC Davis

Permalink:
http://escholarship.ucop.edu/uc/item/0rd2n5vb

Keywords:
delta smelt, Hypomesus transpacificus, management, water diversions, population ecology

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april 2011

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Modeling Delta Smelt Losses at the South Delta Expo Facilities

Wim J. Kimmerer[1]

AbStrAct

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KEyWorDS

delta smelt, *Hypomesus transpacificus*, management, water diversions, population ecology

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IntroDuction

I previously calculated proportional losses of delta smelt (*Hypomesus transpacificus*) to the water export facilities in the south Delta (Kimmerer 2008). Here I respond to Miller (2010), who presents analyses to show that my estimates of proportional losses were overstated. Miller raises some valid points but misinterprets some of my original analyses, and offers comments that cannot be addressed with available information. His critique also raises, albeit indirectly, two important general issues for quantitatively estimating the impacts of human activities: (1) how such estimates can and should be made in the absence of complete information; and (2) the nature of evidence useful in quantifying these impacts. I first discuss Miller's more specific comments, and then return to these broader issues.

Kimmerer (2008) calculated proportional losses during times when delta smelt are captured in substantial numbers at the fish salvage facilities, i.e., roughly January to March for adults and March to June for larvae and juveniles. The proportional losses for each life stage were estimated using a rather complex procedure to determine inputs to a survival model (modified from Equation 12 in Kimmerer 2008):

$$P_L = 1 - \prod_{d=1}^{D}\left(1 - \frac{\Phi_d}{N_d}\right) \quad (1)$$

where P_L is the proportional loss during the season of vulnerability, that is, the decrement in the population by the end of the season attributable to export pumping. D is the number of days in that season, N_d is the population size on each day, and Φ_d is the daily loss to the fish facilities, including pre-screen mortality and assuming no successful salvage. Note that this formulation ignores mortality not attributable to export pumping, which was taken into account in the original analysis (see below).

To clarify Miller's arguments and my responses, I consider the following components of these calculations: (1) efficacy of the sampling programs used to estimate model inputs; (2) estimating the number of fish lost to entrainment per day Φ_d; (3) estimating the population size N_d; and (4) accumulating daily loss over the season of vulnerability.

Efficacy of Sampling

Sampling for fish involves numerous assumptions about their distribution and about the efficiency of the sampling gear used in relation to the particular species and size of fish collected (Rozas and Minello 1997). Generally, in any sampling process, the confidence limits around the estimate being made decrease as the number collected increases. Thus, very small catches do not invalidate a sampling effort, but the results are more uncertain than with large samples.

Three sets of sampling data were used in the original analysis. The Kodiak trawl survey of adults is considered to be an effective method that is roughly 100% efficient for fish in the channels. The 20–mm survey of larval and juvenile fish is most efficient for fish larger than 20mm, but less so for smaller fish. Kimmerer (2008, Equation 20) used a logistic model to correct catches for low gear efficiency for smaller fish. This model is based on the fact that surviving fish must grow through all size classes, and that therefore the abundance of the poorly sampled smaller sizes is constrained by the abundance of larger sizes. The principal assumption of the logistic model was that parameters of the model were constant *within* years but could vary *among* years. Statistical error in fitting the model contributed to rather large uncertainties in proportional losses, as much as a three-fold uncertainty in the relative abundance of the smallest (5 mm) size class. This error was propagated through subsequent analyses of proportional losses.

Miller argues that low catches of smaller fish in the 20–mm survey should not be scaled up using catch-efficiency curves. This is equivalent to saying that gear efficiency cannot be determined for small fish, and implies that the numbers in each size class must be determined independently of those in other size classes. However, he offers no argument why the logistic function cannot be used to estimate abundance of all size classes, how the larger fish might have arisen except by growth of the smaller ones, or what is wrong with providing estimates based on small catches if confidence limits are included. Furthermore, he labels as "unreliable" data from some 20–mm stations with zero catch, without an adequate explanation of why such data should be considered unreliable; 73% of the 20–mm tows from 1995 through 2005 had no delta smelt, but these contribute to the calculations of means and other population parameters.

The south Delta fish facilities sample far more volume and capture larger numbers of fish than the field surveys, but capture efficiency—the ratio of salvage to entrainment—is low and variable. Delta smelt are unlikely to be guided by the louvers, which were designed for and are most efficient for salmon (Bowen and others 2004). Mark–recapture studies with adult delta smelt gave an average 24% recovery of fish at the federal fish facility that had been released in front of the primary louvers. Castillo and others (2009) conducted a mark–recapture study of delta smelt in Clifton Court Forebay and concluded that pre-screen mortality presumably from predation was the largest source of mortality for fish entrained in the forebay, and likely much larger than for other studied fish such as salmon. These studies provide limited support, though not quantitative information, for the low capture efficiency of the salvage facilities.

Kimmerer (2008) found that catch per volume of water sampled differed between the two salvage

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facilities on a daily basis, but that the overall mean differences were very small. This was the basis for using the same salvage efficiencies for both facilities. The salvage values are useful for indicating the timing and relative magnitude of entrainment events, but underestimate entrainment and mortality of delta smelt many-fold as discussed above. Without calibration to field data, salvage is not a useful proxy for mortality.

Miller reports a lack of correlation between salvage of young delta smelt and estimated flux to the pumps, concluding from this lack of relationship that the calculated flux is biased upward. The reason for this putative bias is not really explained. Three factors interfere with such a correlation: (1) the low and variable efficiency of the salvage facilities, (2) the high variability and small number of samples per survey (six) used in calculating the flux (see below), and (3) the distance from the sampling stations to the export facilities. None of these should introduce bias. I previously showed that the south Delta catches and salvage during springs of 4 years matched reasonably well in timing and magnitude but with a lot of error, and a low but non-zero correlation (Figure 7 in Kimmerer 2008). Thus, there is evidence for substantial statistical error but not for bias.

EStIMAtES oF FISh FLux

The flux or entrainment of fish toward the salvage facilities Φ_d comprises three factors: pre-screen mortality, losses through the louvers, and salvage. Because salvage is likely a small fraction of entrainment (see above), it gives a poor estimate of Φ_d, which must therefore be determined using other information, such as the density and rate of movement of fish in the waterways leading to the fish facilities.

The basis for such calculations (not spelled out by Kimmerer 2008) is a simple hydrodynamic flux calculation for a channel:

$$\Phi_C = A\left[(U + U_s)C - (K_h + K_s)\frac{dC}{dx}\right] \qquad (2)$$

where Φ_C is the flux of a substance or particles with concentration C, A is cross-sectional area of a channel, U is water velocity, U_s is additional velocity of C (e.g., due to swimming in the positive x direction), K_h is a horizontal dispersion coefficient, K_s is an additional dispersion coefficient due to randomly directed swimming, and the last term is the longitudinal gradient in C. If the gradient is small and the particles are passive, the flux is simply $AU_C = Q_C$, where Q is the volume flow rate. Kimmerer (2008) used this to calculate the flux of young smelt with Q represented by the southward net flow in Old and Middle rivers and C by the catch per unit volume at six 20–mm stations in the south Delta. This calculation was not possible for adults because of low (often zero) catches, so the catches were used to calibrate salvage density (fish per unit volume of water) to catch per volume in the Kodiak trawl, and this calibration factor was applied to all salvage data to estimate flux.

Miller argues that since fish are not passive particles this calculation is invalid, but offers no alternative way to compute the fish flux. Larval fish have very limited swimming abilities and are essentially passive particles before they obtain a swim bladder, after which they can affect their position only through vertical migration. Tidal vertical migrations were found in pelagic fish larvae in the low-salinity zone but the sample size for delta smelt was small, and migration was not detected (Bennett and others 2002). Even the fish and copepods that demonstrably migrate tidally can overcome net seaward flow only in water that is stratified in salinity (Kimmerer and others 1998), which is not the case in the south Delta. The smelt that leave freshwater in early summer are post-larvae over 20mm long with developed swim bladders and initial distribution near the surface (also in the low-salinity zone, Bennett and others 2002). If this behavior applied in freshwater it would move most of the population westward to their brackish rearing habitat except those in the south Delta, which would move toward the pumps. Thus, during spring they can be treated as passive particles at the scale of the south Delta, and Equation 2 applies to these fish. Miller's argument implies that the fish are somehow escaping the

southward flow of Old and Middle rivers, but there is no evidence that they are capable of doing that, nor do environmental cues exist that would persuade them to orient away from the export facilities.

Adult smelt move up-estuary during their spawning migration and are, therefore, demonstrably capable of moving against the net downstream flow in the Delta. However, high salvage numbers indicate the existence of a large southward flux of adults. I calculated an efficiency Θ (Equations 16 and 17 in Kimmerer 2008) relating salvage to the estimated fish flux based on the Kodiak trawl samples in the south Delta, and applied that to salvage to get the fish flux for all days of the season.

Miller argues on several grounds that Θ was overestimated. The most cogent argument is that there were too many zeros in the data to use a Poisson model to fit the data. I therefore re-fit the model in Equation 17 (Kimmerer 2008) with a zero-inflated Poisson model (Lambert 1992) which has two parameters; the Poisson mean and the proportion of excess zeros. This model was fit using a Bayesian approach in WinBUGS (Lunn and others 2004) using fitting and model checking procedures in Kimmerer and Gould (2010). The resulting estimate of Θ was 22 with a 95% credible interval of 13 to 33. This estimate is about 76% of the previous estimate but with better resolution. Estimates of mean adult loss in Kimmerer (2008) should, therefore, be reduced by 24%. Miller also argues that the data are contaminated by a single high catch of 17 fish. This might be true if the model were improperly cast as a linear regression, but for a properly formulated model it poses no problem. In any case, the analysis should be based on the data at hand.

Miller also argues that the adults are not passive particles, implying that they can overcome the effects of net flow in the south Delta. That is, the term U_s in Equation 2 may be negative, reducing the actual fish flux Φ_C. In that case salvage would be lower than expected if U_s were zero, and the effect of a negative U_s would be accounted for in the calculation of Θ.

According to Miller, Old and Middle river flows are unrelated to salvage of either adult or young delta smelt and therefore are insufficient for calculation of fish flux. The relationship between these flows and salvage is actually quite obvious, if nonlinear and noisy (Figure 4 in Kimmerer 2008): when these rivers flow southward, salvage is often high, and when they flow northward, salvage is either mostly zero (juveniles, adults in the state facility) or sometimes non-zero (adults for the federal facility only). The latter case is likely due to U_s in Equation 2 being positive for some fish, i.e., toward the export facilities. Thus, while the fish are not entirely behaving as passive particles, their behavior is not necessarily oriented to take them away from the facilities.

The calculations of proportional losses of young smelt were remarkably consonant with predictions made using the DSM2 particle tracking model (Figure 16 in Kimmerer 2008). This supports the use of Old and Middle River flows for the calculations, and the assumption of passive transport for this life stage. Furthermore, the estimate of Θ above is, if anything, low—considering the estimates to date of pre-screen losses and losses through the louvers.

Delta smelt are more abundant where the water is turbid (Feyrer and others 2007) and, therefore, salvage and salvage-related losses should be more predictable using information about turbidity than without this information. This issue arose after I had finished the final draft of the 2008 paper, but, in any case, turbidity data for the south Delta were not available for the time–period of this study. Ignoring it introduces error in the calculations but there is no reason to expect bias, since all the calculations were based either on salvage (adults) or fish collected in the south Delta (juveniles).

SIzE oF thE popuLAtIon

The denominator in Equation 1 is essentially the mean catch in all samples times the volume over which those samples were taken. An alternative is to calculate mean catch per trawl by region of the estuary, multiply by area or volume of each region, and sum the result to get an index of abundance. The assumptions underlying these two approaches are somewhat different, but there are no data to suggest one is superior to the other. The annual abundance indices in several monitoring programs are calcu-

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lated by region, but simple mean catch per trawl over all stations is closely correlated to these indices (Kimmerer and Nobriga 2005). Thus Miller's calculations of population size using a region–by–region approach are unlikely to be much different from the simpler calculation in Kimmerer (2008).

The fish fluxes Φ were calculated so that efficiency of the sampling gears was factored out of Equation 1. Therefore, the remaining issue for this part of the calculation is whether the samples in the south Delta represented the population there to the same degree that sampling throughout the Delta represented the overall population. Catchability is unlikely to differ between the south Delta and elsewhere (and we have no data either way on this), so the degree of representation boils down to whether the spatial coverage of sampling is adequate to represent the population.

Miller argues the contrary on the basis that high catches of adults in the Sacramento River Deep Water Ship Channel (sampled beginning in February 2005) indicate that most of the fish are in that region and are, therefore, under-sampled. Most of my analyses were for earlier years; furthermore, most of the salvage occurred between mid-December and the end of February (Figure 11 in Kimmerer 2008), when relatively few fish are yet in the north Delta (Figure 1). It does appear that more adults are in the north Delta during more recent years, mainly in the later surveys.

Miller makes a similar argument for young fish, although the argument is muddied by a claim that the 20–mm survey collects too few fish to provide a reliable index of total population size, based on projections of abundance of young fish from calculated abundance and assumed reproductive success of adults. If this were true it would call into question the results of all sampling programs. The stronger part of Miller's argument is the same as for adults: i.e., that a greater proportion of the population is in the north Delta and that it has been under-sampled. The data show an increasing proportion of the total catch in the north Delta stations (Figure 2) as the total catch has decreased. However, that proportion was never more than 8% during the period of this study.

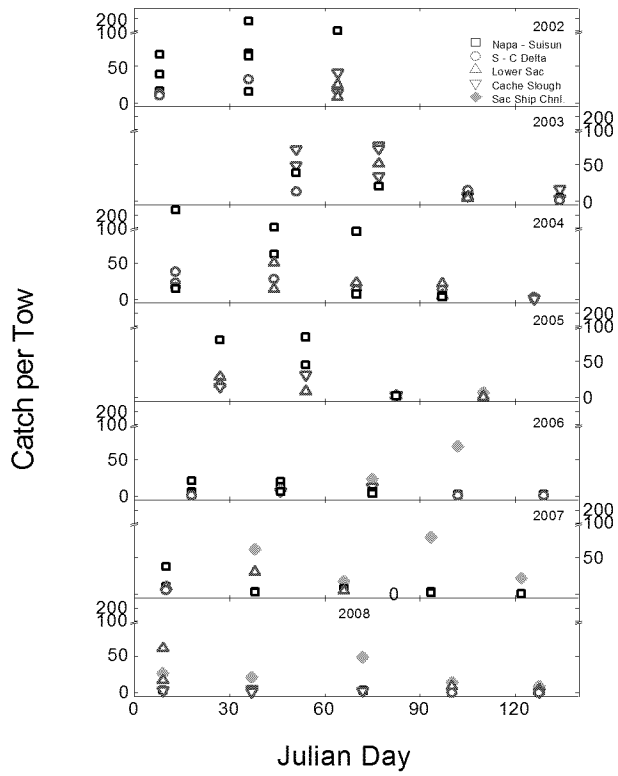

Figure 1 Delta smelt catch per tow in the Spring Kodiak trawl survey for the five stations with the highest catches during each month's sampling, by year. These stations made up at least 62% of the total catch of the respective surveys. Symbols indicate sampling regions, with stations included as follows: Napa–Suisun: stations <699 plus 801; South–Central Delta: 802 to 999; Lower Sacramento River: 704 to 707; Cache Slough area: 711 to 716; and Sacramento Ship Channel: 719, sampled beginning February 2005.

The apparent northward shift in distribution of adult and young smelt means that the exposure of the delta smelt population to export pumping is less in recent years than it was during the time period of my study. Although this might be considered a benefit, conceivable mechanisms for this shift are not promising for the long–term maintenance of the species. One possible such mechanism is that the south Delta is occupied less by delta smelt because of a degradation of the habitat (e.g., by increasing water clarity). The implications of that for proportional losses to exports would depend on the mechanism keeping abundance low in the south Delta, which are not yet known.

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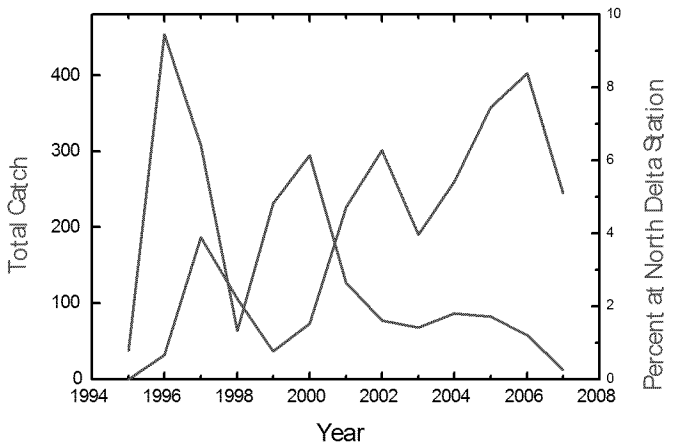

Figure 2 Delta smelt catch in the 20–mm survey. Heavy blue line, left axis: total catch in all samples; thin red line, right axis: percent of catch from Station 716 in Cache Slough in the north Delta. Note that catches at Station 719 in the Sacramento River Deep Water Ship Channel have been high since sampling at this station began in 2008, but there is no information on whether this is a sampling artifact or a result of smelt movement.

AccuMuLAtIng LoSSES ovEr thE SEASon

Accumulating losses means calculating the proportional difference between the population that would have existed at the end of the exposure season with and without export losses. This requires that the relative size of the vulnerable population and other mortality be taken into account. For example, a high daily fractional loss early in spring when few young fish had hatched will have a smaller effect on ultimate population size than a high loss after all the fish had hatched.

Equation 1 could be parsed in a number of different ways, but the end result would not be very different using the same values of the fractional loss terms. The calculations are made a bit more difficult by the need to account for natural mortality of juveniles, as explained by Kimmerer (2008). Leaving mortality out of the calculations results in a modest increase in the calculated seasonal losses (Figure 15 in Kimmerer 2008). Although Miller argues that mortality is unlikely to be constant in space or time, the effects of such undeniable but unmeasured variability cannot, therefore, be very large. Since losses of larvae and juveniles were based on catches in the south Delta rather than salvage, an excess of mortality in the south Delta relative to the entire habitat would bias the loss estimates low, not high as Miller claims.

ALtErnAtIvE ApproAchES to EStIMAtIng Export EFFEctS

To date, nobody has reported a relationship between any measure of flow toward the export pumps or losses of delta smelt, and either subsequent population abundance indices or ratios of successive indices. Miller argues that this lack of statistical link to population estimates is evidence that losses calculated mechanistically are unimportant compared to other effects such as food limitation.

This is part of a broader issue: the nature of evidence to be used in estimating the magnitude of human impacts on a biological population. Fundamentally, such impacts can be estimated through correlative measures, or they can be determined mechanistically. I do not believe that Miller is arguing against the use of mechanistic approaches (as some have done), since far more of our current scientific understanding in most fields of science rests on mechanistic than on correlative analyses.

Mechanistic approaches are based on known or inferred processes that influence the population in some way. In the specific case of estimated mortality to a fish population, the key issue is whether subsequent density dependence compensates for that mortality. If not, it is tautological that mortality will proportionally reduce subsequent population size.

Density dependence is a controversial topic mainly because of statistical difficulties, although conceptual problems also contribute. Compensatory density dependence can arise through a wide variety of causes, most involving food supply or predation (Rose and others 2001). Density dependence in striped bass in the San Francisco Estuary apparently compensated for very high losses to the export facilities, at least during a period of relatively high abundance (Kimmerer and others 2000).

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Density dependence in stock–recruit relationships for delta smelt were driven largely by high values in the 1970s, although some evidence for density dependence remained in the data after 1981 (Bennett 2005); however, these relationships and the influence of environmental factors on them have likely changed over the intervening decades. The key question for interpretation of export losses of delta smelt is whether density dependence is strong in the post-decline population. This seems unlikely: since 2002 abundance of delta smelt has been too low for most potential mechanisms for compensatory density dependence to exert much influence. If so, the delta smelt population does not compensate for reductions in abundance by, e.g., increased fecundity or reduced mortality. Therefore, losses at any life stage permanently and proportionally reduce the population from the trajectory it would have otherwise have followed.

Correlative measures can be useful to the extent that they offer statistical support for a relationship. However, they cannot establish cause. More importantly, there is a clear difference between a finding that a result does not meet statistical standards of significance, and concluding it is not important. Thus, in making such an argument it seems important to determine what level of impact could be detected by correlative methods.

I determined this level through simulations, assuming density-independent population processes by the arguments above. I used the observed ratio of the fall midwater trawl index to the previous year's index as a stock–recruit index that should be sensitive to losses in the spring. The percentage loss in a given year was set as:

$$P_L = P_{max} \left(\begin{array}{c} 0 \text{ if } OMR \geq 0 \\ \dfrac{OMR}{OMR_{min}} \text{ if } OMR < 0 \end{array} \right) \quad (3)$$

where P_{max} is the maximum percentage loss in any year (a free parameter in this simulation), OMR is the mean flow in Old and Middle rivers in spring (negative is southward), and OMR_{min} is the minimum OMR flow (i.e., the maximum southward flow). OMR flows were determined for each spring as described in Kimmerer (2008). In this equation, P_L is zero for positive OMR, and scales linearly with negative OMR to a maximum at P_{max} when OMR = OMR_{min}. Alternative scaling would affect the quantitative results but not the qualitative conclusion.

For each year, the simulation ran using flow data from 1981 through 2006, with each year's fall population reduced by the simulated proportional loss during the previous spring. The choice of years to simulate was made to get a representative range of OMR flows, not to simulate an actual population trajectory, and the simulation was intended only to investigate the effects of export losses at low population size where density dependence would have a minimal effect. The flows were randomized among years to eliminate potential confounding factors from actual annual flow patterns. Then, for each integer value of P_{max} from 0 to 100% a regression was calculated between southward Old and Middle river flow (the quantity in parentheses in Equation 3) and the log of the stock–recruit index. The intent was to determine how large P_{max} had to be before losses become detectable in regression analyses.

The results (Figure 3) show that the losses were not generally detectable in the regression until P_{max} reached about 60% to 80%. The levels of loss reported by Kimmerer (2008) were obscured by interannual variability in nearly all simulations, and maximum losses less than 20% were undetectable. Yet a P_{max} of 20% (mean annual loss of ~10%) results in a 10-fold reduction in population size by the end of the 26–year simulation (Figure 3). Repeating the above simulation 10,000 times with P_{max} = 20%, the upper 95% and 90% confidence limits of the regression slope excluded zero (i.e., was statistically detectable) in 5% and 9% of the cases, respectively. Thus, a loss to export pumping on the order reported by Kimmerer (2008) can be simultaneously nearly undetectable in regression analysis, and devastating to the population. This also illustrates how inappropriate statistical significance is in deciding whether an effect is biologically relevant (Stephens and others 2007).

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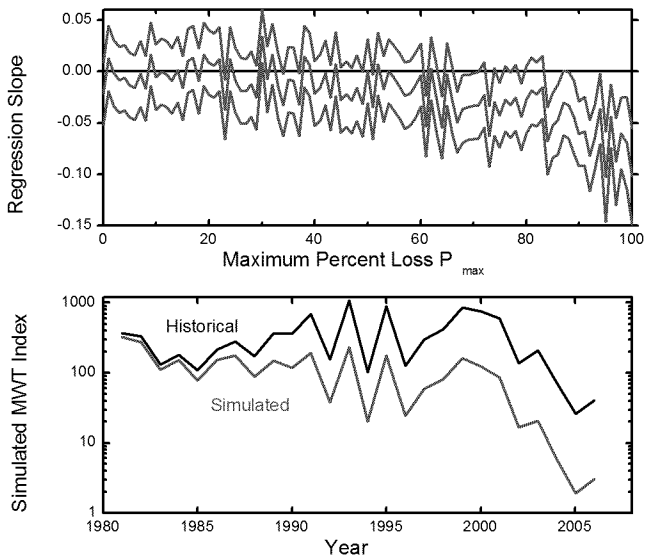

Figure 3 Results of simulation of ability to detect export loss through regression analysis. Upper panel: individual simulation results giving the slope (thick blue line) and 95% confidence limits (thin red lines) for regressions of the stock-recruit index on southward OMR flow. Lower panel: trajectory of the fall midwater trawl index (upper line) and the same index with a 20% P_{max} value imposed for the entire time series (mean P_L ~10%). This is for illustration only (see text), and does not imply anything about the cause of the decline in delta smelt.

conclusions

Miller raises some valuable points about the data and methods used in calculating proportional losses. He also introduces new developments in understanding (e.g., turbidity effects) and in the delta smelt population (e.g, spatial distribution) that occurred recently. I do not believe these points cast doubt on the overall conclusion of my paper, which is that export–related losses to the delta smelt population during some of the years analyzed were substantial.

I previously reported that export effects had little effect on the striped bass population because of density dependence at levels of population abundance that existed up to 1995 (Kimmerer and others 2001). I also previously determined that export losses of mysids (*Neomysis mercedis*) were unlikely to be important to that population (reported by Orsi and Mecum 1996). During my work on the Environmental Water Account, I continually but unsuccessfully challenged my colleagues in the resource agencies to determine the effect of export pumping on fish populations, and therefore the magnitude of the benefit that the Account was having on fish (see Brown and others 2008). Therefore, my labors on export losses of delta smelt began with a strong skepticism about the importance of these losses, and ended with considerable surprise at their magnitude.

All of that said, neither my paper nor this exchange is the final word on this subject. More sophisticated statistical tools and models could and should be brought to bear on what controls delta smelt abundance, and these should be updated as new data become available. Information from new studies (e.g., Castillo and others 2009; Grimaldo and others 2009) and based on more recent distributional data should also be considered, both in refining understanding of influences on the smelt population and in assessing changes in the population itself.

acknowledgments

I thank William (Bill) Bennett for reviewing an earlier version of this paper, and William (B.J.) Miller for helpful comments and clarification.

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