AGENDA D-1(e)
APRIL 1998

# MEMORANDUM

ESTIMATED TIME
6 HOURS
(all D-1 items)

TO: Council, SSC and AP Members

FROM: Clarence G. Pautzke
Executive Director

DATE: April 14, 1998

SUBJECT: Seasonal/Area Apportionment of Atka Mackerel

## ACTION REQUIRED

Initial Review of an amendment to further apportion the Atka mackerel TAC in the Aleutian Islands.

## BACKGROUND

In 1990, the Steller sea lion was designated as threatened under the ESA. Critical habitat was designated in 1993 and includes marine areas within 20 nm of all rookeries and major haulouts west of 144°W. In 1997, the species was split into two separate management populations on the basis of genetics information. The listing status of the western population (i.e., west of 144°W longitude) was changed to endangered while the status of the eastern population remained as threatened. The western population of Steller sea lions (i.e., west of Cape Suckling or 144°W longitude) has declined by 80% or more since the mid 1960s.

Since most of the recent fishery removals of Atka mackerel occur within Steller sea lion critical habitat, preliminary analyses have suggested that the fishery is capable of creating localized depletions of an important Steller sea lion prey where sea lions are likely to forage. The purposes of this proposed action are to reduce the probability of fishery-induced localized depletions of Atka mackerel and reduce the probability of adverse modification of Steller sea lion critical habitat as required by the ESA. Several alternatives were examined.

Alternative 1: No Action: no change in management of the fishery.

Alternative 2: Seasonal A:B split (50%:50%) in TAC.

Alternative 3: Seasonal A:B split (50%:50%) in TAC, plus additional split of TAC to subareas inside and outside of Steller sea lion critical habitat. Possible variations include:

> Critical habitat split (e.g., 40% inside: 60% outside), in areas 542 and 543 during both seasons. Area 541 would not be split for critical habitat because of the 20-nm no-trawl zone during the A season.
>
> Critical habitat split as described in option 1, but split achieved in incremental annual changes (e.g., 10-20% per year) from current split (about 80% inside: 20% outside) to target split.
>
> Critical habitat split of 0% inside: 100% outside.

<u>Alternative 4:</u> Seasonal split in all three regulatory areas, if necessary, and critical habitat split in areas 542 and 543 only, plus setting of maximum TAC in any season-area based on estimates of initial biomass from Leslie regression analyses and application of a target harvest rate.

<u>Alternative 5</u>: Seasonal split and geographic rotation. Establish TAC for each regulatory area, begin with a time-limited season (e.g., 5 days) for 1/3 of TAC in regulatory area 541, then close area 541 and move to area 542 for a second time-limited season on 1/3 of TAC for that area, and then shift to area 543. When all three areas were fished, then return to area 541 and start the cycle again.

<u>Alternative 6</u>: Voluntary fleet distribution of effort throughout regulatory areas throughout year.

An executive summary of the analysis is attached as <u>Item D-1(e)(1)</u>. Tim Ragen (NMFS-AKRO) will be on hand to discuss his results.

[Excerpted from EA/RIR/IRFA to Reapportion TAC of Atka Mackerel and Reduce Fishery Effects on Steller Sea Lions]

## Executive Summary

This amendment is intended to mitigate the potential for competition between the Steller sea lion and the Atka mackerel fishery. In 1990, the Steller sea lion (*Eumetopias jubatus*) was designated threatened under the Endangered Species Act of 1973 (ESA). The designation followed severe declines throughout much of the Gulf of Alaska and Aleutian Islands region. In 1993, critical habitat for the species was defined to include (among other sites), the marine areas within 20 nmi of major rookeries and haulouts of the species west of 144°W longitude. In 1997, two separate populations were recognized, and the western population (west of 144°W longitude) was reclassified as endangered. The estimated number of Steller sea lions in the western population has declined by more than 80% since the mid 1960s. The ultimate cause of the decline is unknown, but lack of available prey may be the most important proximate cause.

The Atka mackerel fishery occurs in shallow waters of the Aleutian Islands, largely within areas designated as Steller sea lion critical habitat. The fishery was dominated by foreign vessels until the early 1980s, then joint venture operations until late 1980s, and finally domestic vessels (exclusively) in the 1990s. From the late 1970s to 1991, the annual catch of Atka mackerel in the Bering Sea / Aleutian Islands (BSAI) region fluctuated between 12,000 mt and 27,000 mt, with the exception of the period from 1984 to 1987, when annual totals were in the range of 30,000 mt to 38,000 mt. In 1992, the catch increased to 50,000 mt and then continued to increase until it reached 104,000 mt in 1996. In 1997, catch decreased to 66,000 mt, and the 1998 TAC was set at 64,000 mt. Since 1977, the portion of catch taken annually within Steller sea lion critical habitat has varied from 15% to 98%, with an average of 71%. The marked increase in the annual catch in the 1990s, and the high percent of the catch generally taken within Steller sea lion critical habitat has resulted in a marked increase in the amount (tons) of fish taken from areas considered essential to the recovery and conservation of the Steller sea lion, thereby increasing concerns that the fishery may compete with Steller sea lions.

The potential for competition between sea lions and groundfish fisheries has been recognized in previous evaluations of the fisheries under the ESA (i.e., Section 7 consultations). The resulting biological opinions have expressed concerns that BSAI groundfish fisheries may compromise the foraging success of Steller sea lions by creating localized depletions of prey. Recent statistical evaluations of catch-per-unit-effort (CPUE) at various sites in the 1990s have indicated that the Atka mackerel fishery has led to localized depletions of Steller sea lion prey (Fritz, unpubl. report), thereby increasing evidence for competition.

Unequivocal proof that the Atka mackerel fishery is impeding the recovery of the Steller sea lion is not possible at this time, nor is it likely to be possible in the near future. Unequivocal proof will require a much more complete description of both sea lion foraging patterns and Atka mackerel stock dynamics. Progress has been made in describing both but, given the complex and difficult nature of the task, complete descriptions cannot realistically be expected in the near future. Importantly, management of the Steller sea lion and the Atka mackerel fishery can not wait for complete scientific descriptions and proof. In the face of uncertainty, management is nevertheless required by the ESA to ensure that the fishery does not contribute significantly to the decline of the Steller sea lion, or impede its recovery.

The purposes of this amendment, then, are to reduce to insignificant levels 1) the probability of fishery-induced localized depletions of Atka mackerel and 2) the probability of adverse modification of Steller sea lion critical habitat through excessive removal of prey, as required by the ESA.

Six alternatives are presented for discussion, including the status quo (no change in management) and voluntary redistribution of fishing effort by fishery participants. The remaining four alternatives are all based on time-area management of the fishery. None of these alternatives involves a reduction in TAC or a change to the manner in which the TAC is set. The key distinguishing elements of these alternatives are 1) whether they involve a seasonal split, 2) whether they involve an apportionment of the TAC inside and outside of critical habitat, 3) the extent to which they use past commercial and scientific data to establish TACs for subareas and seasons, and 4) the number of TAC releases associated with each alternative. As noted on the title page, these alternatives are still under review, and will remain so until after the April Council meeting, to ensure that the Council has had full opportunity to participate in the discussion and development of this amendment.

March 31, 1998
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Mr. John Gauvin
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John,

Here are some comments on the manuscript entitled "Do trawl fisheries off Alaska create localized depletions of Atka mackerel (*Pleurogrammus monopterygius*)?" by L. W. Fritz as per your request.

The manuscript describes a Leslie-Delury type depletion analysis applied to commercial fishery data for Atka mackerel. My first impression of this work was that it was a conventional application of a method commonly used in assessing fish abundance. But, upon reviewing some related papers and in looking at statistical output provided to the Groundfish Forum by the manuscript's author, I realized the analysis had several serious shortcomings that may misrepresent the conclusions drawn in answer to the title question.

First, the paper only presents analyses where statistically significant declines exist. On examining computer outputs (provided by the author to Groundfish Forum), I soon came to realize that there exist nearly twice as many other analyses (for the same study areas, for sometimes the same and sometimes different years) that show *no* significant decline in CPUE over the period of the fishery. In any paper addressing the question of the significance of depletion in an area, both the non-significant as well as the significant results should be shown for the areas and times surveyed. Without the uniform presentation and treatment of all the relevant data the reader will draw the biased conclusion that such analyses always show a decline, whereas in this case the majority of the data show no decline.

Second, although a decline over time is observed intra-annually for the data presented in the manuscript, the relative density of Atka mackerel appears to return to (or in some instances above) the nominal value by the following year (see any of the depletion figures accompanying the text). This would imply a decline locally (and only for certain times and areas), and not globally. How local this phenomena is, if it exists at all, becomes the central issue. The argument, made in the manuscript, is inconsistent on this, arguing first that the depletion is local (via the Leslie model) and then that it is more global (that is that mackerel are migrating in from adjacent areas, specifically sea lion habitat areas). If depletion is indeed local and movement to replace harvest is slow, then the local effect should not greatly affect adjacent areas. If, on the other hand, re-population is quick, then it is important to know from where the mackerel are recruited and whether this affects density globally. It is implied in the manuscript that re-population locally is slow, but that mackerel, when they are recruited, are recruited from the trawl exclusion zones.

There is no information in the manuscript to support this however. Historic fishing records (Fritz and Lowe manuscript), on the other hand, would seem to indicate that mackerel populations exist elsewhere (locally in deeper waters for example), which could easily provide an alternative source of new recruits to harvested areas. There is not enough information provided in the manuscript to address this critical point.

There are a number of other technical and biological issues that I think must play an important role and should be considered as well.

Based on data provided in the manuscript it appears that fishing occurred in and around the Atka mackerel spawning period during the years covered by the study. McDermott and Lowe (manuscript) have found that sex segregation occurs during the spawning period. As these authors note this likely could affect the results drawn from analyses conducted using trawl CPUE for this species. For example, the females may aggregate in areas accessible to the fishery as part of their spawning behavior. If they then depart as part of this same behavior, the result would be a decline in CPUE that is unrelated to fishery removals. And even if the decline is real it may only represent a portion of the population (i.e. only females) and thus not reflect the population as a whole. Furthermore, it appears that the females increase in size more dramatically than do the males during this time period (Fritz and Lowe manuscript). I speculate that this may in part be due to reproductive development. I am curious to what degree changes seen in CPUE (tons per effort expended) over the intra-annual time period reflect changes in individual size rather than changes in population size.

Another more technical issue is the process of pooling the data to form data points for the regression. This process appears to vary from one data set to the next depending upon the intensity of fishing in that area. This pooling of data to form regression points can affect trends and significance of outcomes, but to what degree it affects the conclusions drawn is not clear based on what was presented in the manuscript.

A bit off the central topic of the manuscript, it seems to me that the Steller sea lion population began its decline well before the Atka mackerel fishery began to build and during a time when the mackerel population remained steady or increased. If that is the case, then Atka mackerel may not be the limiting component of the system.

I hope these comments help to clarify some of the scientific and statistical issues raised by this manuscript. Please let me know if you require further comment or clarification.

Patrick J. Sullivan

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Fritz, L. W. and S. A. Lowe. Seasonal distributions of Atka mackerel (*Pleurogrammsu monopterygius*) in commercially-fished areas of the Aleutian Islands and Gulf of Alaska.

McDermott, S. F., and S. Lowe. The reproductive cycle and sexual maturity of Atka mackerel (*Pleurogrammus monopterygius*).

# Review of "Do Trawl Fisheries off Alaska Create Localized Depletions of Atka Mackerel (*Pleurogrammus monopterygius*)?"

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## Executive Summary

This brief report provides a review of the analysis by Fritz (1998) "Do Trawl Fisheries off Alaska Create Localized Depletions of Atka Mackerel (*Pleurogrammus monopterygius*)?" We conclude there are numerous possible problems with the Fritz analysis. One of the more alarming problems, however, is the temporal aggregations considered. The temporal aggregation selected by Fritz may, in fact, be forcing the results. We simply cannot determine a reasonable basis for the different time periods considered in the study. We also find potential evidence of heteroscedasticity and serial correlation. Without a formal analysis of the data, however, we are unable to state whether or not heteroscedasticity and serial correlation are actually present. We also suggest that, although it is common practice to estimate the Leslie specification, used by Fritz, by ordinary least squares, the covariance between the error term and one of the right-hand side variables (one-half of current catch) does not equal zero; thus, the required conditions for obtaining best linear unbiased estimators are not satisfied. It is also highly advised that a full realm of regression diagnostics which include an analysis of influential data points be conducted. It appears that for some of the estimates, two observations are determining the statistical relationship. We also suggest that a Monte Carlo analysis, as recommended by Hilborn and Walters (1992, page 395), be conducted. Moreover, we recommend that additional analysis for an open population be conducted to better assess the possibility of depletion. There also is the problem that, for some areas, the confidence intervals for the estimated initial stocks are so large that it is erroneous to conclude localized depletion based on the results of Fritz. Last, we question the applicability of the analysis because of inconsistent results over different areas and different time periods. That is, the statistical results are significant for some areas and time periods but insignificant for other areas and time periods; it is particularly alarming when the estimates are significant for one area and time period but insignificant for the same area but a different time period. A remaining major issue is the appropriateness of using the fishery-dependent measure of resource abundance; Fritz provides no assessment of whether or not the CPUE is a realistic measure of abundance. Standardization of CPUE relative to area would provide a simple indicator of the appropriateness of the CPUE measure.

## Introduction

The report "Do Trawl Fisheries off Alaska Create Localized Depletions of Atka Mackerel (*Pleurogrammus monopterygius*)?" by Fritz (1998) provides estimates of the initial standing stock of Atka mackerel for various Alaska resource/fishing areas. Based on the Leslie-Davis (1939) analysis presented in the report, Fritz concludes that in the Aleutian Islands, the fishery utilizes areas preferred by adult Atka mackerel and that these areas are replenished over time. Fritz also concludes, however, the estimates of the initial standing stock in one Gulf of Alaska area suggest that the local Atka mackerel population size decreased significantly from 1993 to 1994 and that the Gulf of Alaska population may be less resilient to exploitation than that in the Aleutian Islands. Last, Fritz concludes that the repopulation pattern of the Aleutian Islands could disadvantage Stellar sea lions and other Atka mackerel predators. In total, Fritz claims the Leslie regressions support the notion that local Atka mackerel biomass declined significantly over time in eight of the nine time-area mackerel fisheries analyzed.

Out of concern about the possible ramifications of Fritz's conclusions, the Groundfish Forum, Inc. requested Drs. Hoenig and Kirkley to review the report by Fritz. Initially, we provide an overview of the Leslie model. We next discuss the specification used by Fritz. We subsequently review the data and particularly the temporal aggregations used by Fritz. Regarding the statistical analysis, we consider the following potential problems: (1) implicit simultaneous equations bias caused by current catch appearing on both sides the Leslie equation; (2) the potential for heteroscedasticity because of dividing all variables by fishing effort or the large value of the independent variable (cumulative catch plus one-half of current catch) relative to the dependent variable (catch per unit effort); (3) the possibility of serial or autocorrelation; (4) the possibility of using a constrained nonlinear vs. unconstrained linear model; (5) the absence of a rigorous analysis of regression diagnostics (e.g., outliers, leverage values, and influential values); and (6) the apparently large confidence intervals for estimated standing stock of some areas. We also consider the possible need to modify the Leslie model to deal with recruitment and natural mortality; the paper provides no real evidence that the population is closed (no immigration and no emigration), which is a requirement for using a closed population depletion model.

## Depletion Models

The peer-reviewed literature contains a wide array of possible depletion models as well as modifications of the DeLury and Leslie-Davis models (e.g., see Hilborn and Walters, 1992). The initial DeLury model is discussed in DeLury (1947 and 1951); the Leslie or Leslie-Davis model is discussed in detail in Leslie and Davis (1939). The two models are similar in that they both attempt to develop procedures for estimating the initial population and total standing stock of a resource. The Leslie-Davis model offered an approach for determining the absolute number of rats on a given area, and DeLury was specifically concerned with estimating the population of fish. The two procedures, however, are routinely applied to fish populations.

Hilborn and Walters (1992) describe the two approaches as depletion estimators. As per Hilborn and Walters (p. 391), "the concept behind depletion estimators is to examine how measured removals of fish influence the relative abundance of fish remaining in the total stock or in a designated depletion study area." The two approaches should provide similar estimates of the initial stock.

To use the Leslie model for closed populations (no new recruits or immigrants and no losses to natural mortality or emigration), Fritz, needed to assume:

$$N_t = N_0 - K_t$$

where $K_t$ is the cumulative catch ($\sum_{t=1}^{T-1} catch_{t-i}$) taken prior to time t, T equals the total number of observations, and $N_0$ is the size of the population beginning of the period when $K_t = 0$. Now consider the conventional short-run model $Y_t = q\,N_t$, where $N_t = N_0 - K_t$. With the Leslie model, abundance, $Y_t$, may be measured in any way and completely independent of the fishing process that generates K (Hilborn and Walters 1992); Fritz measures resource abundance in terms of catch per unit effort (CPUE). The Leslie model does require the assumption that the value of q does not depend upon the level of effort. Thus, the Leslie model offers an extremely convenient framework, which does not require fishery-dependent estimates of resource abundance, for estimating initial stock abundance as well as determining whether or not resource depletion has occurred.

Fritz adopts the modified Leslie model to assess whether or not the resource has been locally depleted over time (Bratten 1969):

$$Y_t = q\,N_0 - q\,K_t$$

where $Y_t$ equals catch per unit of effort, q is catchability, $N_0$ is the initial stock size when $K_t = 0$, and $K_t$ equals lagged cumulative catch plus one-half of the catch at time t. Since the specification is linear, ordinary least squares is advocated by Fritz as the appropriate procedure to estimate the parameters and subsequently $N_0$.

## Potential Caveats of the Conventional Leslie and DeLury Models

Hilborn and Walters (1992), Seber (1982), and Ricker (1975) indicate that both the Leslie and DeLury estimates of q and $N_0$ are approximately unbiased provided all fish are equally vulnerable to fishing, K and E are measured exactly, and the value of q does not depend upon the level of effort. Hilborn and Walters (p. 395) also indicate that "there is much practical and Monte Carlo simulation experience to indicate that the estimates must be treated with considerable care." More important, Hilborn and Walters (p. 395) offer the following advisory note: "*Warning*: Be sure to estimate the reliability of depletion estimators by Monte Carlo methods for your specific problem." Fritz provides no Monte Carlo or sensitivity analysis of the estimates.

Hilborn and Walters and Ricker (1975) all point out that errors in measurement of the independent variable (K or E) cause the estimate of q to be biased downward and $N_0$ to be over estimated. Depletion may thus appear to be more significant that it is actually. They also suggest that extreme bias in the estimates of q and $N_0$ may occur if catchability is not constant; alternatively, q should decline progressively as depletion proceeds. In this latter case, the estimate of q may be biased upward and the estimate of $N_0$ may be biased downward. Subsequently, the depletion estimate of $N_0$ may be too low. Fritz concludes that catchability is not believed to be changing. There is, thus, support for suggesting that the estimates indicate a greater depletion than might actually have occurred.

## Data

Fritz uses catch and effort data obtained from the National Marine Fisheries Service Observer Program. Data collection procedures appear to conform to conventional sampling procedures. There appear to be some inconsistencies, however, in the manner in which catch and effort were aggregated over time. For some areas and years, catch and effort data are analyzed relative to a weekly period. In other cases, catch and effort data are specified relative to a wide variety of time periods (e.g., 1.75 days, 3.5 days, 0.5 days, etc.).

The time periods over which data were aggregated appears to be quite arbitrary. The only rational given for the aggregation was to ensure that there were at least ten hauls of data for each period and at least four periods of observations for each analysis. The notion of having ten observations appears to be related to Hilborn and Walter's recommendation that an experiment should be carried out for at least 10 time periods.

Ten observations according to Hilborn and Walters should be sufficient to permit examination of the depletion regression when catchability is not constant. Fritz does not, however, provide a rigorous analysis of the stability or possibility of changing regression estimates; a simple statistical test (F-test) over different time periods is all that is necessary to examine whether or not catchability is changing over time.

If one wanted to follow some statistical protocol, it might be argued that data should have been aggregated such that at least 30 hauls were included and 30 observations were available for analysis; a large sample, which is usually desired, is 30 or more observations. The number of observations only has to equal three; three observations allow the two parameters to be estimated and to have one degree of freedom upon which to base all statistical tests. Alternatively, it would make sense to use daily data in order to have a consistent set of observations.

Without reestimating the Leslie model for all areas and relative to all selected and possible time periods, the ramifications of Fritz's selected time periods for the results are unknown. In general, but not always, aggregation of data over time typically provides more stable results and distinct temporal patterns. Alternatively, the influence of extreme data values may be masked by temporal aggregation.

## Statistical Analysis

### Potential Simultaneous Equation Bias:

Although we suggest that the results suffer from implicit simultaneous equation bias, the actual problem is that an endogenous or dependent variable appears on both sides of the estimating equation. Bratten (1969) originally proposed adding one-half of the current catch, as done by Fritz, to compensate for the discontinuity in catch by treating the catch as if it was from the center rather than the end of the time interval. Alternatively, the Bratten approach was offered as one possible way to avoid losing an observation which might be serious when the number of observations are few.

Kelejian (1981, p. 242), however, shows that the covariance between the regressor, lagged cumulative catch plus 0.5 x $catch_t$ in the Fritz study, and the error term, $u_t$, does not equal zero. As a consequence, estimates of the slope (catchability coefficient, q, in Fritz ) and the intercept--product of catchability and initial standing stock, q $N_0$, in Fritz--are not unbiased, minimum variance or efficient, or even consistent. Results of statistical tests based on the normal distribution are, therefore, inappropriate (e.g., the significance of the regression, all t-tests, and all similar parametric tests requiring normal or asymptotic normal distribution). As shown by Kelejian, the estimate of the slope coefficient, q in Fritz, may be biased downward which leads to $N_0$ being overestimated. This is the same problem identified by Hilborn and Walters and Ricker (1975). It is, thus, possible that the empirically-determined pattern of depletion, as obtained by Fritz, may be more severe than it appears. Hilborn and Walters and Ricker (1975), however, suggest that nonconstant catchability may cause a more severe bias in the estimates--they suggest that depletion estimates of $N_0$ may be too low.

### Possible Heteroscedasticity:

Although heteroscedasticity typically does not pose a problem when time-series data are used, it is possible that the estimates presented in Fritz suffer from heteroscedasticity or the case of nonconstant residual variance. While heteroscedasticity does not pose a problem for bias, its presence prevents the estimates from being minimum variance or efficient. A consequence of heteroscedasticity is erroneous conclusions regarding hypotheses (e.g., significant regression when results are actually not significant and inappropriate confidence intervals).

The issue of heteroscedasticity relates to the formulation of the Leslie model and the use of fishery-dependent data, $catch_t/effort_t$. The formulation used by Fritz specifies abundance to be a function of the initial standing stock, cumulative catch plus one-half of current catch, and catchability. Following DeLury (1951), catch may be specified as a function of catchability, initial standing stock, cumulative catch, and effort:

$$catch_t = q\,N_0\,effort_t - q\,K_t\,effort_t$$

Adding an appropriate error term and subsequently dividing by effort imposes heteroscedasticity. Regardless of the traditional catch formulation, heteroscedasticity might also be possible because the right-hand side regressor, lagged cumulative catch plus one-half of current catch, is quite large relative to catch per unit effort--a common problem which often results in heteroscedasticity.

In empirical work, it is quite common to provide results of tests for heteroscedasticity. Fritz provides no analysis of heteroscedasticity. A review of graphs for the various resource areas suggests that heteroscedasticity may be a problem relative to the following area estimates: (1) Kiska-1995; (2) Delarofs-1992; (3) Akutan-1991; (4) Sequam-1992--but inversely related to right-hand-side regressor; and (5) Buldir W-1996. Without formal testing for heteroscedasticity, it is problematic as to its presence and severity.

Serial or Autocorrelation:

Autocorrelation is simply the case in which the value of the disturbance term in one period is related to the value of the disturbance term in one or more previous periods. If autocorrelation is present, parameter estimates obtained from ordinary least squares regression are not efficient, and inference based on the least squares estimates is adversely affected.

Fritz's paper provides no statistic results of an analysis for serial correlation. Instead, all results are available in a separate analysis. The separate analysis, however, also contains no results of tests for first or nth order autocorrelation. A review of the residual plots available in Fritz's report suggests that serial correlation may be present for the following estimates: (1) Kiska-1995; (2) Petrel Bank-1994; and (3) Sequam-1993. Examination of the residual plots in the separate analysis indicates that serial correlation may problems for the estimates of the following resource areas: (1) Kiska--1994 and 1995; (2) Kiska--1996 A and 1997; (3) Delarofs April 1994; (4) Delarofs 1997; (5) Akutan-1991; (6) Amichitka W-1996; (7) Amchitka E-1997; (8) Amchitka E-1995; (9) Sequam-1993 (all vessels); (10) Sequam-1994; (11) Sequam-1996 A; (12) Sequam-1996 B and 1997; (13) Buldir W-1997; and (14) Buldir E-1996 B. Unfortunately, we cannot determine whether or not autocorrelation poses a problem without an extensive analysis of all estimates.

At a minimum, all estimates should be examined for at least first-order autocorrelation. The author may use the Durbin-Watson statistic to test for first-order serial correlation; this statistic is typically available on ordinary-least-squares regression packages. Alternatively, other tests such as the Ljung-Box and Breusch (1978)-Godfrey (1978) tests may be used to examine serial correlation.

## Linear vs. Nonlinear Model:

While it is common practice to estimate the Leslie model via ordinary least squares and the assumption that all parameters are linear, it is possible that the specification may be nonlinear as well. An alternative specification and estimation is the nonlinear model in which q and $N_0$ are estimated separately, but q is constrained to be equal in the intercept term and the slope. With the limited number of observations in the study data set, it may not be possible, however, to obtain estimates via a nonlinear specification.

## Regression Diagnostics:

It is now common practice by statisticians and applied researchers to conduct regression diagnostics of estimates. Belsley et al. (1980) provide an extensive array of methods for identifying influential data points. The Fritz report and study provides no regression diagnostics; given the limited number of observations available to Fritz, it is highly likely that one or two data points may have significantly influenced the parameter estimates.

Two common measures for determining influential data points are studentized residuals and leverage values. These two measures are available in most statistical packages. The studentized residual is actually a standardized residual which permits determination of an influential data point; values in excess of 1.96 or 2.00 suggest observations deserving additional examination. The studentized residual permits the determination of the possibility of an influential y value. In contrast, leverage values, which also permit determination of influential data points, indicate influence relative to x variables.

A review of the plots contained in Fritz's report and separate analysis suggest that influential data points may be forcing several of the statistical results (e.g., last data point in Delarofs-1995 and 1997; first and last point in Amchitka W-1996; and first two data observations and last observation for Sequam-1992). Without conducting a comprehensive analysis of all resource areas and time periods, it cannot be stated, a priori, whether or not there are influential data points.

Using the 1992 data for Sequam, one outlier was determined to characterize the data. The outlier value corresponded to week number 7. Reestimating the same model of Fritz, the initial standing stock was estimated to equal 42,652 rather than 44,535 as obtained by Fritz. While the difference is only 1,883 metric tons, the level of significance of the regression is quite different. Moreover, the results without the outlier suggest that depletion was considerably lower than indicated by Fritz. Fritz's estimates are significant only at the 5% level of significance; they are not significant at the one percent level of significance. With the one observation deleted from the estimation, the regression was determined to be significant at the one percent level of significance.

Another potential problem is that the estimated parameters may not be stable over time. Fritz provides no analysis of parameter stability (e.g., F or Chow tests or cusum and cusum-squared values). Although these are not "fool-proof" tests, they are usually reliable indicators as to whether or not parameters have changed over time; such tests would be particularly useful for testing the assumption of constant catchability over time which is a required assumption of the analysis.

### Confidence Intervals for Initial Standing Stock:

Perhaps the most alarming result of the Fritz study is the extremely large confidence intervals for the estimated initial standing stock. The large confidence intervals raise the issue of the usefulness of the estimates. The 95% confidence intervals are extremely large for the following resource area and time estimates: (1) Petrel Bank-week 32-37, 1993; (2) Petrel Bank-week 37-41, 1993; (3) Sequam Bank-week-3-15, 1992; (4) Sequam Bank-week 3-10, 1993; (5) Sequam Bank-week 3-9, 1993. Values corresponding to the 95% confidence intervals are extremely large and suggest, in many cases, the cumulative catch was very high relative to the standing stock, or alternatively, the total cumulative catch was minuscule compared to the initial standing stock (e.g., Sequam in 1992 has a cumulative catch of approximately 29,000 metric tons, but the initial standing stock might have been as high as 373,272 metric tons; the initial standing stock also may have been lower than the total cumulative catch, which suggests recruitment and/or growth).

## Consistency of Results and Verification of Approach

Although the aggregation of observations over time presents a potentially serious problem for verifying the results and the applicability of the approach of Fritz, the fact that estimates are consistent for some areas and time periods and not consistent for other areas and time periods seriously calls into question the veracity of the results and the use of the approach. There is no basis for suggesting localized depletion in one area and time period, but not in the same area and another time period when the area was fished at similar levels of prosecution during both periods. Alternatively, such results may be possible, but Fritz fails to offer any explanation.

Examples of significance and nonsignificance for the same area over different time periods include the following: (1) Kiska 1994 (possibly significant) vs. Kiska 1995 (nonsignificant); (2) Kiska with some hauls (not significant) vs. Kiska with other hauls (significant); (3) Delarofs--not significant for most years and significant for 1996; (4) Amchitka W--1995 (not significant), 1996 (significant), 1997 (significant); (5) Amchitka E--1995 (not significant), 1996 (significant); and 1997 (significant); and (6) Sequam--1994 (not significant), 1995 (significant), 1996 (not significant), and 1997 (not significant). We also find similar patterns of significance and nonsignificance for the other resource areas over time.

## Closed and Open Population Model

Hilborn and Walters (1992), Chien and Condrey (1985), and Collie and Sissenwine (1983) all call into question the appropriateness of assuming a closed population. All suggest that immigration in the form of recruitment and emigration (e.g., natural mortality) likely occur for many fishery resources. The data plots contained in the report and the separate analysis, in fact, suggest that recruitment may be quite significant for some areas. The effect of recruitment is to lower the slope and lower the intercept; thus, $N_0$ may be overestimated and the decline in abundance may be underestimated. In addition, it is difficult to accept that natural mortality equals 0.0 for all areas.

Chien and Condrey (1985) offer one approach for estimating the initial stock when natural mortality is not negligible. They include variables to consider natural mortality or the case in which the ratio of natural mortality to fishing effort is relatively constant. Hilborn and Walters also offer several alternative approaches for estimating depletion in an open population. The various approaches should be further explored for examining the possible localized depletion of Atka mackerel.

## Additional Concerns

Besides the statistical and closed vs. open population concerns previously discussed, there are some additional aspects of the analysis which raise concerns.

There is the possible issue of standardizing q and abundance relative to area to determine if the data even make sense. If after standardizing abundance, catch per unit effort, for area, it appears that Atka mackerel are so dense they could be walked on, there would be considerable reason to doubt the usefulness of CPUE as a measure of abundance. Fritz provides no discussion about whether or not the fishery-dependent measure of abundance is appropriate.

Then there is the issue of variability in q over time and over different areas. Catchability, q, should be independent of effort but, of course, may vary by area. However, if q were standardized for area, the value of q should be relatively homogeneous over different areas. Fritz provides no analysis of the values of q.

## Conclusions

The analysis by Fritz appears to have numerous possible problems. First, there is the problem of data aggregation over time. Fritz provides only a tenuous reason for selecting periods of different lengths. Second, there is the possibility that estimates are biased, inconsistent, and inefficient; this is caused by the use of catch on both sides of the equation. Third, there is a possible problem of heteroscedasticity which causes the parameters to be inefficient and inconsistent, and may lead to erroneous conclusions based on conventional parametric tests. Fourth, there is the potential problem of serial correlation which also causes estimates to be inefficient and can lead to erroneous conclusions. Fifth, the analysis provides no regression diagnostics, particularly with respect to influential data points. Plots of the data for the various resource areas and time periods suggest that many estimates may be subject to influential data points (e.g., only one or two observations are giving the fit and mathematical relationship between abundance and cumulative catch). Sixth, the extremely large confidence intervals seriously undermine the usefulness of the models and the conclusions about depletion. Seventh, the results relative to resource areas and different time periods are inconsistent in that statistical results are significant at the five percent level of significance for some years or time periods and not other years or time periods. Last, the analysis by Fritz assumes a closed population, which simply may be incorrect; alternative models for dealing with an open population have been proposed Hilborn and Walters and should be further developed to more precisely examine whether or not there has been localized depletion. Last, we concur with Hilborn and Walters that analysis of depletion should include a rigorous Monte Carlo analysis.

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