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ISSUES IN POPULATION DYNAMICS OF TUNAS

--DRAFT WORKING PAPER PREPARED FOR--

ICCAT - WORKSHOP ON THE POPULATION DYNAMICS OF TUNAS

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The motivation for this paper lies in the need to take stock of where we are in fishery biology. Formulation of our present position regarding the major issues in fishery biology will accelerate the utility of fishery biology for the resolution of contemporaneous jurisdictional and management problems. Hilbert provided a tremendous amount of stimulation for the entire field of mathematics by "simply" posing the right set of questions. We are not presuming that this work is in anyway comparable to that of Hilbert, but we do hope that some of the questions set forth here will provide greater insight into an important branch of fishery biology; population dynamics and in particular the population dynamics of tunas.

In considering these issues we should be (1) thinking of new dialogue, (2) searching for omissions in the formulation of issues or conversely to determining if we are giving undue importance to certain issues, (3) developing a focus upon certain areas and in achieving this focus enhancing the generality of existing results, and (4) to providing guidance to the developing countries regarding tuna dynamics.

The perspectives treated are I, statistics; II, measurement of fishing effort; III, age specific F; IV, use of length to estimate age in tunas; V, effort models; VI, population models; VII, non-parametric approaches; and VIII, fishery management.

I. STATISTICS

It is clearly recognized among scientists and managers that management decisions require good fishery statistics. This is, of course, a general fisheries problem, and thus the discussion is more in the context of this general problem. In considering the problem, the measure and criteria of "goodness" depends upon the fishery problem at hand. Nevertheless, the inadequacy of fishery statistics (basically measures of catch, measures of effort, and measures of size composition) is commonly known and results from two principle causes. The first is that there is a lack of accountability and responsibility among the community of individuals involved or otherwise associated with the fishery to provide good statistics. The second is that the technology used to transmit or to transfer the statistics from the fishing boats to the ultimate management sources is apparently limited. The latter situation results from what must be an implicit disinterest on the part of decision makers in requiring good statistics as well as the inability of managers to interface with modern data processing technology or for managers to receive adequate funding for statistics collection. Could it be that the inadequacies of statistical systems result from their piecemeal design whereby the existing subsystems of each entity, each with different funding, different views of urgency, and different concepts of adequacy, are forced together in a "best we can do" mode. Do we not need to design the best system we can think of and then search for alternative ways to approach our ideal system? The collection and processing of good statistics is, of course, costly. On the other hand, we are not necessarily requiring more money, but rather developing the question of how funds are allocated.

The question of accountability and responsibility of the community need not be dwelt upon--beyond observing the status quo regarding fishery statistics--except to note that the acquisition of this responsibility and accountability will not just happen of itself. It has to be made to happen in a very explicit way and might indeed be considered as part of the Law of the Sea (LOS) deliberations.

With respect to the development of an ideal statistical system it is important to examine criteria or measures of performance of an ideal statistical system. These measures of performance involve:

A. Are the right data being collected?

B. Are the data unbiased and sufficiently precise?

C. Are the data timely?

D. Are the data accessible?

I.A. The "right data" have been discussed at some length at ICCAT and many other sessions. These involve data on catch, data on effort, and data on size distribution.

I.A.1. Data on catch are, in principal, the most easily obtainable. While there are particular problems with estimating the total catch from particular countries these problems are recognized and need to be handled specifically. Problems do remain in species identification, multiple counting, country, and location of catch.

I.A.2. Data on fishing effort have been more difficult to collect. There are basically two difficulties with effort data, the first being the difficulty in obtaining the data and the second regards the fact that while longline data are relatively simple, data from surface fisheries are exceedingly complex and could be subtly misleading. It may be that the wrong kinds of effort data are being taken for surface fisheries.

I.A.3. Size distribution data.--There are serious questions regarding the representativeness or the existing availability of size distribution data for tuna fisheries. This problem needs to be faced explicitly.

I.B. Bias and precision of the data.--We know _a priori_, owing to the real difficulties of data collection that the collection procedures are non-random and thus likely provide biased and imprecise estimates. The question that we need to face involves (1) the magnitude of the bias, (2) the causes of the imprecision, and (3) the sensitivity to whatever we are trying to estimate to the bias and imprecision.

I.C. Timeliness.--Some data are available virtually instantaneously, other data takes a few years to become available. The question is whether we have the right data in sufficient time to make decisions--not only the decisions that we perceive at present on an annual time scale, but--particularly in the future--on a much finer time scale. In present problems, thinking of decisions on an annual time scale is probably obsolescent and analogous to "always building the airport too small."

I.D. Accessibility.--Data are frequently inaccessible owing to lags in reporting, technological constraints in getting the data out, or administrative constraints. Modern fishery management should make all data accessible (accessibility here means two things (a) "saying" the data is available, (b) but more importantly keeping the data in a format so that it is "immediately" available) - preserving of course requisite business privacy - to the scientific community. Scientific review reduces the possibility of introspective and self-justifying analysis, but such review is difficult or hampered if data are inaccessible.

The remedies to the data problem involve four areas:

1) attitudes
2) hardware
3) software
4) institutional.

The first action is that decision-makers in all countries need to develop an attitude which affirms the need to collect good fishing data and commit their resources to this endeavor. It is not good enough for scientists to recognize this need or to approach this problem on a piecemeal basis with some countries doing a good job and other countries ignoring the problem. Furthermore, this is not just a tuna problem, but a general fisheries problem. On the other hand, scientists need to specify the scope and magnitude of the problem to decision-makers and thus resources need to be committed to the specification endeavor.

The second action that needs to be taken is the identification of hardware systems. The level of data collection around the world is weakened by the lack of application of modern computer systems to the problem. The magnitude of the job won't allow (I don't think) a "friendliness requirement" for filling out logbooks or hand processing of data. We need to explore real-time communication systems involving radio, FAX, and satellites between the boats and shore stations, we need to have computers suitably located that can process the incoming information to perhaps distant terminals on CRT or in extremely fast printout modes, and finally we need ways of storing and accessing the data. All in virtually real-time.

The third action that needs to be taken is the development of software - the logic that guides the flow of information through the system. In short, we need the development of information systems that can provide data for analysis and simulation modes as well as for real-time fishery management and dissemination of program information. Anyone who has worked with masses of fishery data will acknowledge that the development of software systems is a non-trivial problem.

The fourth action is the development of an institution to chart the progress of such a development. Such institutions already exist in other areas of science (e.g. World Data Center A) and in fisheries as well. It is clear, however, that if we are going to make a quantum jump we need to get people with skill and experience in modern information technology.

A fifth action is to provide a framework for developing an action program for a global fisheries statistics system. The development of such a framework would specify whether such a system would indeed be better than the existing situations in terms of the costs and benefits of the new system in the context of any contemplated changes in jurisdiction as well as treat explicitly the questions of attitude, hardware, software, and institutions. It is clear that such a framework needs to be aired and it might be that a conference similar to the FAO Technical Conference on Fishery Management and Development (Vancouver, Canada, February 13-23, 1973) would be a convenient mechanism.

II. MEASUREMENT OF FISHING EFFORT

This is still one of the significant areas in fishery biology, being of economic as well as biological import. Definitional detail is in the literature and we simply note that the problem relates to recognizing that for effort measures to be meaningful in dynamics it needs to be, in some way, proportional to fishing mortality. Four concepts are involved:

A. Traditional effort measurement.

B. Estimating F.

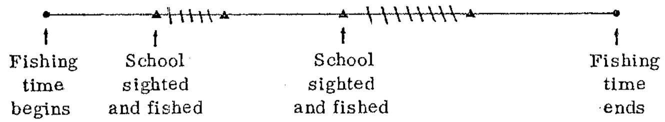
C. Effort models.

D. Don't bother estimating effort (i.e. use catch only).

II.A. Traditional effort measurement.--One of the great difficulties in measuring effort involves fisheries that have what might be called "complex effort structure," in other words the usual measurements of nominal effort are not clearly related to fishing mortality. That is because the usual nominal measures are based upon a mix of activities. A good example of this is a surface tuna fishery (or other surface fishery, for example, see Neymann,) where the usual nominal unit of effort is composed of a mixture of time spent fishing and time spent searching.

What we are after is schools sighted per hour searched, but since boats spend some time "catching," search time needs to be subtracted from the days fishing.

The idea can be seen a little more clearly by applying elementary results from queing theory. Let us adopt the following model.



Thus the day is partitioned into search time and fishing time. Let us assume that the schools behave as in the simplest queing model whereby a single seiner (i.e. the fishing boat) processes schools arriving according to a poisson process (this is an interesting concept because if the schools are arriving according to a poisson process, then the individual fish are not). Further assume that the fishing time is distributed according to an exponential parameter by μ. Suppose the mean arrival rate is

$\lambda = 1$ school per time period

and the mean processing rate is

$\mu = 2$ schools per time period.

Then it is well known that

$$P_n\ [\text{n units are in the line}] = (1 - \frac{1}{2})\frac{1}{2}^n$$

Thus

n	P_n
0	0.5000
1	0.2500
2	0.1875
3	0.0937

Thus, the probability that the system is free for sighting is only $P_o = 0.5$ or the expected time during a day (given the assumption and $\lambda = 1$, $\mu = 2$) implies that only 1/2 the days fishing is "free" for sighting. Should the catch per days sighting then be doubled? If we reduced our school density to 1/2 school per time interval $P_o = 0.75$ and thus, 3/4 of a day would be used per sighting. In other words, if hour searching is a measure of effort, this measure of effort needs to be adjusted by the density of the schools (obviously the CPUE needs to be related to the school size - and this would modify the problem both from a density point of view - arrivals of single fish are obviously not poisson and schools are not of uniform size and further size of the school clearly effects μ).

Further aspects of the system can be seen in that with $\lambda = 1$, $\mu = 2$ the average length of the queue is 1/2 school again implying that fish are waiting to be "searched" because some fish are being processed; in fact, the average wait for processing is 1/2 time unit.

These results can be considerably generalized, but unfortunately with very considerable difficulty if arrivals are not poisson.

Thus it is clear that if one wants to use these effort measures, operations research analysis needs to be conducted upon the surface fishing operation and questions of the distributions of the various activities, among other things, need to be addressed.

Unfortunately data on search time and fishing time are not ordinarily available. We have been able to acquire some data on this problem from our porpoise investigations. These data are "first-cut" and have not been quality checked and pertain only to porpoise sets, but they do give us an idea of the problem and the variability involved. In Figure 1 we have set forth the frequency distribution of the percent of each day spent searching. The data were stratified as to east of 95° W and west of 95° W and by January and February. The data show

1) Considerable variability in the percentage of each day searched.
2) A central tendency of about 70 to 90% of each day's searching.
3) An important number of days are involved in 100% searching.
4) Most importantly, a consistent variability between offshore and inshore areas, indicating that perhaps 10% more time is spent searching in offshore areas. This implies that the percent searching statistics might vary among years, and raises the question of whether the data require adjustment. If the schools in the

inshore and offshore areas are of equal size and the contact rate inshore and offshore are the same, then the fish would be more abundant offshore than inshore (even though the catch per day would not vary).

We have also obtained frequency distributions of search time and fishing time from the same data set. The mean and standard deviation for each stratum are set forth.

Search time in minutes

	East	West
January	$\bar{X}$ = 430 (293) S = 261 (187)	$\bar{X}$ = 357 (279) S = 261 (172)
February	$\bar{X}$ = 460 (296) S = 312 (187)	$\bar{X}$ = 308 (247) S = 245 (161)

Fishing time in minutes

	East	West
January	$\bar{X}$ = 155 S = 48	$\bar{X}$ = 175 S = 53.3
February	$\bar{X}$ = 159 S = 60	$\bar{X}$ = 160 S = 59

Note: The numbers in parentheses refer to means and standard deviations that were computed without including days having 100% search time.

The search times again show the same consistency as the percent of each day spent fishing. There does appear to be some interaction, however, regarding the fishing time.

The above data further gives us quick insight into the nature of the arrival process. If the schools arrive according to the poisson process, then the distribution of time between schools should be exponential. In an exponential distribution the variance is equal to the square of the mean. We can see that in all of the distributions the square of the mean is consistently greater than the variance suggesting that the poisson assumption for the particular strata chosen is not likely to apply. The "particular strata" is stressed because a mixture of poisson distributions is not necessarily poisson. The same statement may be made regarding fishing times and so it will be difficult to use exponential holding times in the development of an analytic model.

We conclude then that a more detailed analysis of surface fishing operations will enable more refined analysis but it remains to be determined whether the more refined analysis will give substantially different results. This needs to be examined.

III. AGE SPECIFIC F

It is clear that tuna fisheries do not exert the classical ogive type of selectivity where F is constant above a particular size. Therefore, when computing yield-per-recruit and production functions, it is important to know how F varies as a function of size. We really need to think in terms of $\vec{F}$ rather than F.

Not only is it desirable to compute $\vec{F}$ for estimating yield-per-recruit and production functions, but the ability to estimate $\vec{F}$ from the age distribution of the catch enables us to pass around one of the very difficult and troublesome issues in fishery biology and that is the estimation of $\vec{F}$ from catch per unit of effort when the catchability varies in an unrecognized way each year.

Thus having F and f allows us to estimate $\vec{q}$, an analysis which might reveal among other things, that $\vec{q}$ is not at all a function of the physical design of the gear or of the wile of the skipper but a function of the distribution of fishing relative to the fish or of the size of the stock. At any rate, the methodology depends upon developing a set of ratios between the catch in each age group in the i-th year and the (i + 1)-th year. The number of unknowns are reduced by making an assumption on M and some initial value of F.

The difficulties in estimation are intrinsically difficult because the error in one equation is a component of estimating the next equation and so on.

Simulation studies need to be conducted to study the effect of:

1) Constant M when M may be changing.
2) Further studies on errors in estimating the initial value of F.
3) Cascading error propagation.
4) The effect of non-representative sampling of the catch or systematic errors in aging fish.

An explorate of some of these sensitivities may be found in Lenarz et al (1973).

These simulation studies will be aided by knowing the large sample means, the sampling variances, and sampling covariance of the approximately normally distributed F and M. The formulae for these statistics are available.

IV. USE OF LENGTH TO ESTIMATE AGE IN TUNAS

Using the so-called iterative solution to the catch equation is particularly difficult for tunas because of difficulty in determining the age of fish from "hard parts."

This necessitates the use of alternate aging techniques. There are basically two of these. The first involves plotting a length frequency distribution and separating modal groups by eye, algorithm, or probability paper, and the second involves estimating growth from tagging data and then applying these estimates to the size distribution to estimate age composition.

The peculiar life history of the tuna, however, makes these approaches somewhat tenuous. These peculiarities involve:

1) The tunas are difficult to age from hard parts.
2) Because tunas spawn over much of the year, there may be no such thing as conventional year classes.
3) Size-specific or age-specific migrations through area may confound the estimation problem.

The fact that tunas are difficult to age and they spawn over much of the year leads to possible interpretive difficulties regarding growth. This can be seen from the comparison of two lexis diagrams (Figure 2), one which might pertain to tunas and the other which might pertain to "conventional fish."

The virtually continuous spawning of tuna make it very difficult to think of discrete age groups. Thus, when we sample at arbitrary time 2.5 we have for conventional fish two clear age groups represented, and knowing the size of each we can usually deduce growth as a difference in size among age groups. In contrast the specific age groups in the tunas are very difficult to distinguish. Yet on the other hand modal groups do appear, the size of which is no doubt related to

1) growth
2) differential or size-specific migration pattern.

All of this points to the basic need of having a better understanding of the life history of tunas with regard to periodicity in spawning.

V. EFFORT MODELS

It is well known that one of the central techniques in fishery management involves the control of effort to somehow optimize catch. It is also well known that we do not have a very good fix on fishing effort, particularly for fisheries with "complex effort structures." In fact, one of the methods that is commonly used to control effort is to control catch, because

$$Y = F\bar{N} \Rightarrow F = \frac{Y}{\bar{N}}$$

Thus, given some average population, $\bar{N}$, and fixing the catch at Y* for example will yield an effort of F*. This, of course, assumes that we correctly divine the average population. This is not a trivial matter particularly when there are effort measuring difficulties and when the time distribution of the population during the year is a complex function of time. Again, all of this points to the need to have a better measure of fishing effort.

Furthermore, there is some question as to whether we can have a timely enough appreciation of fishing effort simply from monitoring the catch. Certainly, with increased amount of nominal fishing effort it is becoming more and more difficult to control real effort--the control of effort has to almost be accomplished before the vessels leave the dock.

It seems that there are, however, some alternatives. These stem from the discussion by Gulland (1969: 66-67) of the swept area method for deducing fishing effort. The discussion reflects that, if the stock is evenly distributed over area A and the gear covers area A then the mortality generated by the fishery is $F\Delta t\, A^{-1} \Sigma a_i$ where the sum is taken over the a_i areas fished. It is further pointed out that because not all fish are caught in the swept area and because fishing tends to concentrate in locations with greater stock density leading to underestimates and overestimates of fishing mortality respectively that such a technique has certain biases.

The beauty of the technique, however, is that a measure of fishing mortality, however crude, is available in terms of $A^{-1} \Sigma a_i$ before the vessel even leaves the dock. Thus it is possible, in principal, to predict the fishing effort exerted before a vessel departs for the fishing grounds. Looking at this problem more carefully we can see somewhat greater complexity. In particular the fishing effort exerted by a vessel is not only a function of vessel "hardware" (e.g. its size, horsepower, etc.), but it is also a function of distributional characteristics of the stock and/or the density of the stock and the skill involved in the decision processes involved in where and when to fish. Thus, one of the goals in fishing dynamics should be to develop equations of the form

F = ∅ (<u>hardware</u>: e.g., vessel size, hull configuration, and horsepower; <u>stocks</u>: location fished and density; <u>skill</u>: entropy of decision environment and "information" processing of skipper).

With such information we can enter into more advanced and finely tuned management techniques. Of course it can be argued that if a vessel has a 1,000 ton capacity it will generate $(1{,}000)\bar{N}^{-1}$ units of fishing effort during the year, but this gets us back to defining $\bar{N}$ in this context and again opens all the difficulties that we have in the first place with measuring effort. Further, it can be argued that the functional relation described above simply just advances the fishing power argument in which we would obtain data on hull configuration etc. and then use this data in an analysis of variance "fitting constants." This, of course, is a first cut in appreciating the importance of various kinds of information that might be useful in deducing the kinds of variables that would enter into the above equation but it still is not sufficient to develop a functional predictive equation in either the deterministic or in the Monte Carlo mode.

Thus, the development of analytic or simulation effort models would serve two functions. First, it would help us better understand the intricacies of measuring effort particularly in fisheries with relatively complex effort structures and secondly, such models will open the door for more finely tuned fishing effort management.

VI. POPULATION MODELS

Basically the difficulty with production models involves the fact that they simply assume $c/f = a - bf \Rightarrow c = af - bf^2$ (further assumptions may be made by estimating the exponent). This does not denigrate production models. but rather serves to emphasize that certain features of the production model need ample discussion. These have been pointed out by various authors. For example Pella and Tomlinson (1969) discuss

1) random variation in production and catching rates
2) equilibrium size and age structure
3) time lags
4) closed populations
5) constant catchability.

In addition, discussion is required on the sensibility of the model including

1) It is implicit that there are tradeoffs in the model. If recruitment increases does growth really decrease or does mortality change? Where are the compensations? Do they make sense?
2) Are the number of parameters large relative to the kind of data that are being fit?
3) The correlation between c/f and f is:

$$\frac{\frac{1}{m}\sum c - \frac{1}{m}\sum \frac{c}{f} \cdot \frac{1}{m}\sum f}{\sqrt{\widehat{\text{Var}}\left(\frac{c}{f}\right) \cdot \widehat{\text{Var}}\ (f)}}$$

How do we remove this correlation effect to obtain a better idea of the underlying relation between catch and effort. For example, if catch and effort are uncorrelated we can have a nice relation between catch per unit of effort and effort. In the eastern Pacific, for example, there is no correlation between catch and effort for skipjack, but shouldn't there be a correlation between CPUE and f?

4) The parameters are likely to be functions of time rather than constant.
5) The form of the production model must be consonant with changes in F and yield per recruit calculations.
6) How do we develop an appreciation for the fact that in the real world there is a different production model each year and how do we gain appreciation for the shifts among production models?

VI.A. Age-structure models.--It is well known that the production models give an on-the-average view of population structure and do not admit, per se changes in age structure. Yet, in the real world, changes in age structure do occur and because they occur they can have significant effects on the understanding of the yield structures. Age structure models have been known in the literature for some time as Leslie matrix theory.

For discussion of this theory we need the following definitions:

$$M = \begin{bmatrix} f_1 & f_2 & . & . & . & f_m \\ s_1 & 0 & . & . & . & 0 \\ 0 & s_2 & . & . & . & 0 \\ & . & & & & \\ & . & & & & \\ & . & & & & \\ 0 & . & . & . & s_{m-1} & 0 \end{bmatrix}$$

When M is a matrix of fecundity (f_i's) and survival (s_i's). Next we have a vector of age distribution

$$\{K\} = \begin{Bmatrix} k_1 \\ k_2 \\ \cdot \\ \cdot \\ \cdot \\ k_n \end{Bmatrix}$$

Where the k_i's are the number of individuals in the i-th age group. Now we are interested in determining whether there is a $\{K\}$ which remains stable, that is

$$k_{ij}/k_{i+1,j} = k_{i,j+1}/k_{i+1,j+1}$$

for the i-th age group and the j-th time interval. For any particular M, then the stable distribution is that which satisfies

$$M\,\{K\} = \lambda\{K\}$$

where λ is a constant. If the equation holds then obviously the ratio of $k_{ij}/k_{i+1,j}$ will not change since moving through the M $\{\ \}$ cycle just multiplies every element in k, the k_{ij}'s by the same constant λ. Thus the dynamicist, given M, is interested in identifying the stable distribution which we denote $\{K_1\}$ and determining λ which identifies if the population is increasing or decreasing (there are also eigenvectors $\{K_2\}$, $\{K_3\}$, etc. corresponding to the several latent roots of M). If $\lambda = 1$ it should be clear that at each $M\{K_1\}$ cycle the population does not change in size and the ratios $k_{ij}/k_{i+1,j}$ also do not change; but if $\lambda < 1$ the ratios of course still do not change, but the population declines; if $\lambda > 1$ the population increases but of course again stability is maintained. Thus, when $\lambda = 1$ we have the condition that is equivalent to the production model $dp/dt = 0$ and the age structure is indeed stable, but we note that the stability is simply a function of the constant M and if $\lambda = 1$ the "balance" between the f_i's and the s_i's depends only on the relative magnitude of the f_i's and s_i's and nothing else--changes which are, unlike most forms of the production model, independent of density.

Naturally we want to find λ and $\{K_1\}$ and this is obtained by noting that $M\{K\} = \lambda\{K\}$ implies $K[M - \lambda I] = 0$, yielding the determinantal equation $|M - \lambda I| = 0$ and thus $M\{K_j\} = \lambda_j\{K_j\}$.

In addition to the stable age distribution, we can also derive from M, a stable horizontal vector $\{H_1\}$ which has the property $\{H_1\}M = \lambda_1\{H_1\}$. Now the $\{H_1\}$ gives us a measure of the reproductive potential in the stable population. Suppose for a particular age group j, $h_j = 0.5$ and there were 1,000 individuals of that age in the population, then the aggregate reproductive value gives 500 individuals from that age group in a stable population. The normalized reproductive value is $[H_1]/[H_1][K_1]$.

All of this then gives a rough idea of some of the definitions and properties of the Leslie matrix approach. We can see

1) That the usual density-related functions of production models do not obtain.

2) That fecundity is almost impossible to estimate for fish populations--on the other hand, the effects of certain fecundities (i.e. recruitment) can be determined.

3) Intuitively the stable age structure is unlikely to obtain in fish population as evidenced by year-class bursts.

Nevertheless we can see that because short-term fluctuations (1, 2, or 3 years; say) can be treated with this theory, from the theory we can obtain insights to these fluctuations, again even with constant M. There are several points of departure. These are generated from an interest in the dynamics of how a population reaches stability or from a different point of view, given a stable population, how perturbations in the stability affect the age distribution or the rate of increase λ. It is of considerable significance, I think, that using this methodology we can identify "non-random" variations, whereas conventional consider these variations as random "components."

In this connection, it is possible to partition any observed age distribution into its components,

$$\{K^{(0)}\} = \frac{\{H_1\}\{K^{(0)}\}}{\{H_1\}\{K_1\}}\{K_1\} + \frac{\{H_2\}\{K_0\}}{\{H_2\}\{K_2\}}\{K_2\} +.$$

Also, at any time

$$\{K^{(t)}\} = M^t\{K^{(0)}\} = {}_1^t\, c_1\{K_1\} + {}_2^t\, c_2\{K_2\} + \ldots + {}_n^t\, c_n\{K_n\}$$

which shows how stability is approached since $\lambda_2 \ldots \lambda_n$ are less than λ_1, they eventually disappear leaving the stable population.

As an example consider the population projection of Canadian women from Keyfitz. Figure 3 shows the discrepancy between the observed age structure and the predicted stable age structure, and the contribution to instability from the second through third terms of the spectral decomposition.

Thus, if Figure 1 was a fish population (this would not be unrealistic) changing the years to months, particularly for the tuna model, we can see that departures from the stable distribution in terms of oscillation which are out of phase. The components that develop the oscillations may be examined then in terms of analytic functions.

It is possible in fishing applications to incorporate the matrix approach by examining equations of the form (see Jensen, 1974; Beland, 1974)

$$Y(t) = \{F\}^{1 \times n} W^{n \times n} M^{n \times n} \{K\}^{n \times 1}$$

where Y(t) is yield, $\{F\}$ is the vector of fishing mortalities; M is the survival fecundity matrix and $\{K\}$ is the age distribution vector. The yields given by such an approach admit no dependency of M on population size.

It is possible, however, to remove the density independent criticism by letting $M = \phi(D)$, yielding $\lambda(d)$ and $\left\{K_1\right\}(D)$ where D is density (see Fowler and Smith, 1973). They developed density-dependent relationships between mortality, survival, and age at first maturity. The problem in fisheries though is to obtain the estimate of the density-dependent parameters. With such a model we can search for that density which will keep the population stable in number (see Figure 4).

Unfortunately, even though density-dependent mechanisms exist and could possible be treated with existing data, explicit examination of these has never been made. The analogue between the present approach and the "Schaefer model" is that the Schaefer model recognizes neither the causes for natural increment or natural decrement to the population.

These causes are, however, treated explicitly in the density-dependent matrix model. If we are to tune management we will be needing to examine these features more explicitly.

We should also consider the relation between continuous and discrete population models. The matrix model is, of course, discrete and we simply point out that theory is available for the continuous version.

The principle value at the present time of this approach would appear to be in its ability to express in analytic terms, expectant short-term, age-dependent departures from equilibrium;

VII. NON-PARAMETRIC APPROACHES

In almost all fishery applications, there has been a heavy reliance on a parametric presentation of phenomena. For example, in stock and recruitment we started out with a dome-shaped curve and ended with an asymptotic curve and now with the aid of the computer we can fit any shape curve. In production models, we started out with a dome-shaped curve and ended with an asymptotic-like curve and now with the aid of the computer we can fit any shape curve. It is clear that we have little understanding of underlying mechanisms on how these mechanisms change with time. We have placed ourselves in the mode of being complacent in fitting many parametric functions which turn out to have, for obvious reasons, little predictive value.

A good example is the right-hand side of production models for which there is frequently little or no data and for which we must place greatest predictive weight. Yet we are using little more than a polynomial to fit the function.

It seems that the alternatives are twofold. The first is to plan research that is consonant with getting a better fix on the various dynamic variables. This might involve

1) Identify and obtain an understanding of density-dependent mechanisms.
2) Laboratory work which would probably, in most instances, be much more cost effective than 1.

3) Review and consolidation of non-fish dynamics to search for new areas and applications.

It might also involve treating some of the data as being non-parametric.

We will ask, do such approaches lead to different inferences than with the parametric approach? Even if they do not, then the non-parametric approach requires less precise measurement and no sophisticated calculations are required.

As an example of the approach we have simply stratified eastern tropical Pacific data presented in Pella and Tomlinson according to decade, catch, and boat days. The results of this stratification are shown in Figure 5. No assumptions on equilibria or time lag etc. are made. It can be assumed that conditions in the future are representative of those sampled in the past although this assumption is not necessary. Further, there is no need to measure catch precisely or even effort (it would be nice, however, to be in the right year). No computers or calculators are required for computation. We would conclude that at the end of the first decade we would have been optimistic; at the end of the second decade we would have noticed a decline in catch over previous years, but in the last decade we would continue to be optimistic. If we eliminated the years when effort was less than 10,000 days we might arrive at yet different conclusions. There are also probability tests available for these non-parametric models.

VIII. FISHERY MANAGEMENT

Population dynamics is, of course, one of the basic building blocks of fishery management. The building block needs to evolve certain properties. Of these we have gone about as far as we can go with the traditional production model, yield-per-recruit and stock and recruitment techniques. These are all useful and will be useful for some time. On the other hand, little or no information will accrue and the level of management will remain about the same until the problems are approached differently.

1) We need to realize explicitly the artificiality of many of the assumptions in extant models. Realizing this, we must delve into the cause and effect mechanisms of multiple species, density-dependent and stock and recruitment phenomenon.
2) We need to develop allocation theory and relate this to population dynamics.
3) We need to investigate whether management schemes are at all feasible and if they are not, we need to determine ways of removing constraints.

The latter is particularly important in tuna fisheries where catch-quota-only management is drawing tremendous amounts of effort into the fishery; where commissions are not set up to come to grips with allocation problems; and where problems of even regulating catch or minimum size are technically formidable tasks even given the specification of the catch quota or of the minimum size.

Thus, there are basically several points of view, those of the scientists who want to set up mechanisms for management but who are apparently not sufficiently conversant with the socio-economic driving forces of the commissions and further are not really communicative with the decision-makers. There are the decision-makers who are influenced by the industries or constituencies and their own judgment and consist of a very wide mix of individuals whose understanding of fishery management ranges from excellent to inadequate. And finally, there are the industry and constituent groups whose private interests understandably outweigh the interests of the social good. Such a mix of interests, abilities, and motivations generates a decision atmosphere that is typified by "standoffs." In a standoff nothing happens. One of the major difficulties then is to explicitly attack the standoff problem. While this is not exactly a problem in population dynamics, it appears that individuals who are active in the field of population dynamics will have to contribute toward its resolution.

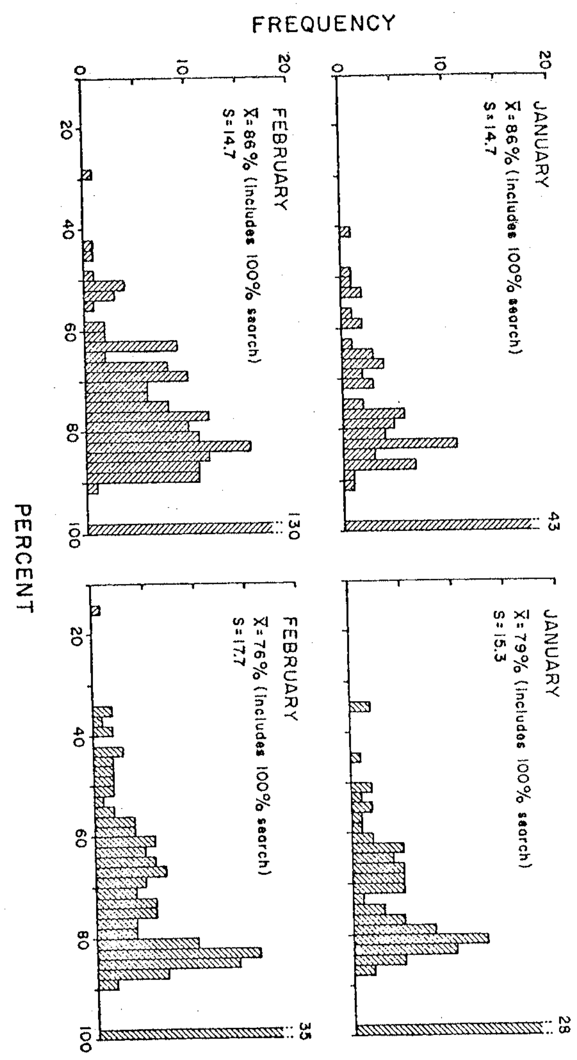


Figure 1. Frequency distribution of the percent of each day spent searching. These data were stratified as to east of 95° W and west of 95° W and by January and February. The data show 1) considerable variability in the percentage of each day searched, 2) a central tendency of about 70 to 90% of each day's searching, 3) an important number of days are involved in 100% searching, and 4) a consistent variability between offshore and inshore areas, indicating perhaps 10% more time is spent searching in offshore areas.

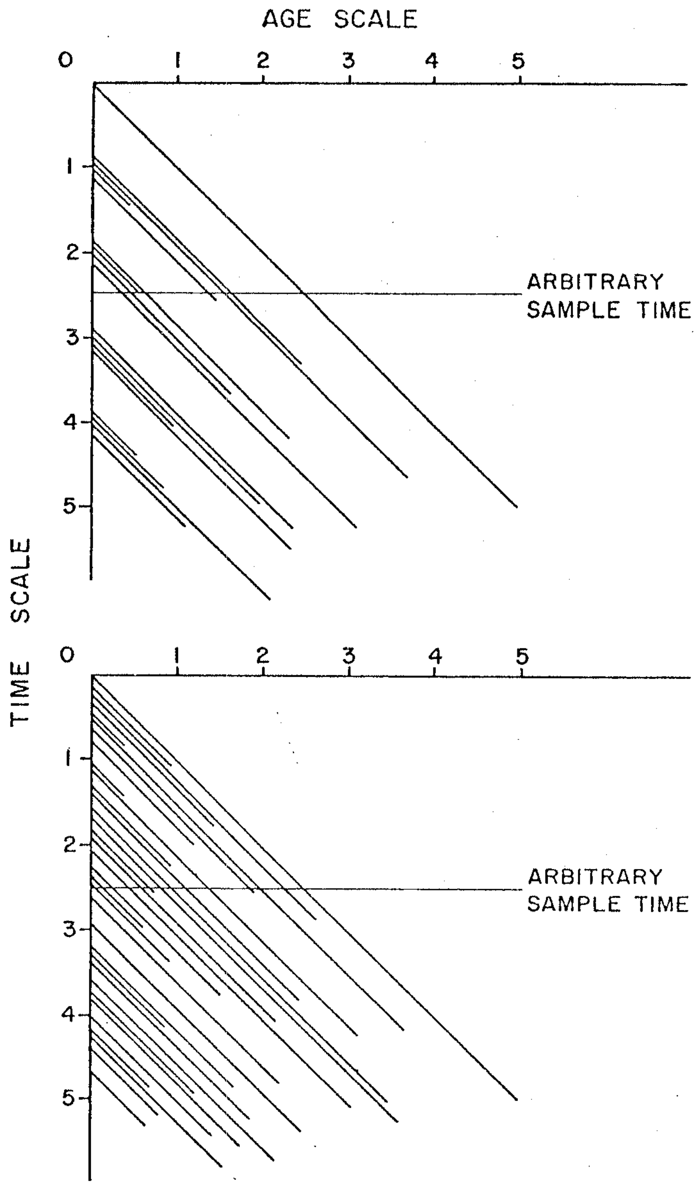


Figure 2. Top graph: Lexis diagram, "conventional fish"; bottom graph: diagram, "tuna".

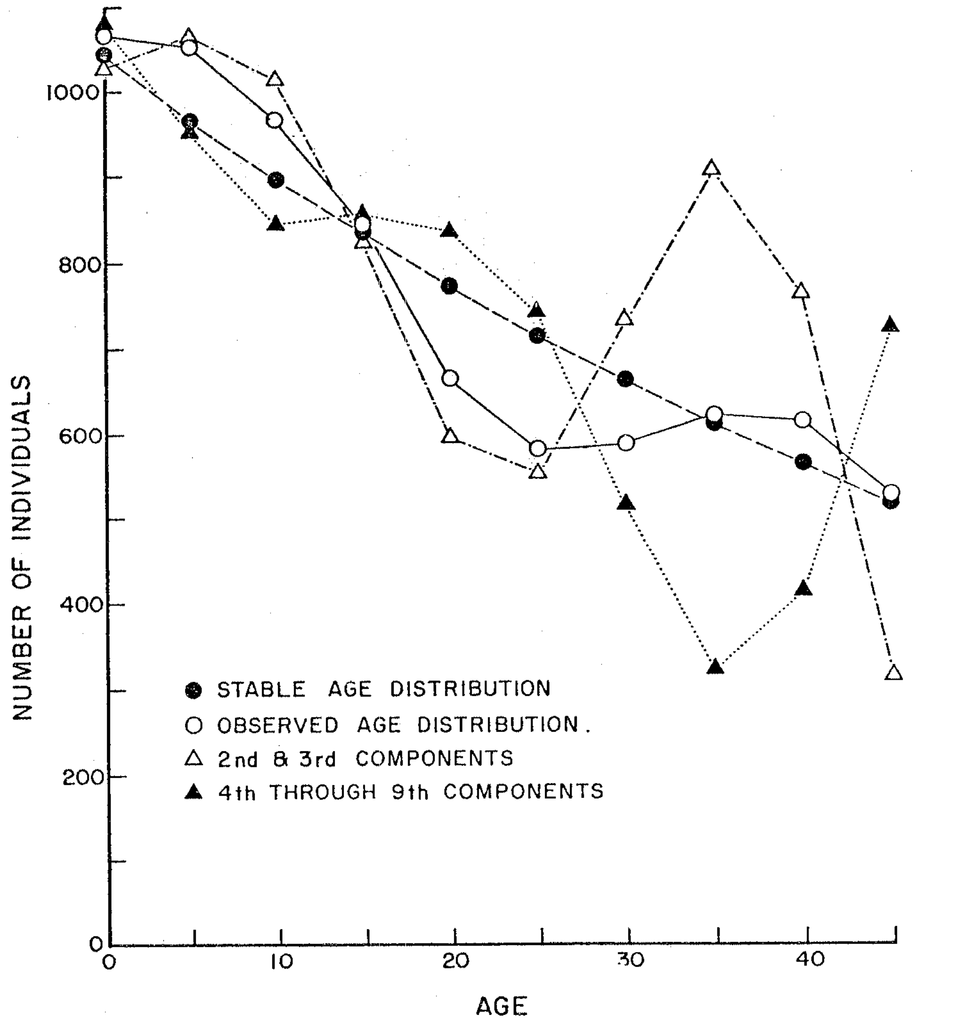


Figure 3. Population projection of Canadian women, 1965. Shows the discrepancy between the observed age structure and the predicted age structure, and the contribution to instability from the second through third terms of the spectral decomposition. (From Keyfitz, 1968.)

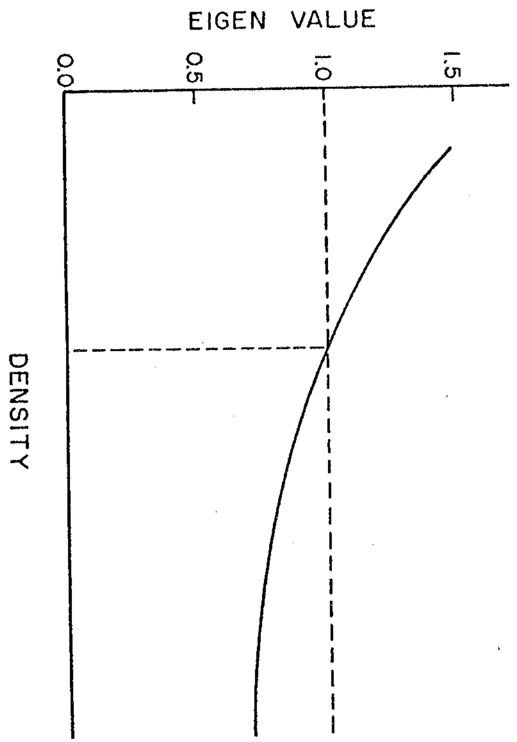


Figure 4. Magnitude of eigenvalue as a function of density showing the density at which the population is stable. (After Fowler and Smith, 1974.)

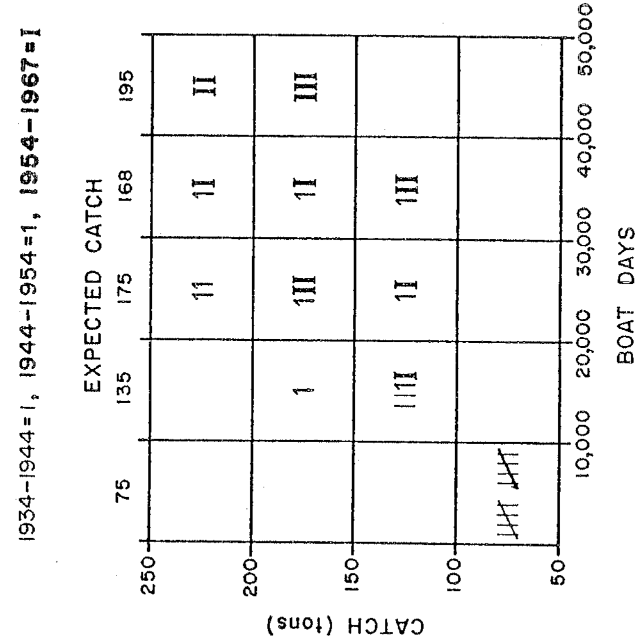


Figure 5. Results of stratifying eastern tropical Pacific data (as presented in Pella and Tomlinson, 1969) according to decade, catch, and boat days.