A BRIEF REVIEW OF SURVEY METHODOLOGY
WITH REGARD TO GROUNDFISH STOCK
ASSESSMENT

by

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I. Introduction

Exploited fish populations cannot be rationally managed without an understanding of the functional relationships between biological and fishing parameters and the consequent effect on stock dynamics. Often, resource scientists and managers have a critical need to know the biomass of exploited stocks and to have reliable estimates of these parameters in order to predict population or stock dynamics. While information obtained through analysis of commercial fisheries data does allow some estimation of these parameters, these data are biased both by the spatial distribution of fishing effort relative to that of population and by the selectivity of the fishing gear. Analyses performed on these data require the invocation of a number of assumptions which, in the absence of supporting evidence, often lead to conclusions which are inconsistent with other observed facts. It is for the purposes of resolving these contradictions and providing alternate, independent estimates of abundance and biological parameters that scientists undertake resource surveys. Naturally, such surveys carry with them their own set of assumptions which varies according to the design of the survey and the characteristics of the study organism(s). It is the purpose of this paper to review some of the common survey designs, their assumptions and applicability to groundfish resources, the limitations and uses of data from such surveys, and to suggest improvements to these designs consistent with the known attributes of the resources being studied.

While the majority of this paper will be devoted to survey or sampling design, it will also touch briefly on one aspect of surveys that is often ignored, that of accurate subsampling of catches. The estimation of species composition and catch weights has been dealt with by some authors...
(Hughes 1976, Westrheim 1967, 1976) but relatively little attention has been directed to the development of procedures for obtaining representative size/age frequencies, etc. from trawl hauls. These data become particularly important when used to generate vital parameters for population modelling studies. Known problems of stratification by species and size within the net codend should be accounted for in the subsampling procedures.

The analysis of data obtained from surveys presents some fundamental problems to the resource scientist because the analytical methods must make a number of major assumptions about the distributional nature of the population. Often, there is no way of determining the absolute distributional characters and the investigator must be satisfied with a proof that the data obtained do not deviate significantly from a postulated distribution. While this is relatively easy to do, unfortunately, many studies have not done so.

As a final note on results, it is seldom the case that survey results are used on a 'stand alone' basis, more often they are used as an independent corroboration of results obtained through other methods such as tagging studies, catch-curve analysis or various classes of dynamic population models. If the survey design fits the characteristics of the study organism and the data are analyzed appropriately, then the results must necessarily validate trends and parameter estimates derived elsewhere, or they must provide some alternate explanation consistent with known stock attributes. Where designs have minor deficiencies resulting from peculiarities of sampling situations, the investigator should provide some qualitative or quantitative assessment of the bias which may result.
II. Design of the Survey

Many of the criticisms directed at surveys as they are normally practised could be avoided or mitigated if sufficient time were expended beforehand in precisely defining the object(s) of the survey and reviewing relevant information on general problems and those specific to the study. Failure to do so often results in surveys which are of little direct use to the resource scientist because confidence limits are unacceptably large, etc. Such surveys are then justified in the basis that they may be 'good indicators of annual trends' whereas what the scientist really needs is a good estimate of the absolute value of the parameter being measured. The failure to produce such estimates can lead to articles, such as that in National Fisherman (June 1979), in the industry press.

Pre-survey activities should involve the formulation of a clear statement of the objectives, i.e. is the survey to: measure abundance; determine size or age composition; delineate distribution of various segments of the stock; determine the diel activities of the species; or some combination of these and other elements? This process should help the scientist assign the level of sampling which may be necessary at each station. For instance, sampling intensity of stock segregation studies would be considerably less than that required for determination of diel behaviour patterns.

The other major pre-survey activity should be a collation of available information which may be relevant to the variable under study. For example, any known characteristics of distribution or environment may be critical determinants of the eventual survey design and should be reviewed, even if only dealing with the generic or ordinal level of the subject species.
A great deal of time and effort in the field may be saved through this process, not to mention the obvious impact on results.

One of the primary constraints facing the resource scientist is the time or funding available with which to conduct the survey. Because the latter is generally limited the scientist may decide to collect the maximum possible amount of information, hoping that some compensation between quality and quantity of data will occur. In other words, the investigator may consider that he has little time available in which to modify the survey based on real-time results, particularly if the resource/area cell has received little previous attention. This is generally the case for widely distributed resources so that the investigator may be unable to quickly sample a 'representative' segment of the range. Where the latter is true caution must be exercised to avoid interpretation of local features as general population characteristics. Naturally, no one design will be applicable to all species due to interspecies differences in dispersion or behavioural patterns and intra-species seasonal effects such as migrations or spawning aggregations. Unfortunately, the investigator seldom has sufficient resources to delineate these temporal aspects, particularly for widespread stocks.

For most, if not all groundfish stocks there is some degree of overdispersion within the population, i.e. the distribution of individuals is contagious rather than regular or random. It may be confined to a grouping of population subunits in response to environmental features or physical characteristics, such as substrate availability or water mass attributes; or there may be distinct groupings within any homogeneous area. In the former case there may be random distribution of subunits within which there is
regular or random dispersion while in the latter there will be contagious distributions of individuals. Contagious distributions may thus arise in a number of ways and the size of sampling units must be appropriate not only to total area coverage but also to the magnitude of the spatial dispersion. The effects of the size of the sampling unit relative to heterogeneity in distribution will be discussed later.

It is uncommon that the investigator will be totally ignorant of the basic distributional properties of the resource. It is therefore possible to postulate a generalized distribution and design the survey not only to collect desired biological information but also to test the distributional hypothesis. In postulating the distribution, the investigator should be prepared to justify the assumptions made, on the basis of observed facts. For example, if a species of flatfish is thought to be contagiously distributed in relation to the presence of mud bottom, the hypothetical probability distribution must generate frequencies of occurrence which reflect this substrate distribution, as well.

Having postulated the distribution, the investigator must then design the survey appropriate to it. It is at this stage that the aforementioned fiscal and temporal constraints generally manifest themselves. The basic concern of the scientist will be either to obtain maximum precision of an estimate (e.g. mean) within a certain budget or to minimize the cost (generally with regard to areal coverage) for a specified precision. In two-stage sampling where the selection of clusters (= schools) may be the first stage, in some designs and the units of the clusters are the second stage, these constraints may affect either or both stages of the design. In this regard it is extremely useful to have some idea of the variance normally
associated with sampling so that optimum allocation of effort can be made. Techniques for such allocation will be discussed with individual designs.

A further exercise which can be undertaken at the design stage, given the postulated distribution and an estimate of the coefficient of variation expected, is to determine the efficiency of various designs. This is often difficult however, when the resource being surveyed shows highly contagious dispersion and is widespread, making estimation of variance or coefficient of variation subject to large errors.

Five general survey designs (random, systematic, random-stratified, cluster and encounter-response) will be examined with regard to allocation of sampling units by area, the optimum size of the sampling units, and relative efficiency of various designs. The treatment of data obtained will be left to Section IV. As a hypothetical framework I will assume that our resource: is highly overdispersed (aggregated); is normally distributed with depth, showing a mode of abundance that varies with time of year; and shows no latitudinal cline of abundance -- in short, a typical schooling groundfish.

(i) Random survey. A truly random survey is almost never used in groundfish research because the underlying assumption of random distribution is rarely hypothesized, i.e. that the population is dispersed either by chance or in response to some variable which is itself random in distribution. As noted earlier with regard to substrate and water mass characters, prime distributional variables are rarely, if ever distributed randomly. Random surveys are more common in agricultural surveys where the resource is immobile. There are, however, some significant points about random surveys which should be noted.
The basic procedure of random sampling will be to divide the total survey area \((N)\) into equal size units \((n)\) and select the desired number of units to be sampled at random. Most often this is achieved by serializing the \(n\) units and selecting the units to be sampled with the help of a random number table. In theory each unit has equal probability \((\frac{1}{n})\) of being chosen on any single sample, \(i\). A proof of the independence of the probability of selection \((\frac{1}{n})\) and the number of the sample, \(i\), is given by Sukhatme and Sukhatme (1970). The number of samples necessary to attain a certain precision is not estimable unless some estimate of the coefficient of variation or the variance is available. The problem is to determine what level of error, \(\varepsilon\) in the statistic is acceptable and how many samples, \(r\), will result in such a level of error. If we were estimating the mean abundance per unit area \(\bar{Y}_N\) for the population and wished to have an error \(\varepsilon\) with confidence \(1 - \alpha\), then we need to know \(r\) such that:

\[
P\left\{ \left| \frac{\bar{Y}_r - \bar{Y}_N}{\bar{Y}_N} \right| \geq \varepsilon \frac{\bar{Y}_N}{\bar{Y}_N} \right\} = \alpha
\]

Several authors give formulae for the derivation of \(r\) when the coefficient of variation, \(S^2/\bar{Y}_N\), is known however this is most often unavailable in groundfish resource surveys. Neyman (1934) suggests improving the sample design by preliminary sampling to derive an estimate, \(S^2\), of \(S^2\) through a small set of samples, \(k\). The additional samples needed to assure the desired precision can then be estimated by the following relationship
Again, assuming some data from preliminary sampling is available, Elliot (1970) gives a similar procedure for calculating the number of random samples required (n) for a specified index of precision (D):

\[ n = \frac{S^2}{D^2 \hat{\lambda}^2} \]

A further consideration with regard to the size of the sampling element applies to all sampling but will be outlined here and simply acknowledged for other designs. The detection of a particular distribution is critically dependent upon the relationship between the size of the individual or cluster and the size of the sampling element. If the distribution of the resource is either regularly or randomly distributed and there is a substantial difference in the size of the sampling element and the size of the individual or group, then the dispersion of the population is apparently random. As noted by Elliot (1970), "Most samplers will detect non-randomness if the sampling unit is small .... but even a small [unit] will not detect a contagious distribution if there are only a few individuals in each clump .... the dispersion of a population is effectively random if the density of the Population is low". This becomes critically important with random sampling because individuals may be clumped as well as solitary, in a randomly dispersed population. For contagiously dispersed populations the size of the sampling unit is also important, although only when the population is contagious with units of the contagion (clumps, schools) distributed regularly. When
this situation exists and the sampling unit is much smaller than the school, the dispersion is interpreted as random \( (S^2 \approx \bar{Y}_N) \); when the sampling unit is only slightly smaller, the dispersion is contagious \( (S^2 > \bar{Y}_N) \); when the sampling unit is only slightly larger, the dispersion is random \( (S^2 \approx \bar{Y}_N) \); and when the sampling unit is considerably larger, the dispersion is apparently regular \( (S^2 < \bar{Y}_N) \). When inferences about distribution are to be made, the investigator should ensure that no bias due to the size of the sampling unit exists; most simply achieved by varying the size of the sampling element and examining results for change in variance.

(ii) **Stratified-random survey.** Since most groundfish resources are aggregated (over-dispersed) to some degree, simple random sampling often results in excessively large variance of data obtained. A method of decreasing this variance is to design the survey so as to reduce the apparent heterogeneity in the population or survey area. The most common procedure for accomplishing this is to divide the area into strata which are more homogeneous than the entire area. While the strata may be of equal size, most often they are specified by some known distributional property of the resource, as interpreted through commercial catch records, known biology, etc. Strata may be of equal size, in which case the total area \( N \) is divided into \( k \) random samples (or some multiple thereof) are selected equally among the \( k \) strata. More often there will be reason to select strata of unequal size because equal-sized strata do not achieve the goal of generating homogeneous (or at least, less heterogeneous) sampling areas. When strata are of unequal size there are several methods of allocation of samples to the strata.
In the simplest case the total number of samples are divided so that the number of samples in each stratum \( n_1 \) is proportional to the area of the stratum \( k_1 \) relative to the total area \( A \): i.e.,

\[
\frac{n_1}{k_1} = \frac{n_2}{k_2} = \cdots = \frac{n_i}{k_i}
\]

If the \( \Sigma n_1 = N \) and \( \Sigma k_1 = A \), then the actual number of samples in each stratum will be determined according to the relationship

\[
\frac{k_1}{A} = \frac{n_1}{N}, \quad \frac{k_2}{A} = \frac{n_2}{N} = \cdots = \frac{k_i}{A} = \frac{n_i}{N}
\]

The investigator must then only determine the total number of samples feasible during the survey. The values \( \frac{k_1}{A} \) are the weights for each stratum and the sampling is called proportional stratified sampling and is said to be "self weighting" (Elliot 1970). The mean of the entire survey would then be

\[
\bar{y}_N = \frac{\Sigma y}{N} = \frac{n_1 \bar{y}_1 + n_2 \bar{y}_2 + \cdots + n_i \bar{y}_i}{N}
\]

and, if the total sample is less than 10% of the population, then the standard error is

\[
\sigma_n = \frac{1}{\sqrt{n}} \sqrt{n_1 s_1^2 + n_2 s_2^2 + \cdots + n_i s_i^2}
\]
While the surveyor may be satisfied with sample allocation proportional to the area of the strata there are other criteria for allocating sampling effort by strata. As in simple sampling the cost of the survey may be an overriding factor in determining the intensity of sampling, therefore the objective of the sampling may be to minimize the variance or standard error of the mean for a fixed cost. Such an allocation is called optimal allocation and the sampling intensity will reflect the standard deviation of the stratum. Where the funds available for the survey are fixed \((C_0)\), the size of sampled area in each stratum \((k_i)\) relative to the area of the stratum \((A_s)\) is given by

\[
 k_i = \frac{P_i S_i}{\sqrt{C_i}} \cdot \frac{C_0}{n \sum P_i S_i / \sqrt{C_i}}
\]

when \(P_i = \frac{A_s}{A_T}\) where \(A_T = \text{total area under survey}\).

\[
 C_i = \text{cost per sample in the } i^{\text{th}} \text{ stratum}
\]

\(k_i\) is then the sample size under an optimum allocation scheme. Such an allocation will yield an estimate of the mean with the maximum precision for the cost \(C_0\). Where the object of the survey is to derive a mean with the maximum precision and costs are a relatively minor consideration, the optimum size of sample per stratum is

\[
 k_i = \frac{N P_i S_i}{n \sum P_i S_i} \quad \text{where } \sum k_i = N, \text{ the total area sampled}
\]
Such an allocation of sampling by strata is normally attributed to Neyman (1934) and is referred to as Neyman allocation. Sukhatme and Sukhatme (1970) provide formulae for weighted means and variances under various allocation schemes.

Optimal allocation of sampling is not often employed in practice because of the requirement for previous knowledge of the variance within the strata. One method of overcoming this shortfall is to conduct preliminary sampling to provide estimates of \( S_1 \). Subsequent additional samples will then be determined according to:

\[
k_i = \frac{N P_i \hat{S}_1}{\sum_{i=1}^{n} P_i \hat{S}_1}
\]

The system of preliminary sampling is similar to that described for random sampling. This type of sampling involving approximation and resolution is generally referred to as sequential sampling. While of general usefulness in some fields such as agriculture, it has not gained widespread use in those concentrating on mobile, widespread and temporally dynamic resources such as demersal fish stocks. The reasons for this are more pragmatic than theoretical: where organisms are widely and contagiously distributed and survey time is limited, a significant portion of the survey time may be taken up with the travelling necessary to complete this preliminary sampling. In general, the \( \hat{S}_1 \) estimates must be fairly close together before optimal allocation loses its advantages.
Unfortunately, one of the goals of the stratification is to produce relatively homogeneous strata, viz. ones in which the $S_1$ values are lowest. One can readily envisage an optimal allocation survey where judicious strata selection results in similar $S_1$ values and consequently little gain over proportional sampling. A secondary consideration concerns the allocation of sampling when more than one characteristic is being examined; when this is the case optimal allocation may vary among the characteristics. One method of solving this dilemma is to choose an additional characteristic that is correlated to those of interest and use it in the construction of the strata.

The gains in efficiency due to stratification will be left to Section IV of the paper. Several authors have addressed the problem of the construction of strata, notably Dalenius (1950), Dalenius and Gurney (1951), Dalenius and Hodges (1957; 1959) and Ekman (1959). Cochran (1961) has investigated the assumptions of several of these authors, with regard to the values of $S_1$ and $\mu_1$ among strata, when applied to natural populations with leptokurtic distribution of character values.

There have been numerous groundfish surveys described by authors as 'stratified-random' designs, notably those of Grosslein (1969 et seq.). Indeed, the surveys initiated by Grosslein have received the most attention with regard to design and treatment of data (Clark 1979; Pennington and Grosslein MS 1978). Discussion of these results is contained in Sections IV and V.
(iii) Systematic surveys. In the initial stages of the investigation of a widespread resource, or for exploratory studies in which little is known of the organism under study, the investigator may choose a systematic design for his survey. This design has a number of advantages which make it attractive for some resource surveys and while there are some major disadvantages, systematic sampling will often answer the questions that the investigator is asking with equal precision and less cost than some surveys more elaborate in design.

Systematic sampling generally results in a regular spacing of sampling units. If there are \( N \) elements in the population (in our case this may be the total area, \( A \), under study) then they may be regarded as coming from a \( j \times k \) two-dimensional array. To select a systematic sample, the most common procedure would be to determine what interval between rows \( (i; i < j) \) and columns \( (m; m < k) \) is desired followed by the selection of a starting point for sampling. The latter is accomplished through the selection of a 'seed' pair of random numbers \( (a, b) \) where \( a < j \) and \( b < k \). Subsequent samples would then be in rows \( a, a + i, a + 2i \) \ldots \ and in columns \( b, b + m, b + 2m \) \ldots \. Points of intersection would be sample points. This scheme will result in a regular spacing of sampling units, called an aligned sample. Another approach to systematic sampling results in a pattern of sampling units which appears random but is constructed in a systematic manner. If we consider our survey area to be divided into a two-dimensional array composed of \( ml \) rows, each with \( nk \) units. The procedure is to select, independently, \( n \) random integers \( i_1, i_2 \ldots i_n \) (all \( i_n \leq 1 \)) and \( m \) random integers \( j_1, j_2 \ldots j_m \) (all \( j_m \leq k \)). The sampling coordinates will then be:
\((i_1 + r1, j_{r+1}), \ldots (i_a + r1, j_{r+1} + k), (i_a + r1, j_{r+1} + 2k) \ldots (i_n + r1, j_{r+1} + (n-1)k)\); for \(r = 0, 1, 2, 3, \ldots, (m-1).\) (After Sukhatme and Sukhatme 1970). This design results in a pattern of sampling stations which appears random and is called an unaligned sample.

Under certain conditions, an unaligned sample may provide better results than either an aligned or a stratified random sample (Das 1950; Quenouille 1949).

The primary advantages of the standard (i.e. aligned) systematic design are:
- sampling units are distributed uniformly throughout the area;
- relatively lower cost; and
- sampling stations are fixed, yielding better organizational control of field work.

There is therefore a low probability of missing a large contiguous part of the population. The major disadvantage of systematic surveys is that while they may give adequate representation of the spatial distribution of the resource, they do not give precise estimates of parameters (e.g. mean density) over the entire area. This is especially true for overdispersed populations where there may be high degree of autocorrelation among elements. As such, if the interval between sample units is similar to the inter-school (= cluster) distance of the population, variance of sample values will be high.
Two variants of systematic sampling are: **centric systematic area sampling** (after Milne 1959) in which the sampling units are selected from the exact geographic centre of each stratum (making this system closer to a stratified random sample); and **circular systematic sampling**. Circular systematic sampling is a modification to aligned sampling which helps to overcome the handicap of using constant size sampling units over fixed intervals, when dealing with periodically varying populations. In this method the allocation of sampling units is the same as aligned sampling but additional samples are taken by adding an integer unit to each of the \( i_n \) values, which has the effect of shifting the baseline of the sampling pattern; this process may be repeated as desired.

For groundfish surveys designed to provide some indication of population abundance systematic surveys have limited usefulness, primarily due to the difficulties associated with error calculation. This situation can be improved however, by the application of a systematic design to an area that has already been stratified by some criterion. When this is done and the strata are reasonably homogeneous then the systematic design can be used, as the underlying assumption of \( N_T = \sum \bar{n}_k \) (where \( \bar{n}_k \) = sample mean of \( k \) samples in the \( i^{th} \) stratum) is met. Many of the surveys conducted on the North American Pacific coast are of this type.

There are two other adjuncts to systematic sampling that have some kinship with encounter-response surveys (subsection (iv) of this section): the method of **contiguous quadrats** (Creig-Smith 1964) wherein
a fixed pattern is generated from a single point; and **pair-sampling** (Hughes 1962) wherein one sampling unit is located randomly and the second of the pair located a fixed distance from the first.

As a final note, while some authors treat **cluster sampling** as a separate technique, it is really a combination of systematic sampling and optimal sample unit choice.

(iv) **Encounter-response surveys.** While this designation has not, to my knowledge, been previously used to describe survey techniques, I believe it is appropriate to describe those surveys which are now being investigated by several agencies. This type of survey is a natural extension of area stratification and systematic sampling. It is uniquely attractive to overdispersed resources such as demersal fish. There are two basic approaches to encounter-response surveys: (a) an aggregation is encountered and a predetermined sampling pattern around the aggregation is generated to estimate its distribution and abundance; or (b) an aggregation is encountered and its distribution is mapped, after which the distribution is subsampled with a systematic pattern to determine its abundance.

The major advantages of the encounter-response survey are that: it reduces the necessity for an assumption of homogeneity over the sampled area; it greatly reduces the number of zero elements in the data; it closely resembles commercial fishing activity and is therefore amenable to charter-boat operation and promotes more confidence in results by the fishing industry (cf. National Fisherman, June 1979);
and, it is appropriate for multi-purpose surveys. The major disadvantages of the encounter-response survey are that: areas in between aggregations are not sampled or sampled at much lower densities; the planning of cruises is hampered by uncertainty as to time necessary for sampling; optimal planning of search patterns requires some foreknowledge of the distribution and dispersion of the resource. Fortunately, many of these disadvantages are mitigated by information available through commercial fisheries.

Where surveys are designed to provide estimates of abundance of stocks subject to commercial exploitation and information on the general distribution of that fishery is available, the search pattern for the cruise can be generated around this distribution. Where the survey is for exploratory purposes more time will be required for searching.

Estimates derived from encounter-response surveys will be minimum estimates due to the lack of, or minimal, coverage for areas between aggregations. There will however be greater precision to the estimates derived because the assumption of homogeneity over the sampling area will be met; this is seldom the case in even the best stratification. As noted earlier the application of this type of survey is unique to highly aggregated species; there is however, a limitation to this general feature. The encounter-response technique will only be of more relative value when the size of the response element is significantly smaller than that specified by any other design resulting in the same number of samples over the area. This is the case with widely distributed aggregations having relatively
uniform density such as Pacific hake and spiny dogfish. This is not a severe limitation however because other designs generally have a high proportion of zero elements, through sampling of intervals between aggregations.

The estimation of stock parameters (e.g. mean density) is enhanced by encounter-response surveys but the calculation of error limits about them may be problematical depending on the nature of the aggregations encountered. Where individuals in the aggregations have a distribution that is constant among aggregations, the values obtained for the estimates should describe their own distribution, for which error limits can be calculated. Often, however, the distribution of the fish school may be of the same order as the sampling unit and the result will be a Poisson distribution. In some instances, the distribution of individuals will vary among aggregations but when the foregoing holds true this variation will not be detected.

III. Sampling Techniques

The estimation of the values of various characteristics for a species requires some minimum number of fish. In most catches there may be considerably more fish than are needed and the catch must be sub-sampled. In addition to the features of an individual species, the species composition of the entire catch must be determined. These are two different problems and may require two approaches. Westrheim (1967; 1976) and Hughes (1976) have examined this problem and developed methods to cope with the inherent variability of trawl hauls and the stratification of species and sizes within the codend of the net.
One of the problems of subsampling of the catch for age composition of a given species is that it is impossible to know the ages of the fish when taking the sample. Most often the catch may be sampled on the basis of size using some understanding of the size-age relationship, however this relationship is not often verified. Indeed, to obtain proper estimates of biological parameters such as growth and mortality rates, sampling may depart substantially from proportional subsampling. This problem is acute for slow-growing and long-lived species such as rockfishes, dogfish, halibut, etc. Much more effort needs to be directed toward the statistical basis of age sampling particularly as concerns the distribution of ages at a given size.

While relatively little has been published on the subsampling of trawl catches, there is a rich literature on the general subject of subsampling. Most statistics textbooks give an adequate treatment of subsampling; Cochran (1964), Hansen, Hurwitz and Madow (1953) and Sukhatme and Sukhatme (1970) are noteworthy.

As a final point the investigator should not become so entangled with the perfecting of a method for obtaining representative samples of the catch that he or she loses sight of the fact that strong selective influences of fishing gear have already performed their work on the catch and that the object is generally to estimate population features, rather than those of the catch. It is therefore advisable to attempt to obtain samples using other or modified gears to validate or improve data obtained with standard survey gear.
IV. Treatment of Data Obtained From Surveys

Having designed the survey and conducted it as well as possible the investigator is then faced with mountains of data that must be collated, interpreted, analyzed or otherwise managed. At this point it is often instructive to reconsider the objectives of the survey and assess the damage to the original design wrought by weather and circumstances. If he or she has been blessed by fortune the damage will be minimal and energies can now be turned to the aforementioned tasks. Most often, these will result in some estimate of the mean value of a parameter complete with variance and confidence limits. The first thing that is evident in the data from most surveys is that the distribution of elements contributing to the mean is anything but normal and consequently these data must be examined in relation to some other distribution. If one of these distributions is appropriate, the data may be transformed to the normal distribution and statistics calculated in the usual way. As a caution, it should be noted that further analysis on transformed data may not always be recommended and the appropriate statistical references should be consulted.

(i) Distribution testing. In all of the surveys outlined in Section II, with the exception of the encounter-response surveys, the data obtained will normally exhibit some distribution of values which cannot be approximated to the normal distribution. This is almost always the result of the high degree of aggregation in fish species. This is obvious even at first glance and Taylor (1953) commented, "While variability ..... is expected to arise from imperfections associated with the sampling technique, it is clear that if the data are taken from a population distributed at random these imperfections would have to be of the
grossest kind to account for the variability observed". Fortunately, there are a series of papers dealing with the distributions exhibited by biological data and their relationships (Cassie 1962; Grieg-Smith 1964; Gurland 1957, 1958; Pielou 1960; Skellam 1952). Fisheries data in particular have been examined by Clark (1974, 1979), Lambou (1963), Moyle and Lound (1960), Roessler (1965) and Taylor (1953), among others.

The characteristic leptokurtic nature of biological data is similar to a number of unimodal or polymodal distributions and several authors have attempted to fit them to these data. Anscombe (1950) provides a rather technical examination of eight of these distributions (Thomas, Fisher Hh, Neyman A, Neyman B, Neyman C, Polya-Aeppli, negative binomial and Discrete lognormal). These distributions vary in skewness and in the number of modes possible: the negative binomial and the discrete lognormal have one mode; the Polya-Aeppli may have either one or two modes, while the Neyman Type A may have an unlimited number of modes. The negative binomial distribution has been applied to a number of fisheries studies, the classic paper being Taylor (1953). Anscombe (1950) examines several theoretical situations which may give rise to the negative binomial while Bliss and Fisher (1953) examine the relationship of the negative binomial to other distributions: "The negative binomial is an extension of the Poisson series in which the population mean, \( m \) [\( \lambda \) of the Poisson distribution] ... is not constant but varies continuously in a distribution proportional to that of \( \chi^2 \). As the variance of the negative binomial approaches the mean, or the overdispersion decreases, \( K \) [coefficient of aggregation] tends to infinity and \( p \to 0 \). Under these conditions, it can be shown [Fisher et al., 1943] that the distribution converges to that for the Poisson ... if the overdispersion increases sufficiently, \( k \to 0 \). If
we disregard the units containing no individuals, the negative binomial converges to Fisher's logarithmic series, which describes effectively the apparent abundance of species".

Two parameters define the negative binomial distribution, $X$, the mean value of the sampling unit and, $K$, which is an index of aggregation, inversely correlated with the degree of aggregation of the population ($K \approx \frac{X^2}{s^2 - X}$). The probability of obtaining $x$ units ($P_x$) in a sampling unit is

$$P_x = \frac{(k+x-1)!}{x! (k-1)!} \frac{R^x}{q^k}$$

Where $p = \frac{X}{K}$, $q = p+1$ and $R = \frac{p}{q}$

To obtain the actual frequency distribution the $P_x$ are multiplied by $N$, the total number of units encountered. This distribution is unimodal and as noted by Bliss and Fisher, "in fitting the negative binomial to a given distribution any apparent bimodality (or multimodality) is attributed to random sampling".

Several authors have investigated the fitting of the negative binomial to biological data (Anscombe 1949, 1950; Bliss and Fisher 1953; Debauche 1962). The most accurate method is the maximum likelihood method of Bliss and Fisher, however an approximation of $k$, $\hat{k}$, can be obtained thusly:
\[ \hat{k} = \frac{\bar{x}^2}{s^2 - \bar{x}} \]

When \( k < 4 \) this method is not very efficient unless \( \bar{x} \) is also less than 4, however this rough estimate of \( k \) can be inserted in the maximum likelihood equation

\[ n \log_e \left( 1 + \frac{\bar{x}}{\hat{k}} \right) = \sum \left( \frac{A(x)}{\hat{k} + x} \right) \]

where \( n \) = total number of sampling unit and \( A(x) \) is the total number of counts exceeding \( x \). The equation is balanced by iteration. For an alternate form of this procedure see Clark (1974). Anscombe (1950) gives formulae for five different methods of estimating \( k \) and their efficiencies. An asset to fitting the negative binomial are the tables in Williamson and Bretherton (1963) which include expected probabilities for 1480 negative binomial distributions.

The simplest and most common method of testing the goodness-of-fit of the data to the negative binomial is to estimate \( \bar{x} \) and \( \hat{k} \) from the sample (using maximum likelihood where \( n > 50 \)) and compare the data with the predicted negative binomial using a \( \chi^2 \) test. Anscombe (1950) gives two other tests (also in Elliot 1970) for estimating the goodness-of-fit for the negative binomial; one involving the frequency of zero elements and the other involving the difference between the sample estimate and the expected values of the third moment of the data. This reference is valuable in that Anscombe indicates not only when the data do not fit but also which distribution (discrete lognormal, Pólya-Aeppli or Neyman) is more appropriate.
(ii) Transformation of original data to approximate the normal distribution.

Since the majority of data sets from surveys are highly asymmetrical and do not lend themselves to the calculation of variances and error terms, it is desirable to effect a transformation to the normal distribution to facilitate this. There are also a great many analytical methods associated with the normal distribution which the investigator may wish to use, however the aforementioned constraints on the use of transformed data in subsequent analysis should be noted. In general these constraints are concerned with assuring both the independence of the variance and the mean and the additive nature of the variance (involving the t-test and analysis of variance, respectively).

Elliot (1970) provides several sections dealing with asymmetrical distributions and the appropriate transformation. A detailed treatment of transformations is contained in Quenouille (1950).

There are numerous methods of analysis used on survey data and it is not the purpose of this paper to review these methods, rather I will concentrate on the assessment of the survey as indicated by the data.

(iii) Comparison of survey schemes. Several authors present comparisons of various sampling schemes, among them Cochran (1964), Hansen, Hurwitz and Madow (1953), and Sukhatme and Sukhatme (1970). The following account is primarily derived from the first and last treatments. The most appropriate comparison to be made between surveys is the relative values of the variance of the mean of the characteristic under study. Since the precision of this estimate is a direct cost function, it will be of
importance in the survey design.

(a) Comparison of systematic and random sampling.

The variance of the mean of a simple random sample can be expressed

\[ V(\bar{y}_n)_R = \left( \frac{1}{n} - \frac{1}{N} \right) S^2 \]

where \( n \) units are chosen from a population of \( N \) and the mean square between units of the population is \( S^2 \). The most convenient expression of the variance of the mean of a systematic sample of the same population (as defined in the discussion of the unaligned sample) is

\[ V(\bar{y}_1)_{sy} = \frac{kn-1}{kn} \cdot \frac{S^2}{n} \left\{ 1 + \rho (n-1) \right\} \]

where \( \rho \) is the intra-class correlation between units of a column. The variance of the systematic sample mean relative to the random sample mean is therefore:

\[ \frac{V_{sy}}{V_R} = \frac{(nk-1) \left\{ 1 + \rho (n-1) \right\}}{n(k-1)} \]

The critical variable in this comparison is \( \rho \) which is essentially the measure of effectiveness of randomly formed columns in describing the physical distribution. For \( \rho = -1/(kn-1) \), the systematic and random samples are of equal precision; for \( \rho > -1/(kn-1) \) systematic sampling is inferior;
and for $\rho < -1/(kn-1)$ systematic sampling is superior. $\rho$ ranges between $-1/(n-1)$ and 1.

(b) **Estimates of the gain in precision due to stratification**

It is often difficult to estimate the gain in precision accounted for by stratifying the survey area because the true population values for the strata will be unknown. If the stratified sample is $n_1, n_2 \ldots n_k$ the variance of the weighted mean will be estimated by:

\[
\text{Est. } V(\bar{Y}_s) = \sum_{i=1}^{k} \left( \frac{1}{n_i} - \frac{1}{N_1} \right) p_i^2 s_i^2
\]

where $p_i = N_i/N$ and $n_i$ is the sample in stratum $i$ of a total of $N_1$ elements. If however the total sample was drawn without stratification then the variance of the mean would be the familiar

\[
V(Y_N) = \frac{N-n}{N} \cdot \frac{S^2}{n}
\]

We therefore must estimate $S^2$ given $\bar{Y}_{n1}, \bar{Y}_{n2} \ldots \bar{Y}_{nk}$ and $s_1^2$, $s_2^2 \ldots s_k^2$. This derivation is rather involved (Sukhatme and Sukhatme 1970) however when samples are allocated proportionately then an estimate of the gain in efficiency due to stratification will be
Est. \[
\frac{V(\bar{Y}_n) - V(\bar{Y}_{n-1})}{\text{Est. } V(\bar{Y}_n)_{\text{p}}} = \frac{N(k-1)}{(N-1)n} \left[ \frac{n s_b^2}{s_w^2} - 1 \right]
\]

\[
\approx \frac{k-1}{n} \left[ \frac{n s_b^2}{s_w^2} - 1 \right]
\]

where \( s_w^2 \) and \( n s_b^2 \) are called the mean squares among and between strata.

(c) **Comparison of systematic and random stratified sampling**

While Sukhatme and Sukhatme (1970) note that general conclusions cannot be drawn in this regard because of uncertainties about \( \rho \), the non-circular serial correlation coefficient, Cochran (1964) does provide some useful measures of relative efficiency. The former authors do provide some estimates of relative efficiency for auto-correlated populations but inter-class correlations in aggregated populations preclude general conclusions.

Cochrane (1964) estimated the variance of a systematic sample as:

\[
V(\bar{Y}_s) = \frac{s^2_{wst}}{n} \left( \frac{N-n}{N} \right) (1+(n-1) \rho_{wst})
\]

where:

\[
s^2_{wst} = \frac{1}{n(k-1)} \sum_{j=1}^{k} \sum_{i=1}^{2} (\bar{Y}_{ij} - \bar{Y}_s)^2
\]

is the variance among units from the same stratum,
and $\rho_{WS} = \frac{2}{n(n-1)(k-1)} \sum_{i=1}^{k} \sum_{j=1, j < i}^{n} \frac{(Y_{i,j} - \bar{Y}_{i,j})(Y_{1,i} - \bar{Y}_{1,i})}{S_{WS}^2}$

is the correlation between the deviations of pairs of items in the same systematic sample from the strata means. If $\rho_{WS} = 0$ the precision of the systematic sample is the same as the corresponding random stratified sample; if $\rho_{WS} < 0$ the precision of the systematic sample is greater than the corresponding random stratified sample; and if $\rho_{WS} > 0$ the precision of a systematic sample is less than the corresponding random stratified sample.

(d) A brief look at the adequacy of distributional hypothesis.

As noted earlier in the paper, the investigator fitting the survey data should be prepared to explain how such a distribution arises from the postulated dispersion of the population. This is of critical importance to subsequent treatment of data and inferences made about the population studied. Several authors have examined how the negative binomial distribution of survey data may arise from natural populations (Anscombe 1950; Bissell 1972; Bliss and Fisher 1953; Pennington and Grosslein 1978; Quenouille 1949 and Taylor 1953). It is pertinent to note that no consensus exists among authors as to the root causes: some favour heterogeneous Poisson sampling; others, randomly distributed clumps; while still others favour 'true contagion'. Indeed Bliss and Fisher (1953) quote an instance wherein the same frequency distribution of statistics is derived from distinct and quite contradictory hypotheses. As Elliot (1970) noted, "... the negative binomial
is often a good empirical description of a distribution, but agreement with a negative binomial should not be used as the sole basis for justifying one particular hypothesis."

V. Discussion

This review has covered most aspects of survey problems very thinly, however it is hoped that these highlights may help to focus attention on some of the shortcomings of surveys as we currently conduct them. It is important to note where the benefits of improved designs lie. The primary benefit, of course, is the derivation of more precise and hopefully more accurate estimates of various stock parameters. This will in turn allow us to more accurately predict stock dynamics and consequent yield. Another major benefit is the saving in time and resources which results from efficient cruise tracks and optimal sample allocation. Of particular note is the recent interest in encounter-response surveys; further development of this type of survey may well overcome some of the more significant shortcomings of traditional designs.

I would also like to reiterate an oft-heard plea; that biologists should seek the assistance of statisticians in the preliminary stages of surveys -- there is much mutual benefit to be gained. As a final note, an element of solace often creeps into the rather technical literature on survey results and one in particular stands out. Buried in Appendix E of Taylor's (1953) paper is a sentence that does help to revive flagging spirits; "When the variance is a function of the mean of a distribution, the effect of the size
of sampling unit on the efficiency of sampling and on the precision of estimates cannot be expected to be obvious to the biologist." Taylor's wisdom in placing it in the Appendix rather than the Introduction is admirable.
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1979 December 7

To    L. Margolis, Canada
      I. Ikeja, Japan
      F.H. Fukuhara, United States

From   Assistant Director

Gentlemen

We have been advised of the following correction required for INPFC Doc. 2251 titled "A brief review of survey methodology with regard to groundfish stock assessment" by E.M. Leaman, Department of Fisheries and Oceans, Canada.

In the Literature Cited, the reference to National Fisherman, 1979 should be deleted and the following reference added:


C.R. Forrester
Assistant Director
November 30, 1979

Dr. C. R. Forrester
Assistant Director
International North Pacific Fisheries Commission
6640 Northwest Marine Drive
Vancouver, B.C.
V6T 1X2

Dear Clif:

Enclosed, please find a revision of my paper on survey methodology. There was an error in one of the references in the original and since it was a fairly significant reference, I felt it should be corrected. The original document no. is 2251. Apologies for the oversight.

Regards,

B. M. Leaman
Groundfish Biologist
BML:cr
encl.

is it necessary to issue an add. of 'revision to a Doc.'?