

Effects of Measurement Errors on the Assessment of Stock-Recruitment Relationships

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Error in measuring spawning stocks can have a profound effect on the appearance of stock-recruitment relationships. Large errors make recruitments appear to be independent of spawning stocks. This effect promotes overexploitation rather than simply making the relationship noisier and harder to measure. Efforts to explain variation in recruitment through factors other than spawning stock may be deceptive as well. Much fisheries theory and practice are based on the assumption that recruitment is largely independent of spawning stock; that assumption is not credible except in cases where spawning stocks are known to have been measured accurately.

Key words: stock, recruitment, statistics, overexploitation

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Les relations stock-recrutement peuvent être profondément influencées par des erreurs de mesures des stocks reproducteurs. De grandes erreurs font que le recrutement semble être indépendant des stocks reproducteurs. Cet effet encourage la surexploitation plutôt que de simplement rendre la relation plus criante et plus difficile à mesurer. Il peut être tout aussi trompeur de faire appel à des facteurs autres que le stock reproducteur pour expliquer la variation du recrutement. La théorie et la pratique des pêches reposent surtout sur l'hypothèse que le recrutement est pour une bonne part indépendant du stock reproducteur: cette hypothèse manque de crédibilité, à moins qu'on ne soit certain que les stocks ont été mesurés avec précision.

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THE relationship between stock and recruitment is a central problem in fishery dynamics. Suggestions to deal with it range from pretending that it does not exist (assume recruits independent of spawners) to arcane "adaptive control" schemes for probing the underlying relationship through deliberate manipulation of harvest policies. Some workers (the artists among us) suggest drawing recruitment curves by eye, while others advocate objective statistical procedures for curve fitting. Still others observe all this with disgust and seek to explain recruitment variation as response to various environmental factors. Most of us have missed a very basic point that has been mentioned in the literature and which statisticians call the "errors in variables" or "structural relationship" (Kendall and Stewart 1973) problem: spawning stocks can rarely be measured or indexed without considerable sampling error, and observed values should therefore not be used as an independent variable in standard correlation and regression analyses. This paper will address some consequences of that statistical point.

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Recognizing Recruitment Overexploitation

Fisheries managers should be particularly concerned with the appearance of spawner recruitment data from stocks that are suffering "severe" recruitment overexploitation. By severe we shall mean any situation where, in the absence of sampling errors, each additional fish allowed to spawn will appear to contribute (on average) at least as many recruits as each spawner already being allowed. In the absence of compensatory mortality effects, this rather complicated definition translates into the simple recruitment relationship

$$(1) \quad \hat{r}_{t+1} = A s_t$$

where \hat{r}_{t+1} is average total recruitment, s_t is the number of spawners one generation earlier, and A is recruitment per spawner. In the presence of compensatory effects, we expect to see the ratio \hat{r}_t/s_{t-1} increasing (A not constant) with s_t .

A simple statistical test for severe overexploitation would be to regress linearly r on s for data collected during the suspect years. The intercept of this regression should be less than or equal to zero if severe overexploitation has occurred. If environmental effects are expected to be multiplicative

(more random variability at higher s), then a less-biased alternative would be to regress $\ln(r/s)$ on s . This regression should have a zero or positive slope if there has been severe over-exploitation. The common objection to regressions of ratios, namely that a negative slope will be apparent even if r is completely independent of s , does not apply in this case: if r is independent of s , then recruitment overexploitation has not occurred, and we would actually want to see this reflected as a negative regression slope.

Most basic statistics texts warn that standard linear regressions are valid only if the independent variable (s in this case) is measured without error. Ricker (1973a, 1975: Appendix IV) and Peterman (1978) have recently carried the warning specifically to fisheries analysts. Much of what follows can be viewed as a demonstration of how important these warnings are to the fisheries manager.

Visual Appearance of Data from Overexploited Stocks

This section uses a simple simulation model to demonstrate the effect of spawning count errors on the appearance of stock recruitment data. The modeled population is always severely overexploited, as in equation (1), and is further subject to random environmental effects:

$$(2) \quad r_{t+1} = s_t e^{\alpha + u_t}$$

where $s_t = r_t (1 - h_t)$, h_t is harvest rate, α is a productivity parameter, and u_t is a normally distributed random variable with mean 0.0 and variance σ_u^2 (representing effects of environmental variation). The assumption of normally distributed u is not critical to our argument, but can be justified theoretically on the grounds that survival from egg to recruitment is determined by a large number of nearby independent but variable risks. The harvest rate h_t is assumed to increase with r_t , so that population size is regulated by fishing, and the harvest rate exhibits some further variation unrelated to stock size. There are several ways to generate h_t with such properties: we simulate a collection of stocks each following equation (2) but having independent environmental effects, and h_t is then chosen so as to provide a fixed total escapement across the collection.

To generate "observed" data, we assume that spawning stocks are estimated with multiplicative errors:

$$(3) \quad S_t = s_t e^{v_t}$$

where S_t is observed spawners, s_t is actual spawners, and v_t is a normally distributed random variable with mean 0.0 and variance σ_v^2 . Observed recruitment is then estimated as $R_t = S_t + r_t h_t$, i.e. we assume that errors in estimating catch are insignificant. Various other assumptions about counting errors were also tested (i.e. $S_t = s_t (1 + w_t)$, w_t normally distributed or uniformly distributed): they produce essentially the same effect and will not be discussed further. The magnitude of counting errors implied by equation (3) can be most easily seen by example: for $\sigma_v^2 = 0.1$, 95% of S_t values will be between 0.54 s_t and 1.86 s_t ; for $\sigma_v^2 = 0.5$, the 95% limits are 0.25 s_t and 3.97 s_t . Occasional errors of these magnitudes (i.e. off by a factor of 2 to 4) would not be considered unusual in most fisheries stock assessments, especially when the esti-

mates are based on tagging studies.

Figure 1 shows "actual" and observed stock-recruit data for two simulation cases, both with $\sigma_u^2 = 0.1$ and $\sigma_v^2 = 0.25$. Because of counting errors, the observed spawning stocks appear to vary more widely than was actually the case. The visual effect on recruitment is dramatic: most observers would conclude in both cases that recruitment was independent of spawning stock. A more daring analyst might see a dome-shaped relationship in the first case, and would at least conclude that very high spawning stocks result in below-average recruitments.

We examined dozens of simulation cases, with σ_v^2 ranging from 0.01 to 0.5. It is hard to find objective criteria for visual distortion, but we noticed significant curvature in most "observed" relationships for which $\sigma_v^2 \geq 0.05$ (counting errors of roughly $\pm 50\%$). For $\sigma_v^2 > 0.1$, we generally concluded that observed recruits were either independent of observed spawners or showed some dome-shaped relationship. In most cases where $\sigma_v^2 > 0.1$, the highest observed spawning stocks were associated with observed recruitments below average, and there was a tendency for the highest observed recruitments to occur at very low observed spawning stocks.

Patterns similar to the high variance cases are obtained when one plots successive pairs of "stock-recruit" points from a time series of completely independent observations (i.e. $R_t = \mu + w_t$, where μ is the population mean and $\text{cov}(w_t, w_{t+1}) = 0$). Eberhardt (1970) notes that such series will on average show an R_{t+1} vs. R_t correlation coefficient of -0.707. An intuitive explanation for Fig. 1 is that counting errors simply obliterate the stock-recruit relationship and replace it with a time series of nearly independent observations.

A common reaction by biologists faced with noisy stock-recruit data as in Fig. 1 has been to seek "environmental factors" that might explain variation in recruitment. Such efforts have not been particularly successful and could well be as deceptive as the stock-recruitment analyses that they seek to replace. Factors such as river flows (for Pacific salmon) and ocean temperatures may affect measurement processes as well as (or instead of) biological responses. As an obvious example, high river flows may cause difficulties in counting Pacific salmon spawners: underestimates of spawning would then lead to overestimates of recruits per spawner, and high river flows at spawning times would thus appear to have positive effects on spawning success.

Biases in Estimated Stock Recruitment Curves

One might argue that the deceptive patterns mentioned above are just visual illusions, and that objective statistical procedures should be used to detect the actual recruitment relationship. That is, one should assume some model for the relationship, then estimate the model's parameters from the data. If the model contains equation (1) as a special parameter case (i.e. $\beta = 0$ in Ricker model $r_t = s_{t-1} e^{\beta s_{t-1}}$, or $\beta = 1$ in the power model $r_t = \alpha s_{t-1}^\beta$), then the parameter estimates should behave accordingly. That is, the null hypothesis of equation (1) should not be falsely rejected because of environmental fluctuations and observation errors.

It does appear possible to develop unbiased estimators for the parameters of simple stock-recruit models, given inde-

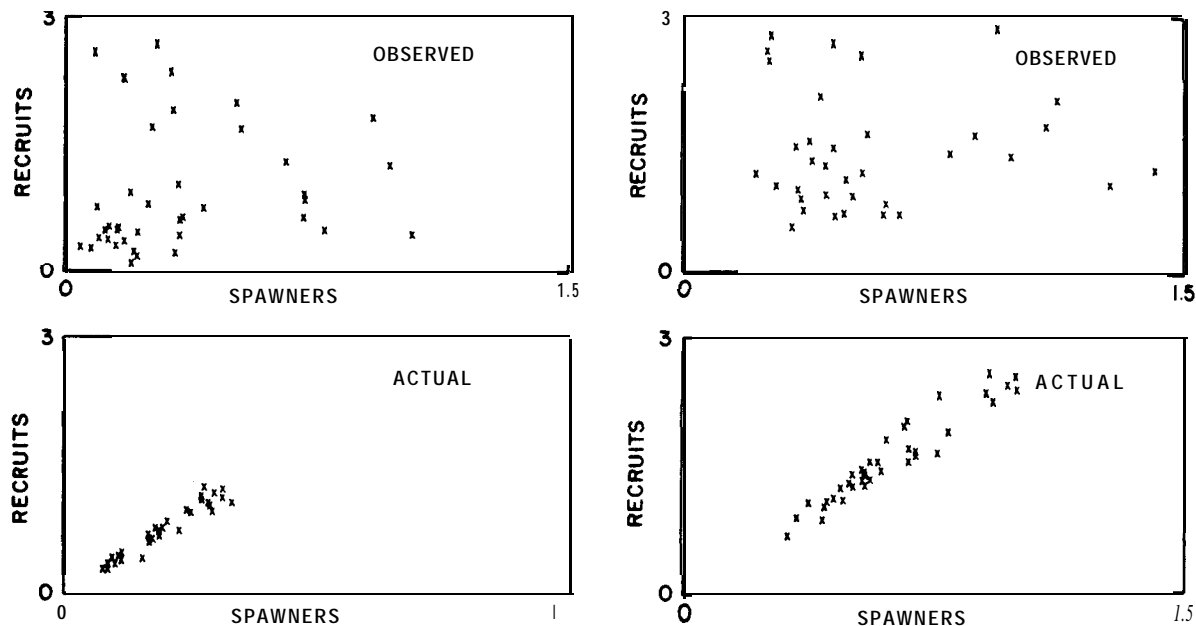


FIG. 1. "Actual" and observed stock-recruit relationships for two simulated populations that are severely overexploited. The actual data were generated with recruitment proportional to spawning stock; the observed data were generated by applying observation errors to the actual data.

pendent estimates of the observation error variance σ_v^2 (Ludwig and Walters 1981). However, standard least squares regression procedures, as advocated, for example, by Dahlberg (1973), Cushing and Harris (1973), and Walters and Hilborn (1976), are just as deceptive as visual examination of the data.

To illustrate what goes wrong with standard procedures, this section develops simple, consistent (asymptotically unbiased) estimators for two stock-recruitment models, and examines the bias in uncorrected estimators. The algebra will be somewhat involved, and general readers may skip the following without missing any basic arguments. The models to be examined are

$$(4a) \quad r_j = s_{j-1} e^{a+u_j}$$

$$(4b) \quad r_j = s_{j-1} e^{a+bs_{j-1}+u_j}$$

Here r_j denotes the number of recruits in generation j , and s_{j-1} denotes the number of spawners in the previous generation. The random survival of offspring is accounted for by u_j ($j=1, \dots, n$), which are assumed to be independent, normally distributed random variables with mean 0 and variance σ_u^2 . The parameters a and b which appear in (4) may be estimated by linear regression (Dahlberg 1973; Ricker 1975). Let

$$(5a) \quad y_j = \ln r_j = a + b x_{j-1} + u_j,$$

$$(5b) \quad q_j = \ln (r_j/s_{j-1}) = a + b s_{j-1} + u_j,$$

where

$$(6) \quad x_j = \ln s_j.$$

Then the method of least squares leads to equations for the power model:

$$(7a) \quad \bar{y} = \hat{a} + 6 \bar{x},$$

$$(8a) \quad 6 = \frac{C_{xy}}{C_{xx}},$$

and for the Ricker model:

$$(7b) \quad \bar{q} = \hat{a} + 6 \bar{s},$$

$$(8b) \quad \hat{b} = \frac{C_{sq}}{C_{ss}}.$$

Here the bar denotes the sample mean and C_{xy} denotes the sample covariance:

$$(9) \quad \bar{y} = \frac{1}{n} \sum_{j=1}^n y_j, \text{ etc.}$$

$$(10) \quad C_{xy} = \frac{1}{n} \sum_{j=1}^n x_{j-1} y_j - \bar{x} \bar{y}, \text{ etc.}$$

The estimates \hat{a} and \hat{b} in (7) and (8) are consistent if r_j and s_j are uncontaminated by observation errors.

THE EFFECT OF OBSERVATION ERRORS

Now we assume as in the previous section that observations of r_j and s_j are subject to the same relative error (r estimated as catch plus escapement, no error in catch estimate). Let R_j and S_j denote the observed values. Then we write

$$(11) \quad R_j = r_j e^{v_j}, S_j = s_j e^{v_j}, Q_j = \ln (R_j/S_{j-1}),$$

$$X_j = \ln S_j, Y_j = \ln R_j,$$

where v_j are independent, normally distributed random variables, with mean 0 and variance σ_v^2 .

We define the bias correction factor

$$(12) B = \exp\left(\frac{1}{2} \sigma_v^2\right)$$

Then consistent estimates of a and b are given for the power model by

$$(13a) \bar{Y} = a^* + b^* \bar{X},$$

where

$$(14a) b^* = \frac{C_{XY}}{C_{XX} - \sigma_v^2}$$

and for the Ricker model by

$$(13b) \bar{Q} = a + b \bar{S}/B, \text{ where}$$

$$(14b) b^* = \frac{[C_{SQ} + \bar{S} \sigma_v^2] B^2}{C_{SS} - \bar{S}^2 (B^2 - 1)}$$

Equations (13)–(14) are derived in the Appendix.

Equation (13a) has the same form as (7a); observation errors may be disregarded in that equation. However, the factor B in (13b) corrects a bias in \bar{S} , which is due to the nonlinear appearance of v_t in S_t . Similarly, the denominator in (14a) corrects C_{XX} by subtracting the variance of the observation error. Thus failure to correct for observation error affects the parameter estimates in two ways: it produces an overestimate of the information provided by the data. For the power model, both effects arise because, in expectation, the sample variance C_{XX} is biased by an amount σ_v^2 . Also one usually has $C_{XX} > 0$, since density-independent recruitment corresponds to $b = 1$. Therefore, observation error generally leads to an underestimate of b^* , which in turn leads to an underestimate of the maximum sustainable yield (MSY) level of spawners. The observation errors encourage over-exploitation, and they produce an illusion that this estimate is based upon more information than is actually present.

The interpretation of (14b) is more complicated. The denominator is analogous to the denominator of (14a). It is shown in the Appendix that

$$(15) \lim_{n \rightarrow \infty} C_{SS} = \lim_{n \rightarrow \infty} [C_{SS} - \bar{S}^2 (B^2 - 1)]/B^2$$

That is, (15) provides a consistent estimate of the denominator of (8b). The term $\bar{S} \sigma_v^2$ in the numerator of (14b) corrects the effect of correlation between errors in Q and in S ; the additional factor B corrects the bias in \bar{S} , as in (13b).

The direction of bias in the uncorrected estimate of b is not clear from (14b). This uncorrected estimate \hat{b} is given in analogy, with (8b):

$$(16) \hat{b} = \frac{C_{QS}}{C_{SS}}$$

It is shown in the Appendix that

$$(17) \lim_{n \rightarrow \infty} (\hat{b} - b) = \lim_{n \rightarrow \infty} -b \left[(1 - B^{-2}) + \frac{1 - B^{-2} \bar{S}^2}{B C_{SS}} \right] - \sigma_v^2 \frac{\bar{S}}{C_{SS}}$$

Since $B > 1$, the coefficient of b on the right-hand side of (17) is positive. Thus if $b < 0$ (as would usually be the case), then the two terms on the right-hand side of (17) will have different signs. However, if b is small (if there is weak density dependence), then the right-hand side of (17) is negative. Therefore, if density dependence is weak, the effect of observation errors is to overestimate the amount of density dependence. This leads to overexploitation, as was the case for the power model. On the other hand, if density dependence is strong ($-b$ is large), then observation errors lead to underexploitation.

Effects on Adaptive Performance

Biases in stock/recruit parameter estimates do not automatically imply poor management of harvests. The biases might cancel one another in assessments of optimum escapements, or the optimum might be relatively insensitive to the parameter estimates. For example, optimum escapements estimated by the Ricker model are relatively insensitive to the α parameter. Further, the observed exploitation history can have a profound effect on performance of any statistical procedure. If data are available only since the stock has become heavily exploited (i.e. all observed spawning stocks relatively low), estimation performance would be generally much poorer than for cases where data are also available from the early development of the fishery (Hilborn 1979).

To see more clearly the management effects of biases, we elected to simulate the performance of a "naive adaptive manager." The manager is faced with a population that behaves according to the Ricker stock-recruitment model (eq. 4b), with "actual" parameters $a = 1$, $b = 1$, and $\sigma_v^2 = 0.25$ (random environmental effects are quite large). He is given an initial exploitation history of five stock/recruit data points. Two cases are considered: in the first, his historical data represent only a period of high exploitation, where the initial recruitment $R_0 = 0.1$ and the historical exploitation rate has been 0.8 on each of the five cycles. In the second, his record involves a development history where $R_{0i} = 1.0$, and the sequence of past exploitation rates has been 0.1, 0.2, 0.3, 0.4, and 0.5.

After each cycle, the naive manager is assumed to know the catch exactly, and to estimate the spawning stock with a multiplicative error as in equation (3). He then has one more stock/recruit data point and proceeds to update his estimates of the Ricker model parameters by applying the usual linear regression equations (eq. 7-8) uncorrected for the spawning count errors.

He then uses the updated parameter estimates and the last spawning stock observation to "forecast" the next recruitment, and estimates the best spawning stock to allow. Here he operates as a passive adaptive manager (Walters and Hilborn 1978); he assesses the best spawning stock according to the equations in Ricker (1975), which do not account for uncertainties. He then estimates an exploitation rate $h_t = (\hat{R}_t - \hat{S}_t^*)/\hat{R}_t$, where \hat{R}_t is his forecast recruitment and \hat{S}_t^* is his estimated optimum spawners. It is assumed that he can achieve this rate exactly: his catch is then $h_t \hat{R}_t$ (rather than $\hat{R}_t - \hat{S}_t^*$).

In Table I results from Monte Carlo simulations for the two-population history cases and for three levels of counting

TABLE 1. Performance measures for a naive adaptive manager who does not account for errors in spawning stock estimate¹

Average estimates from 10 simulations over 20 generations each (ranges in parentheses)	Population initially overexploited counting error variance			Population initially unharvested counting error variance		
	0.0	0.0625	0.25	0.0	0.0625	0.25
20-yr catch	4.00 (2.14-6.65)	3.33 (1.57-5.32)	2.62 (1.21-5.02)	6.69 (4.75-8.41)	6.56 (4.61-8.58)	5.90 (3.88-8.09)
Ricker α parameter (at generation 20)	1.04 (0.90-1.13)	1.12 (0.93-1.39)	1.24 (1.02-1.53)	1.03 (0.85-1.38)	1.12 (0.98-1.46)	1.25 (1.04-1.58)
Ricker β parameter (at generation 20)	1.12 (0.62-1.86)	2.79 (0.70-9.07)	4.57 (0.61-13.4)	1.03 (0.59-1.66)	1.15 (0.77-1.77)	1.32 (0.92-1.83)
"Optimum" escapement (at generation 20)	0.44 (0.71-0.66)	0.34 (0.11-0.66)	0.25 (0.05-0.71)	0.45 (0.34-0.65)	0.42 (0.31-0.55)	0.40 (0.27-0.57)

error are summarized. To simplify comparisons, the same sequences of random numbers were used to generate environmental effects and counting errors for each combination of population history and counting error variance (the random sequence was different from simulation trial to trial for each combination).

Table 1 indicates that the initial data history is a more important determinant of management performance than are biases associated with counting errors. If the historical data have all been collected at low stock sizes, the passive adaptive manager has trouble initially in estimating the equilibrium stock size (though he usually achieves a good estimate by generation 20 in the absence of counting errors) and must give up catches during the recovery period as well. Counting errors have a devastating effect on his ability to "see" that the stock has been overexploited in the first place, and on average he is still using a spawning stock that is almost 50% too low after 20 cycles.

The naive manager generally fares much better when he faces a data history of developing exploitation. The biases in his parameter estimates are not severe even when counting errors are large, and his estimate of the optimum spawning stock is usually quite good after 20 cycles. However, these results should be viewed with caution. In practice, the early development history of a fishery is likely to involve considerable changes in counting methods (and thus σ_e^2) and the actual stock parameters are likely to change as unproductive substocks are reduced (Ricker 1973b).

Discussion

Since a severely overexploited stock can appear to behave like the examples in Fig. 1, it is obvious that the fishery manager should not trust models based on the assumption that recruitment is independent of spawning stocks, at least until he can clearly demonstrate that spawning stocks have been measured "accurately" (i.e. errors less than $\pm 30\%$ or so). Such demonstrations are rare in the fishery literature. For most salmon stocks in British Columbia, the data recording system alone generates unacceptably large errors: counts are recorded only by abundance classes, and the midpoint of the class is taken to be the best estimate. With a bit of digging, we could probably demonstrate comparable errors for other

fishery data sets. A basic point is that stock and recruitment data sets should not be published or used unless they are accompanied by estimates of measurement error variance; otherwise they are meaningless and misleading.

Many researchers have taken for granted that recruitment is largely independent of spawning stock, and have gone on to seek other mechanisms to explain apparent variation. It is frightening to realize that much of this work may be directed to explaining patterns that do not exist in the first place.

We do not mean to imply that recruitment is often strongly dependent on spawning stock, just because evidence to the contrary can be misleading. In fact, the analysis leading to Fig. 1 suggests an interesting model for how recruitment can actually be independent of total spawning stock over a wide range of stock sizes. Suppose total spawners are measured very accurately, but that only a randomly variable and unobserved proportion of these spawners are actually successful. Suppose that each successful fish produces (on average) the same number of recruits, and that there is no effect of density on survival to recruitment. (Such a population would have to be regulated either by fishing or by some compensatory mechanism that is not exhibited over the range of observed stock sizes.) Then data like Fig. 1 will result where recruits are plotted against total spawners: in effect total spawners give a poor estimate of the actual number of spawners participating in the density-independent recruitment process. Suppose further that the successful spawners in any year represent particular genotypes whose eggs and fry carry the right mix of temperature-sensitive enzyme alleles to develop successfully in the temperature regime prevalent that year. Then from year to year, temperature will appear to have some effect on recruitment, but the response will be inconsistent because of changes in gene frequencies in the total spawning population. In this system, the scientist who seeks environmental correlates of recruitment (as a prelude to pursuing more detailed research) may fail to recognize either spawners or temperature as key factors.

It is time for a thorough reexamination of the evidence regarding stock and recruitment in fisheries. We can suggest procedures to help correct for measurement errors (Ludwig and Walters 1981), to make some historical data sets more usable for management. But the real need is for functional rather than correlative studies on factors affecting the re-

cruitment process, and for careful analyses of measurement errors and means to reduce them.

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Appendix

Derivation of Consistent Parameter Estimates

1. POWER MODEL

The basic regression equation is (5a), but the observed quantities are given by (11). These equations imply that

$$(18) Y_j = a + b X_j + u_j + v_j - b v_{j-1}, \quad j = 1, \dots, n.$$

If Y_j is averaged over the observations, the result is

$$(19) \bar{Y} = a + b \bar{X} + \bar{u} + \bar{v} - b \bar{v}_{-1},$$

where

$$(20) \bar{v}_{-1} = \frac{1}{n} \sum_{j=1}^n v_{j-1}.$$

Each of the random variables \bar{u} , \bar{v} , and \bar{v}_{-1} has mean zero and variance proportional to $\frac{1}{n}$, by our assumptions. Therefore

$$(21) \lim_{n \rightarrow \infty} \bar{Y} = a + b \lim_{n \rightarrow \infty} \bar{X}.$$

If Y_j is multiplied by X_j before averaging, the result is

$$(22) \overline{XY} = a \bar{X} + b \overline{XX} + \overline{uX} + \overline{vX} - b \overline{v_{-1}X}.$$

If (19) is multiplied by \bar{X} and subtracted from (22), we obtain

$$(23) C_{XY} = b C_{XX} + C_{uX} + C_{vX} - b C_{v_{-1}X},$$

where the sample correlation coefficients are defined by (10). By our assumption, u and X are uncorrelated, and v and X are uncorrelated. However,

$$(24) \lim_{n \rightarrow \infty} C_{v_{-1}X} = \sigma_v^2$$

Therefore

$$(25) \lim_{n \rightarrow \infty} C_{XY} = b \lim_{n \rightarrow \infty} (C_{XX} - \sigma_v^2).$$

Equations (21) and (25) imply that (13a) and (14a) provide consistent estimates.

2. RICKER MODEL

Equations (5b) and (11) imply that

$$(26) Q_j = a + b S_j + w_j - v_{j-1}, \quad j = 1, \dots, n.$$

where

$$w_j = u_j + v_j.$$

If Q_j is averaged over the observations, the result is

$$(28) \bar{Q} = a + b \bar{S} + \bar{w} - \bar{v}_{-1}.$$

As in the previous case,

$$(29) \lim_{n \rightarrow \infty} \bar{Q} = a + b \lim_{n \rightarrow \infty} \bar{S}.$$

In view of (11) and (12),

$$(30) \lim_{n \rightarrow \infty} \bar{S} = B \lim_{n \rightarrow \infty} \bar{s}.$$

Therefore

$$(31) \lim_{n \rightarrow \infty} \bar{Q} = a + b \lim_{n \rightarrow \infty} \bar{s}/B.$$

If (27) is multiplied by S_j before averaging, the result is

$$(32) \overline{QS} = a \bar{S} + b \overline{sS} + \overline{wS} - \bar{v}_{-1} \bar{S}.$$

If (28) is multiplied by \bar{S} and subtracted from (32), we obtain

$$(33) C_{QS} = b C_{sS} + C_{wS} - C_{v_{-1}S}.$$

By our assumptions,

$$(34) \lim_{n \rightarrow \infty} C_{wS} = 0.$$

$$(35) \lim_{n \rightarrow \infty} C_{v_{-1}S} = \lim_{n \rightarrow \infty} \bar{s} E(v e^1) = \lim_{n \rightarrow \infty} \bar{s} B \sigma_v^2 = \sigma_v^2 \lim_{n \rightarrow \infty} \bar{s}.$$

$$(36) \lim_{n \rightarrow \infty} C_{sS} = B \lim_{n \rightarrow \infty} C_{ss}.$$

$$(37) \lim_{n \rightarrow \infty} C_{ss} = \lim_{n \rightarrow \infty} [B^2 (C_{ss} + \bar{s}^2) - B^2 \bar{s}^2],$$

equation (37) implies (15). When (34)-(37) are substituted into (33), the resulting equation implies (14b).

To derive (17), we substitute (16) into (14b). Thus

$$(38) \lim_{n \rightarrow \infty} [C_{ss} - \bar{s}^2 (B^2 - 1)] \frac{b}{B^2} = \lim_{n \rightarrow \infty} [b C_{ss} + \bar{s} \sigma_v^2].$$

After rearrangement, (38) implies

$$(39) \quad \lim_{n \rightarrow \infty} [(b - b) C_{ss}] = \lim_{n \rightarrow \infty} \left\{ -b [\bar{S}^2 \frac{B^2 - 1}{B^3} + C_{ss} (1 - \frac{1}{B^3})] - \bar{S} \sigma_v^2 \right\}.$$

Then (39) implies (17). There must be an easier way to make a living.