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Evaluating trends in catch rate from voluntary fishing logs in the Nova Scotia lobster fishery

by

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Abstract

Estimates of catch-per-trap-haul (CPTH) in the Nova Scotia lobster fishery have been based on sea samples, interviews, fishing logs and records from lobster cooperatives. Fishing logs or lobster cooperative records are probably the most cost-effective way to obtain CPTH data and these have been maintained in a substantial number of Nova Scotia fishing ports since the mid-1980s. The existing log system has not been evaluated using statistical criteria. Ports have been represented by few logs (usually < 5) and statistical power is a concern. This paper looks at the consequences of low N, and suggests a strategy for evaluating CPTH trends over fishing seasons. To test for differences between seasons, contrasts were evaluated with decision-based error rates using the method of Rodger (1974, 1975).

Two constraints are identified in evaluating seasonal trends in CPTH. The first is that the calculation of season CPTH is sensitive to the weeks of the season that are included, since there is substantial within-season variation in CPTH. Thus if CPTH is to be compared for different seasons, each CPTH estimate should be based on the same weeks. The second constraint is that, since the same fishers reappear over 2 or more seasons, randomized-blocks anova should be used because statistical power would be lost if the correlation of individual fisher's CPTH is ignored. Since there is some turnover in fishers that maintain voluntary fishing logs, the number of logs used in the analysis will usually be less than the total number of logs maintained. To alleviate the problem of losing voluntary log keepers over time, we suggest analyzing trends across a few seasons at a time (say 3). Using this approach large between season differences in CPTH were detected for some ports. CPTH has generally trended upward (as have landings over the same time period) but the different ports have not shown the same pattern. With the current number of logs, annual changes in CPTH of about ± 5% would not be detected; an N of about 13 is recommended to detect these changes.

Résumé

Les estimations de prises par levée de casier (PPLC) dans la pêche du homard en Nouvelle-Écosse sont fondées sur l'échantillonnage en mer, sur des entrevues, sur les journaux de pêche et sur les registres des coopératives de homard. Les deux dernières sources de renseignements représentent probablement le moyen le plus économique d'obtenir des données sur les PPLC. Ces documents sont utilisés dans de nombreux ports de pêche en Nouvelle-Écosse depuis le milieu des années 80. Le système de journaux de pêche actuel n'a pas été évalué en fonction de critères statistiques. Les ports sont habituellement représentés par un petit nombre de journaux (ordinairement < 5). Aussi s'interroge-t-on sur la valeur statistique de ces derniers. On examine ici les conséquences d'une valeur N faible et on propose une stratégie d'évaluation des tendances des PPLC au fil des saisons de pêche. Pour déterminer s'il existait des différences intersaisonnières, on a évalué les contrastes en se servant de taux d'erreur décisionnels fondés sur la méthode de Rodger (1974, 1975).

L'évaluation des tendances saisonnières des PPLC soulève deux problèmes. En premier lieu, comme les PPLC connaissent des variations importantes au sein d'une même saison, le calcul des PPLC saisonnières dépend des semaines comprises dans la saison considérée. Par conséquent, si l'on veut comparer les PPLC de différentes saisons, chaque estimation de PPLC saisonnières doit porter sur les mêmes semaines. En second lieu, lorsqu'il s'agit de comparer les PPLC de deux saisons ou plus, il importe d'utiliser les prises des mêmes pêcheurs; il y a, en effet, perte de valeur statistique si l'on ne tient pas compte de la corrélation relative aux prises de chaque pêcheur. Or, comme il y a un certain roulement parmi les pêcheurs qui tiennent volontairement des journaux de pêche, cela signifie qu'on utilisera habituellement moins de journaux qu'il n'en existe. Pour éviter de perdre éventuellement des teneurs volontaires de journaux, on propose d'analyser les tendances sur quelques saisons à la fois (disons trois). Mettant ces principes en pratique, on a déterminé qu'entre 1985 et la période actuelle il existait des différences intersaisonnières entre les PPLC. Compte tenu du petits nombre de journaux, l'influence des saisons était très marquée. En général, les PPLC semblaient à la hausse (comme les débarquements pendant la même période), mais cette tendance ne se vérifiait pas pour tous les ports. On recommande une valeur N d'environ 13 pour déterminer les différences annuelles dans une zone donnée, avec une marge d'erreur de ± 5 %,.

Introduction

The catch rate of lobsters (catch-per-trap-haul, or CPTH) in the Nova Scotia lobster fishery has been used for assessment purposes (Miller et al. 1987), overviews of particular fisheries (Campbell and Duggan 1980, Campbell 1990, Robichaud and Campbell 1991), estimates of lobster yield (Pringle and Duggan 1985), and in annual presentations to fishermen at advisory committee meetings. Lobster CPTH can provide a useful index of stock abundance, but the relationship between CPTH and true abundance is obscured by such factors as temperature, lobster size, sex, physiological state, habitat, trap design, soak time and fishing strategy (McLeese and Wilder 1958, Thomas 1973, Austin 1977, Skud 1979, Miller 1989, 1990).

Lobster landings in Lobster Fishing Areas (LFAs) in Atlantic Canada (Fig. 1) have increased substantially over the last 5-10 years (Fig. 2). In LFA 27 landings are more than double all-time highs; in LFA 34 landings are near all time highs for this century. While these increases have been attributed to better recruitment, increased effort may also have played a role. As an index of lobster abundance, CPTH provides a tool for distinguishing whether changes in landings are due to changes in lobster abundance or in fishing effort.

Estimates of CPTH in the Nova Scotia lobster fishery have been based on sea samples (Miller et al. 1987, Campbell 1990, Robichaud and Campbell 1991), interviews (Campbell and Duggan 1980, Miller et al. 1987, Pringle and Duggan 1985), fishing logs (Miller et al. 1987) and records from lobster cooperatives (Campbell 1990). Fishing logs or lobster cooperative records are probably the most cost-effective way to obtain CPTH data. Properly maintained, they provide a record of CPTH over the entire season, rather than a few days during the season as is usually the case with interviews or sea samples. Within-season changes in catch rate are substantial (Campbell 1990) and a complete season record of CPTH is preferable to point estimates.

In Atlantic Canada, fishing logs are kept on a voluntary basis. Two important decisions must be made by biologists interested in obtaining CPTH data from logs: the number and location of 'representative' sampling ports, and the number of logs to obtain in any given location. Papers that identify areas with similar landings trends (e.g. Campbell and Mohn 1983, Harding et al. 1983) provide some guidance for choosing sampling ports. Other factors also come into play---proximity of ports, whether other types of sampling is taking place there etc. As for deciding the number of logs per area, this has been based on a combination of intuition and opportunity. At present the number of records per port is 2-18. The existing log system has not been evaluated using statistical criteria, although the need for evaluating sampling design and precision was recognized in the early 1980s (Anthony and Caddy 1980, Rowell 1983). Peterman (1990) argues that analyses of statistical power in fisheries research can help interpret past results, and improve design for future experiments, impact assessments and management regulations.

The objective of this paper is to develop a strategy for evaluating CPTH trends with a voluntary logbook system after consideration of statistical power. In the process we evaluate whether there were between-season changes in lobster catch rate in Nova Scotia fishing ports during the latter half of the 1980s.

Fishing areas, seasons and fishing log data

The lobster fishing ports that concern us here are on Cape Breton Island and on the southern and southwest coasts of Nova Scotia (Fig. 1). In LFAs 33 and 34, the fishing season runs over two calendar years: from the end of November until the end of May, with many fishers not fishing at all in mid-winter. On Cape Breton Island (LFAs 27-30) the season runs from mid-May until July, with most fishers active each week during the 9-10 week season.

Since 1985 two to five fishers from one or more ports in LFAs 27, 29, 30 and 33 have kept voluntary fishing logs. The logs consist of a daily record of lobster catch (if only number is given we have converted to weight based on the mean weight of individual lobsters in the area), effort (number of traps set), and related information (weather, sometimes number of sublegals or ovigerous females). In addition to these logs the Port Maitland (LFA 34) lobster cooperative has provided the records of 15-20 fishers for 4 years. It was not possible to standardize CPTH for soak time because insufficient information was provided in the fishing logs. For the spring-summer seasons of Cape Breton, there is little variability in soak time among fishermen since most traps are hauled every day except for Sundays and during bad weather. For the winter-spring seasons of LFAs 33 and 34, soak periods are longer during winter.

Individual fishing logs were for the same fishers year after year, but there was some turnover of fishers within the lobster cooperative data. To eliminate analytical difficulties arising from including records from different fishers in different years, the Port Maitland data set was trimmed to include only those fishers who kept logs for each fishing season under consideration. Weekly averages of daily CPTH (Fig. 3) were computed for each fisher in each port after the log data were entered into a database (Hunter and Tremblay 1992). A strong within-season trend is present within each port, but is most pronounced in Port Maitland and Stonehurst.

Statistical procedures

We begin the analysis by looking at the CPTH at Port Maitland. The season figure (the average of weekly CPTH) is taken for each of $N = 8$ fishers for each of the $J = 3$ seasons 1988-89, 1989-90 and 1990-91. Because of weather, fishing by the 8 fishers covered 25, 27 and 27 weeks in each of those years, respectively, but the data analysed are taken from only weeks 2 - 11 and 16 - 23: those weeks were selected to maximize the number of logs kept consistently over the seasons. The data were subjected to a randomised-blocks analysis of variance, which yielded Table 1:

Table 1: Randomised-blocks analysis of variance, for Port Maitland, 1988-89 to 1990/91

Source	df	SS	MS	F	r
Logs	7	0.05432			
Seasons	2	0.04543	0.02272	7.907	2
Residual	14	0.04022	0.00287		
Total	23	0.13998			

The mean CPTH for each season are shown in Table 2:

Table 2: Mean CPTH, Port Maitland, 1988/91

Seasons	1988-89	1989-90	1990-91
Mean	0.548	0.634	0.645

To test for differences between the Seasons the method of Rodger (1974, 1975a,b) will be used. Although this procedure for evaluating contrasts with decision-based error-rates has been used extensively in the social sciences and animal behaviour literature (e.g. Goodkin 1976, Urcuioli and Honig 1980, Kruse and LoLordo 1986, Jacobs and Blackburn 1988), workers in ecology and fisheries science are less familiar with it (e.g. it is not mentioned in Day and Quinn 1989). The Rodger (1974) approach sets the error-rates for the things in which errors occur (the decisions), rather than experimentwise for all possible comparisons. As a consequence there is no serious loss of power as the numerator degrees of freedom for F increases; as happens with the more traditional criterion.

The method first computes the number of mutually orthogonal contrasts rejectable (r) in the data by Rodger's (1975a) new criterion $F[E\alpha]; \nu_1, \nu_2 = F[.05]\ 2, 14 = 2.939$. That is:

$$r = [F_m / F[E\alpha]; \nu_1, \nu_2] = [7.907 / 2.939] = [2.7] = 2 \qquad [1]$$

where F_m is the observed F-ratio (from Table 1). Only the whole number part of the ratio is used, and that is reduced to ν_1 if it is larger. That is because it is mathematically impossible to have more than ν_1 mutually orthogonal contrasts in a space of ν_1 dimensions.

A null contrast across J means takes the form:

$$c_1 \mu_1 + c_2 \mu_2 + \ldots + c_J \mu_J = 0 \qquad [2]$$

The c_j are the contrast coefficients, selected by the investigator to reveal the pattern of differences among the means, and they must be chosen to sum to 0 for any given contrast. Rodger's method also requires that any set of null contrasts selected for decisions must be be linearly independent of one another, to prevent contradiction among the decisions. Linear independence is guaranteed if the contrasts are mutually orthogonal. Two null contrasts of the form:

$$H_1: c_{11} \mu_1 + c_{12} \mu_2 + \ldots + c_{1J} \mu_J = 0 \qquad [3]$$
$$H_2: c_{21} \mu_1 + c_{22} \mu_2 + \ldots + c_{2J} \mu_J = 0 \qquad [4]$$

are mutually orthogonal if the sum of the products of the respective c_{hj}'s are 0.

The rule for <u>post-hoc</u> decision making is that the hth null contrast <u>may</u> be rejected if its sample value satisfies:

$$F_h = \frac{N\,(\Sigma\, c_j\, m_j)^2}{\nu_1\ \mathrm{msr}\ \Sigma\, c_j^2} \geq F[E\alpha]; \nu_1, \nu_2 \qquad [5]$$

Of course, the contrast considered might not be rejected, some other(s) being more revealing in the judgement of the investigator.

In the Port Maitland data the following pair of orthogonal contrasts tell us what has been happening over the three seasons 1988-89 to 1990-91:

$$\mu_3 - \mu_2 = 0 \quad F_1 = \frac{8(0.645 - 0.634)^2}{2 \times 0.00287 \times 2} = 0.084 < F[.05];\ 2, 14 = 2.939 \qquad [6]$$

and

$$\mu_2 + \mu_3 - 2\,\mu_1 > 0 \quad F_2 = \frac{8(0.183)^2}{2 \times 0.00287 \times 6} = 7.779 \ > 2.939 \qquad [7]$$

These say that the average CPTH for 1989-90 and 1990-91 were the same, but was larger than the average for the 1988-89 season. Since these two contrasts are orthogonal to one another, their F values sum to the value of F_m (apart from rounding), but it is linear independence, not orthogonality that is essential. One must never reject more nulls than the r given in [1], but one may reject fewer. The fact that we have rejected only one of r = 2 null contrasts possible by rule [1] means that our decisions are conservative so far as type-1 error-rate is concerned.

The above analysis used weeks 2 to 11 plus weeks 16 to 23 in each season for the N = 8 fishers. If weeks 1 to 11 plus weeks 18 to 23 were used, there would be only N = 6 fishers who had completed logs in that period and their results would be different m_j (0.693, 0.843, 0.804) and msr (0.00473), but the pattern $\mu_1 < \mu_2 = \mu_3$ still fits these data well. There were N = 7 fishers who had completed logs for weeks 2 to 11 plus weeks 16 to 25 and, again their results are different (m_j = 0.569, 0.654, 0.647 and msr = 0.00222), but the pattern $\mu_1 < \mu_2 = \mu_3$ also fits these data well.

Only N = 3 fishers from Stonehurst provided logs for the seasons 1985/86 through 1989/90. Using the first 4 weeks of each of the first three seasons gives the value F_m = 2.028, and that does not reach its criterion F[.05]; 2, 4 = 5.309. The means over the first four weeks of each of the three seasons were 0.570, 0.524, and .474. Of course, the first four weeks of any season may not yield a very valid estimate of the CPTH for the season as a whole. There were N = 2 fishers at Stonehurst who provided logs for weeks 1 to 4 and 20 to 26. Their means were 0.344, 0.296, and 0.274, and these yield F_m = 4.021, which does not reach its criterion F[.05]; 2, 2 = 14.17. This raises the question of power for such pathetically small samples, and to that we now turn.

Sample size and power

Peterman (1990) showed that, of 408 papers in fisheries and aquatic sciences surveyed, 160 reported at least one non-rejection of a null hypothesis yet only 3 of these 160 papers reported power or type-2 error-rate calculations. He discussed the implications of ignoring power and argued that analyses of statistical power in fisheries research can help interpret past results, as well as improve design for future experiments, impact assessments and management regulations.

It needs to be noted that classical work on power in fixed-effects analyses of variance (originating from Fisher's 1928 paper) requires that alternatives to null hypotheses be expressed in the units of the unknown variance σ^2. Thus if

$$H_0\!: \mu_1 = \mu_2 = \ldots = \mu_J \qquad [8]$$

or

$$\Delta_m = N\,\Sigma(\mu_j - \mu.)^2 = 0 \qquad [9]$$

is false, then

$$\Delta_m = N\,\Sigma(\mu_j - \mu.)^2 / \sigma^2 = k \qquad [10]$$

is true for some $k > 0$. In these equations N is the common sample size, $\mu.$ is the mean of the J values of the true means μ_j and Δ_m is the quadratic non-central parameter. Some writers use the symbol λ rather than Δ_m and some prefer

$$\phi = \sqrt{\Delta_m / (\nu_1 + 1)} \qquad [11]$$

in which ν_1 is the numerator degrees of freedom of the appropriate F-ratio. Random-effects applications do not use Δ_m.

Suppose we had J = 5 and we were interested in detecting the pattern

$$\mu_j - \mu. = -.4\ \sigma,\ .2\ \sigma,\ -.1\ \sigma,\ .6\ \sigma,\ -.3\ \sigma \qquad [12]$$

This pattern yields $\Sigma(\mu_j - \mu.)^2 = 0.66\sigma^2$. There are many other patterns which have the same $\Sigma(\mu_j - \mu.)^2$ and are, therefore, equally detectable. For example

$$\mu_j - \mu. = -.4062\ \sigma,\ -.4062\ \sigma,\ 0,\ .4062\ \sigma,\ .4062\ \sigma \qquad [13]$$

$$\mu_j - \mu. = -.1817\ \sigma,\ -.1817\ \sigma,\ -.1817\ \sigma,\ -.1817\ \sigma,\ .7268\ \sigma \qquad [14]$$

and

$$\mu_j - \mu. = -.5745\ \sigma,\ 0,\ 0,\ 0,\ .5745\ \sigma \qquad [15]$$

It might be simpler to choose a value for k in equation [10], though that seems rather divorced from the size of effect one would like to state as worthy of detection with good power.

It was Dantzig (1940) who showed that it is impossible to avoid the unknown σ in power calculations for the usual t-tests, and Stein (1945) extended that proof to the dependence of power calculations on the unknown σ in the traditional analysis of variance. In that same paper Stein (1945) showed how it is possible to avoid the use of σ in power calculations both for t-tests and for analyses of variance, by using a two-stage sampling procedure. In the case of anova a special form of non-central F-distribution applies, and special tables of its non-central parameter are therefore necessary before the two-stage method can be used. Such tables were not published until Rodger (1976) produced them (using the concept of experiment-wise error-rate) and another set in Rodger (1978) using his concept of decision-based error-rate.

Detection rate for false null contrasts

Rodger (1974) has argued that alternatives to null contrasts are best expressed in the form:

$$c_1\,\mu_1 + c_2\,\mu_2 + \ldots + c_J\,\mu_J = \delta = g\,\sigma\sqrt{\Sigma c_j^2} \qquad [16]$$

Here δ is the linear non-central parameter, but it is g that is at choice: the two terms to the right of that are mere scale factors (one for the scale of measurements used, the other for the scale on which the contrast is stated). Thus anova will not be able to detect a difference of 1000 g per trap any better than it can detect a difference of 1 kg per trap (σ will be a thousand times larger when measurements are in g rather than kg). Also both forms

$$\frac{\mu_1 + \mu_2}{2} - \mu_3 = .8\,\sigma\sqrt{1.5} \qquad [17]$$

$$\mu_1 + \mu_2 - 2\,\mu_3 = .8\,\sigma\sqrt{6} \qquad [18]$$

are equally-detectable alternatives to the null since their scales are absorbed by the radical term.

The quadratic form of the non-central parameter for the hth contrast is

$$\Delta_h = N\,\delta^2 / (\sigma^2\,\Sigma c_j^2) = N\,g^2 \qquad [19]$$

and (J - 1) mutually orthogonal contrasts make the overall non-central parameter

$$\Delta_m = \sum_{h=1}^{J-1} \Delta_h \qquad [20]$$

Clearly one might estimate g from previous research results (assuming they are adequately reported) by

$$\hat{g} = \Sigma c_j\, m_j \,/ \sqrt{s^2\,\Sigma c_j^2} \qquad [21]$$

where the m_j and s^2 are the sample means and variance. But Perlman and Rasmussen (1975) give more sophisticated estimators of Δ_m in anova as

$$\hat{\Delta}_m = (\nu_2 - 2)\ \mathrm{SSB/SSW} - \nu_1 \qquad [22]$$

or the more precise estimator

$$\Delta_m^* = (\nu_2 - 4)\ \mathrm{SSB/SSW} - \nu_1\,(\nu_2 - 4)/(\nu_2 - 2) \qquad [23]$$

in which SSB and SSW are the numerator and denominator sums of squares for the F-ratio, based on ν_1 and ν_2 degrees of freedom.

Estimating g or Δ_m is useful, but we can always state how big an effect we believe it is reasonable to seek even though past research may provide little guidance to what might truly exist. Declaring the g being sought, then computing the sample size necessary to detect it (with good probability) is the even-handed way to design experiments. If a g stated is too large, making the sample-size too small to yield a respectable detection rate, future research can take that into account.

Rodger (1974) tells us that $g^2 = 1$ is a moderately large effect that can be detected with decent probability (say, .95) by small samples (say, $N \approx 13$), and he advocates starting with that value in the absence of better information. If $g^2 = 1$ had been chosen for the Port Maitland design, then with N = 8, the expected rate of null contrast rejection with $E\alpha = .05$ would be $E\beta = .84$ (see the tables in Rodger, 1975b).

Of course, the residual mean-square (msr) in Table 1 is not an estimate of σ^2 but of $\sigma^2\,(1 - \rho)$, where ρ is the intra-class correlation of CPTH across seasons. Reference to σ^2 in equations [10] through [19] above should be replaced by $\sigma^2\,(1 - \rho)$ when randomised-blocks is the form of analysis.

If we had intended to make $E\beta = .95$ for $g^2 = 1$ in the Port Maitland investigation, we would have required:

$$N \geq \Delta[E\beta];\ \nu_1, \nu_2 / g^2 \qquad [24]$$

Since we cannot know ν_2 until we know N, because $\nu_2 = J(N-1)$, we begin with $\nu_2 = \infty$. From the tables of Rodger (1975b), we obtain:

$$N \geq \Delta[.95];\ 2, \infty / 1 = 9.257 = 10 \qquad [25]$$

which makes $\nu_2 = J\ (N - 1) = 3 \times 9 = 27$ and $\Delta[.95]; 2, 27 \approx 10.490$; so the investigation should have been designed with N = 11. Using N = 11 would mean that we were seeking to detect $\Delta_m = \nu_1 \times N \times g^2 = 2 \times 11 \times 1 = 22$ (see eqn. [20] above) at a rate better than $E\beta = .95$.

Equation [22] and Table 1 give the estimate *of what is actually present* as:

$$\hat{\Delta}_m = 12 \times 0.04543 / 0.04022 - 2 = 11.554 \qquad [26]$$

and equation [23] gives:

$$\Delta^*_m = 10 \times 0.04543 / 0.04022 - 2 \times 10 / 12 = 9.629 \qquad [27]$$

If the former non-central parameter were true, then our sample size would have made $E\beta = .71$, and if the latter were true $E\beta = .63$.

Both SPSS and Systat will compute the POWER 'relevant' to analysis, if asked. The former uses the word as a subcommand in MANOVA and it uses:

$$\Delta_m = \nu_1\ F_m \qquad [28]$$

which is not too strange because the non-central parameter at [10] above is clearly related to:

$$F_m = N\ \Sigma(m_j - m.)^2 / (\nu_1\ s^2) \qquad [29]$$

But the whole idea of estimating Δ_m from the sample data, then using that to calculate the power one's analysis had, is logically unsound. To ask what power an analysis would have for some independently chosen value of k in [10] or g in [16] makes sense, but to claim that one's power was low because SPSS or SYSTAT put a small observed F_m in [28] is not plausible: it <u>might</u> make more sense to say that the small F_m indicates H_0 at [8] is true.

It should be noted that, if the correlation of fishers' annual CPTH were ignored in Table 1, the Error SS would become 0.05432 + 0.04022 = 0.09454 with 7 + 14 = 21 d.f.; hence MSE would be $s^2 = 0.00450$ and $F_m = 5.049$. Such a reduction in F_m is typical in a repeated-measures situation because the intra-class correlation is usually positive (in our case .36). Ignoring intra-class correlation will, therefore, usually result in a loss of power.

A strategy for evaluating CPTH trends

Since there is not a large number of fishers who maintain logs on a regular basis, we are committed to using the same people over a number of seasons. It is also clear that, as the number of seasons is increased, the number of logs from the same people over all those seasons will diminish. The wisest plan, therefore, may be to analyse trends across seasons a few at a time (say, 3).

Thus at Port Maitland the means for N = 10 fishers for the 1989/90 through 1991/92 seasons were 0.657, 0.667, and 0.607, which yield $F_m = 1.389$ (less than the $F[.05]; 2, 18 = 2.757$ required).

The above analysis used weeks 2-7, 9, 10, 14, 15 and 18-23, but a similar result is found for the N = 9 fishers using weeks 2-7, 9, 10, 14, 15 and 18-25 (F_m = 2.269) and the N = 7 fishers using weeks 1-7, 9, 10, 14, 15 and 18-23 (F_m = 0.467). It seems reasonable to conclude from these and the previous data that $\mu_1 < \mu_2 = \mu_3 = \mu_4$.

Some analysis of Stonehurst data is shown in Table 3. Seasons 1-3 were reported above and are repeated here for comparison. The only triple of seasons that shows a 'significant' result is 2-4 in the third data column. That effect is due to m_4 because $m_2 + m_3 - 2\, m_4$ yields F = 35.004. Season 4 does not produce any 'significant' effect in data columns 4 to 7, though m_4 is always the lowest of the mean triples. Such a paucity of 'significance' is almost certainly due to low power. That is confirmed when all five available seasons are analysed (see the last two data columns of Table 3). Those data tell us that $\mu_1 = \mu_2$ (F = 0.215 and 0.738), $\mu_1 = \mu_2 = \mu_3$ (F = 1.349 and 0.904), $\mu_4 = \mu_5$ (F = 1.051 and 0.104), and $\mu_1 = \mu_2 = \mu_3 > \mu_4 = \mu_5$ (F = 16.768 and 10.832).

Ordinarily one would not want to analyse as many as five seasons together because the number of logs available would likely diminish as J increased. That does not happen with the Stonehurst data, though the number of logs available is the minimum analysable. Also as J increases one would expect that msr would increase, mainly because the correlation between results in seasons widely separated in time would be low, i.e closer to zero. But the majority of the intra-class correlations (r_{ic}) in the Stonehurst data are negative, making the randomised blocks form of analysis not very efficient; so a tendency towards zero would be welcome! Curiously msr for seasons 1-5 (0.00078) *is* not generally larger than its 3-season competitors (0.00063, 0.00107 and 0.00131). The relevant data are the s^2, i.e. the cell variance, and $s^2(1-r)$, i.e. the residual mean-square msr. These are shown in Table 3.

Table 3: Selections of Stonehurst average CPTH by season (season 1 = 1985/86, season 5 = 1989/90)

Season	1-3	1-3	2-4	2-4	3-5	3-5	3-5	1-5	1-5
N	3	2	3	2	3	2	2	3	2
Weeks		1-4,		1-4,	1-3,16-18,	1-4,15-18,	1-4,		1-4
Weeks	1-4	20-26	1-3	20-26	20-26	20-26	20-26	1-3	20-26
m_1	.570	.344						.672	.344
m_2	.524	.296	.624	.296				.624	.296
m_3	.474	.274	.544	.274	.223	.231	.274	.544	.274
m_4			.287	.177	.159	.147	.177	.287	.177
m_5					.189	.164	.195	.393	.195
$100s^2$	.412	.049	.271	.072	.050	.055	.103	.339	.065
$100s^2(1-r_{ic})$	.341	.063	.252	.107	.072	.070	.131	.401	.078
r_{ic}	0.172	-.305	0.072	-.488	-.433	-.268	-.275	-.184	-.202

F_m	2.018	4.021	36.777	7.604	4.307	5.658	4.138	19.338	12.725
ν_1, ν_2	2, 4	2, 2	2, 4	2, 2	2, 4	2, 2	2, 2	4, 8	4, 4
F[.05]	5.309	14.17	5.309	14.17	5.309	14.17	14.17	2.264	3.537

It should be noted that different selections of weeks give quite different mean CPTH. That is because CPTH is high early in the season then drops and often recovers somewhat at the end of each season. The discrepancies are considerable, e.g. 0.570 to 0.344; so mean CPTH from early-season data cannot be validly extrapolated to CPTH throughout a season - the predictive potential of such statistics needs further investigation. Also, if a series of season triples is to be analysed, as suggested above, then the same weeks should be used in each season and in each triple: it would be better if all the weeks in every season could be used.

Another important feature of the data is that we would expect the cell variances (s^2) to decrease as the number of weeks added into the averages analysed increases - remembering that the variance of an average of K *independent* quantities is σ^2/K; so, though our items are not independent, some decrease should occur unless the intra-class correlation was 1.0. In fact s^2 and the number of weeks averaged are closely related: their correlation is -.9192 and msr correlates -.9197 with the number of weeks averaged.

So far we have shown the following season differences for CPTH at Port Maitland: 88/89 < 89/90 = 90/91 = 91/92. At Stonehurst the relationships seem to have been 85/86 = 86/87 = 87/88 > 88/89 = 89/90. It would be wise, therefore, to look a little further into the possibility of an increase in CPTH at Stonehurst in 89/90.

The sample means show an increase from 88/89 to 89/90 (.159 to .189, .147 to .164, .177 to .195 and .287 to .393) but none of these reaches their criterion, or even approaches it closely. What 'power' did our design have to detect a reasonably-sized effect? Let us suppose that a reasonably-sized effect is $\mu_5 - \mu_4 = \sqrt{2[\sigma^2 (1 - \rho)]}$, i.e. g = 1. It is easy to compute the probability of detecting such an effect *by a planned t-test* because when Δ β; 1, 4 = N g^2 = N = 3, then $\beta = .27$. But hypotheses such as $\mu_5 - \mu_4 = 0$ were not 'planned' (or they were among a large set which would all be evaluated). Addressing the 'power' for detecting the falsity of some specific null contrast by *post-hoc* methods is not so straightforward because the contrasts thus selected are chosen in the light of the sample results. We can, however, ask for the probability of $F_m \geq F[E\alpha]$; ν_1, ν_2 if the total variation among all the μ_j comes from $\mu_5 - \mu_4 = \sqrt{2[\sigma^2 (1 - \rho)]}$. That makes $\Delta_m = Ng^2 = N = 3$, then $\beta = .24$ when $\nu_1 = (J - 1) = 2$, $\nu_2 = (J - 1)(N - 1) = 4$; and $\beta = .26$ when $\nu_1 = 4$, $\nu_2 = 4$. Note that, with Rodger's method, there is very little loss of 'power' compared to the t-test. By comparison, Scheffé's (1953) method would have $\beta = .18$ or .12 when $\nu_1 = 2$ or 4.

The above analysis of 'power' does indicate that the acceptance of $\mu_4 = \mu_5$ (88/89 = 89/90) at Stonehurst is dubious. Of course, the same argument can be applied to $\mu_1 = \mu_2 = \mu_3$. There is, however, little doubt about the drop in CPTH at Stonehurst from 1987/88 to 1988/89.

The Cape Breton ports

The analysis of the data from the ports in Cape Breton (Petit de Grat, Fourchu, Cape Breton and Ingonish) is not subject to the same problems as that at Stonehurst or Port Maitland. In the first place, although the samples are again very small (N = 3, 2, 2 and 2), the data come from all 9 weeks of each fishing season. The observations are summarised in Table 4.

Table 4: Mean CPTH for Cape Breton ports for years 1986-91.

Year	P. de Grat	Fourchu	Glace Bay	Ingonish
m_1	0.192	0.502	0.545	0.543
m_2	0.197	0.535	0.572	0.519
m_3	0.215	0.507	0.683	0.484
m_4	0.312	0.754	0.680	0.534
m_5	0.260	0.848	0.763	0.668
m_6	0.267	0.849		0.564
msr*1-3	0.108	1.753	0.154	0.106
r_{ic} 1-3	0.872	-.109	0.693	0.957
F_m	4.027	0.353	69.324	16.663
msr* 2-4	0.210	1.740	1.356	0.025
r_{ic} 2-4	0.457	-.110	0.407	0.988
F_m	55.287	21.050	5.894	52.750
msr* 3-5	0.389	1.680	1.379	0.065
r_{ic} 3-5	-.412	-.217	0.396	0.977
F_m	18.314	36.910	3.216	276.760
msr* 4-6	0.226	1.863		0.084
r_{ic} 4-6	0.356	0.606		0.976
F_m	10.461	3.178		117.184
v_1, v_2	2, 4	2, 2	2, 2	2,2
F[.05]	5.309	14.17	14.17	14.17

* Note that in Table 4 the msr values have been multiplied by 1000.

Analysis of the Petit de Grat data is shown in Table 5.

Table 5: Decisions for Petit de Grat data, 1986-91

Years	Decision	F	Deduction
1986-88	$\mu_1 = \mu_2 = \mu_3$	4.027	$\mu_1 = \mu_2 = \mu_3$
1987-89	$\mu_2 - \mu_3 = 0$	1.157	
	$2\mu_4 - \mu_2 - \mu_3 > 0$	53.505	$\mu_1 = \mu_2 = \mu_3 < \mu_4$
1988-90	$\mu_4 - \mu_3 > 0$	18.141	
	$\mu_3 + \mu_4 - 2\mu_5 = 0$	0.013	$\mu_1 = \mu_2 = \mu_3 < \mu_5 < \mu_4$
1989-91	$\mu_6 - \mu_5 = 0$	0.163	
	$2\mu_4 - \mu_5 - \mu_6 > 0$	10.408	$\mu_1 = \mu_2 = \mu_3 < \mu_5 = \mu_6 < \mu_4$

The choice of contrasts needs to be made with care if we are to avoid confusion, and the outcome should match the sample data well. A similar analysis is shown for Fourchu in Table 6.

Table 6: Decisions for Fourchu data, 1986-91

Years	Decision	F	Deduction
1986-88	$\mu_1 = \mu_2 = \mu_3$	0.353	$\mu_1 = \mu_2 = \mu_3$
1987-89	$\mu_2 - \mu_3 = 0$	0.225	
	$2\mu_4 - \mu_2 - \mu_3 > 0$	20.800	$\mu_1 = \mu_2 = \mu_3 < \mu_4$
1988-90	$\mu_5 - \mu_4 = 0$	2.630	
	$\mu_4 + \mu_5 - 2\mu_3 > 0$	34.300	$\mu_1 = \mu_2 = \mu_3 < \mu_4 = \mu_5$
1989-91	$\mu_4 = \mu_5 = \mu_6$	3.178	$\mu_1 = \mu_2 = \mu_3 < \mu_4 = \mu_5 = \mu_6$

Some confusion is unavoidable in the data from the port of Glace Bay itself because there is an inconsistency between the first two sets of data: the analysis is in Table 7.

Table 7: Decisions for Glace Bay data, 1986-90

Years	Decision	F	Deduction
1986-88	$\mu_1 - \mu_2 = 0$	2.367	
	$2\mu_3 - \mu_1 - \mu_2 > 0$	67.101	$\mu_1 = \mu_2 < \mu_3$?
1987-89	$\mu_2 = \mu_3 = \mu_4$	5.894	$\mu_1 = \mu_2 = \mu_3 = \mu_4$?
1988-90	$\mu_3 = \mu_4 = \mu_5$	3.216	$\mu_1 = \mu_2 = \mu_3 = \mu_4 = \mu_5$?

The contradiction among the first three decisions arises from the small msr 1-3 = .000154. The traditional conservative approach might attribute the confusion to a type-1 error in the second decision and conclude from the data that $\mu_1 = \mu_2 = \mu_3 = \mu_4 = \mu_5$ at Glace Bay.

Table 8 gives the Ingonish analysis, and a new problem.

Table 8: Decisions for Ingonish data, 1986-91

Years	Decision	F	Deduction
1986-88	$\mu_1 - \mu_3 > 0$	16.420	
	$\mu_1 + \mu_3 - 2\mu_2 = 0$	0.190	$\mu_3 < \mu_2 < \mu_1$
1987-89	$\mu_4 - \mu_3 > 0$	50.000	
	$\mu_3 + \mu_4 - 2\mu_2 = 0$	2.667	$\mu_3 < \mu_2 < \mu_4$?μ_1?
1988-90	$\mu_4 - \mu_3 > 0$	19.231	
	$\mu_5 - \mu_4 > 0$	138.123	$\mu_3 < \mu_2 < \mu_4 < \mu_5$?μ_1?
1989-91	$\mu_6 - \mu_4 = 0$	5.357	
	$\mu_5 - \mu_6 > 0$	64.381	$\mu_3 < \mu_2 < \mu_4 = \mu_6 < \mu_5$?μ_1?

The *logic* of equality and order leaves the position of μ_1 unclear. The logic of our decisions says that $\mu_3 < \mu_2 < \mu_1$ and $\mu_3 < \mu_2 < \mu_4$ but that logic cannot place μ_1 relative to μ_4 (the sample data suggest that $\mu_1 = \mu_4$, but we cannot compare the two parameters because they belong in different triples). We could extend our triples to quadruples (if that did not lose fishers and it did not produce too dramatic an increase in msr) or we could insert specific values for our inequalities and work out the consequence of those. Thus we might claim that

$\mu_1 - \mu_3 = \delta$ [30] $\mu_1 + \mu_3 - 2\mu_2 = 0$ [31]

$\mu_4 - \mu_3 = \delta$ [32] $\mu_3 + \mu_4 - 2\mu_2 = 0$ [33]

But the last of these [33] is redundant because [30] and [32] say that the distance from μ_1 to μ_3 is the same as that from μ_4 to μ_3; so μ_1 must equal μ_4: hence if μ_2 sits midway between μ_1 and μ_3 (as in [31]), then μ_2 must sit midway between μ_4 and μ_3 (as in [33]). There is a matrix equation which captures the relations among the μ_j. It is

$$ {}_1\mu_J = {}_1\delta_H \, ({}_H\mathbf{C}_J \, {}_J\mathbf{C}^T_H)^{-1} \, {}_H\mathbf{C}_J \qquad [34]$$

where the matrix ${}_H\mathbf{C}_J$ holds the contrast coefficients for H linearly independent contrasts and ${}_1\delta_H$ holds the values of those contrasts. Using [30], [31] and [32] gives

$$ {}_1\mu_4 = [\delta \;\; 0 \;\; \delta] \left(\begin{vmatrix} 1 & 0 & -1 & 0 \\ 1 & -2 & 1 & 0 \\ 0 & 0 & -1 & 1 \end{vmatrix} \begin{vmatrix} 1 & 1 & 0 \\ 0 & -2 & 0 \\ -1 & 1 & -1 \\ 0 & 0 & 1 \end{vmatrix} \right)^{-1} {}_3\mathbf{C}_4 \qquad [35]$$

$$ = [\delta \;\; 0 \;\; \delta] \begin{vmatrix} 2 & 0 & 1 \\ 0 & 6 & -1 \\ 1 & -1 & 2 \end{vmatrix}^{-1} {}_3\mathbf{C}_4 = \frac{1}{16} [\delta \;\; 0 \;\; \delta] \begin{vmatrix} 11 & -1 & -6 \\ -1 & 3 & 2 \\ -6 & 2 & 12 \end{vmatrix} {}_3\mathbf{C}_4 $$

$$ = \frac{1}{16} [5\delta \;\; \delta \;\; 6\delta] \begin{vmatrix} 1 & 0 & -1 & 0 \\ 1 & -2 & 1 & 0 \\ 0 & 0 & -1 & 1 \end{vmatrix} = \frac{1}{16} [6\delta \;\; -2\delta \;\; -10\delta \;\; 6\delta] $$

The row vector ${}_1\mu_4$ holds the values of $\mu_1 - \mu.$, $\mu_2 - \mu.$, $\mu_3 - \mu.$ and $\mu_4 - \mu.$ with $\mu.$ being the average of the μ_1 to μ_4. These values demonstrate that $\mu_1 = \mu_4$, and μ_2 ($-2\delta/16$) sits midway between the small μ_3 ($-10\delta/16$) and the large μ_1 or μ_4 ($6\delta/16$).

The patterns of changes in CPTH at the ports in Cape Breton, based on the above decisions, are depicted in Table 9. The subscripts are for the years that have been analysed.

Table 9: Patterns of changes in CPTH at Cape Breton ports, 1986 to 1991.

Petit de Grat	$\mu_{86} = \mu_{87} = \mu_{88} < \mu_{90} = \mu_{91} < \mu_{89}$
Fourchu	$\mu_{86} = \mu_{87} = \mu_{88} < \mu_{89} = \mu_{90} = \mu_{91}$
Glace Bay	$\mu_{86} = \mu_{87} = \mu_{88} = \mu_{89} = \mu_{90}$
Ingonish	$\mu_{88} < \mu_{87} < \mu_{86} = \mu_{89} = \mu_{91} < \mu_{90}$

CPTH generally increased over the six years, though there is no common pattern. Combining results from all four ports (to make N = 9) would obscure the separate patterns. Also it can be seen from Table 4 that msr varies widely from the low of .000025 for Ingonish means 1987 to 1989, to the high of .001863 for Fourchu means 1989 to 1991. The sampling distribution of variances *does* have a long tail, but combining data from the Cape Breton ports would swamp some trends and reveal others (which one might doubt).

Although we have shown that there are clear differences in CPTH for some seasons, and that there has been a general trend upwards, there could remain quite large undetected differences. Suppose a typical CPTH of 0.60 kg (which is a middle value of the m_j at Fourchu, Glace Bay, and Ingonish) and suppose a three-year trend of ± 5% (i.e. 0.57 kg, 0.60 kg, 0.63 kg) with $\sigma^2(1-\rho)$ = .001 kg^2 (which is a mid-value for msr at Fourchu, Glace Bay, and Ingonish), then

$$\Delta_m/N = \Sigma(\mu_j - \mu.)^2 / \sigma^2(1 - \rho) = (.03^2 + .03^2)/ .001 = 1.8 \qquad [36]$$

(compare this with equation [10]). To make $E\beta = .95$ we would need N = 12, hence $\Delta_m = 21.6$, because $\Delta[.95]; 2, 22 = 10.735 \approx \Delta_m/2$. This is the same advice as that given by Rodger (1974), i.e. when in doubt use $g^2 = 1$, $E\alpha = .05$, $E\beta = .95$, and that will usually require $N \approx 13$.

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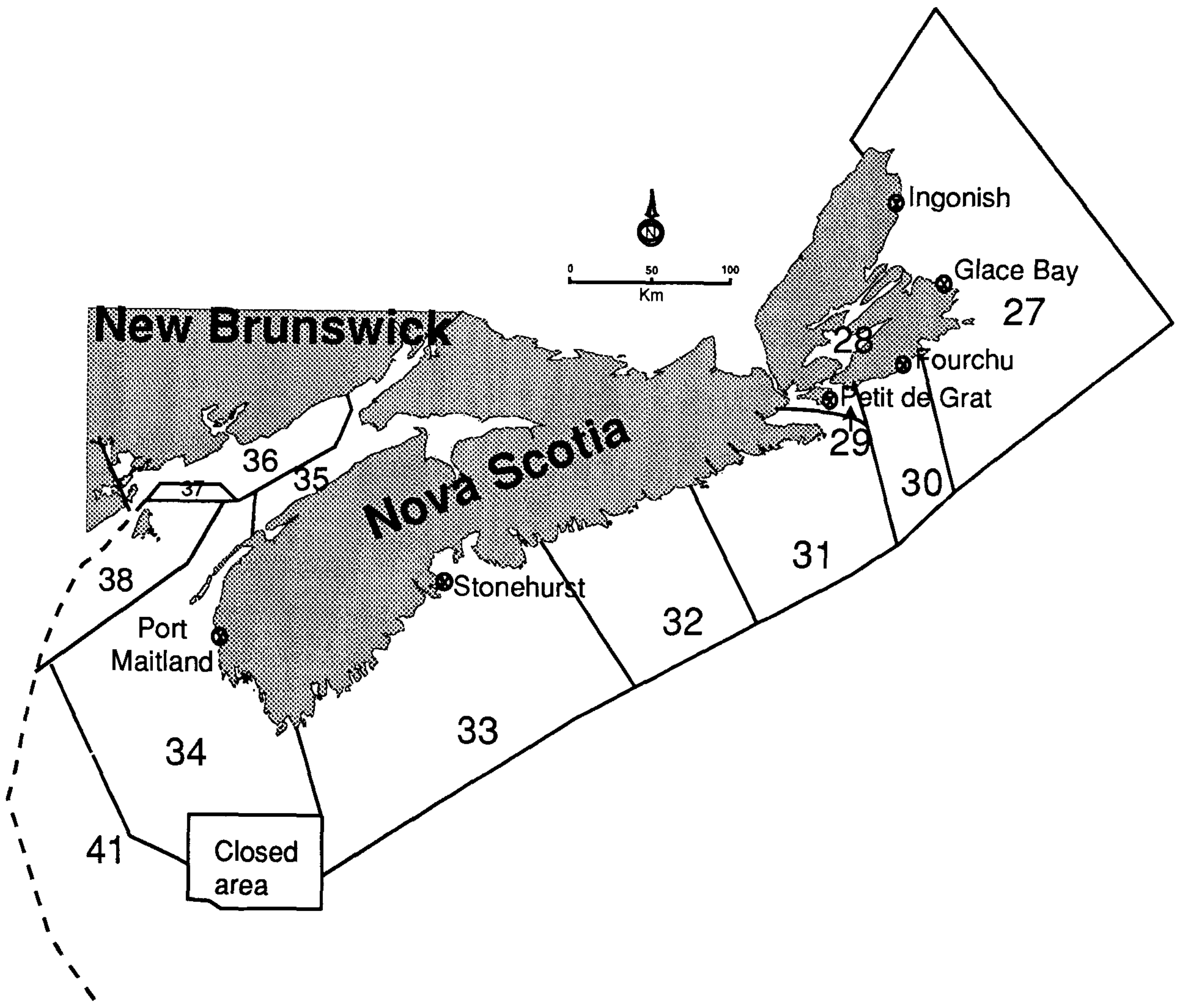

Figure 1. Lobster fishing areas (LFAs) within the Scotia-Fundy region. Ports shown are those for which catch rate data from voluntary logbooks were obtained.

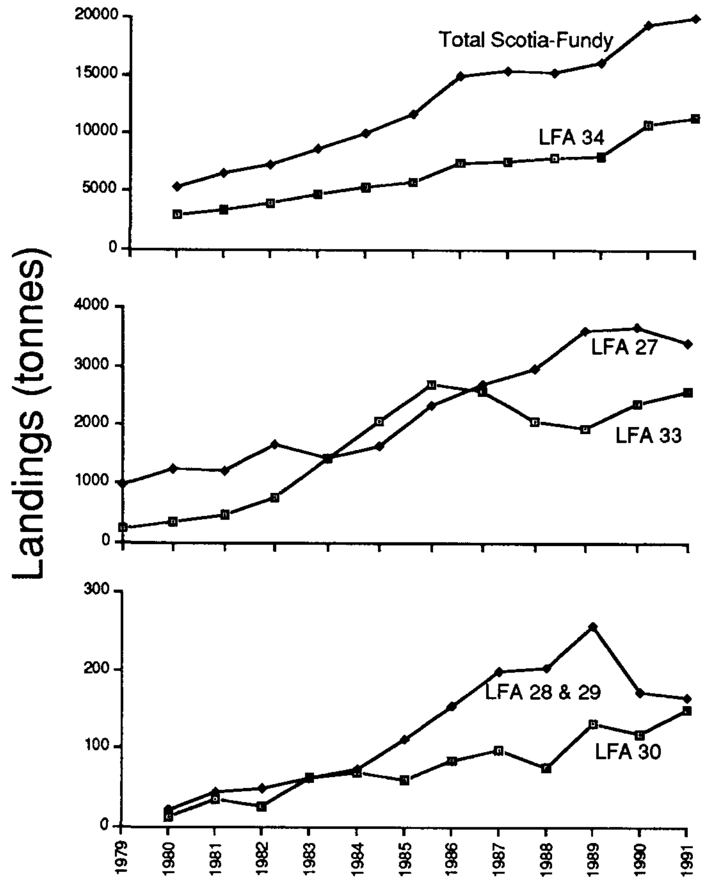

Figure 2. Recent trends in lobster landings in some Scotia-Fundy lobster fishing areas (LFAs).

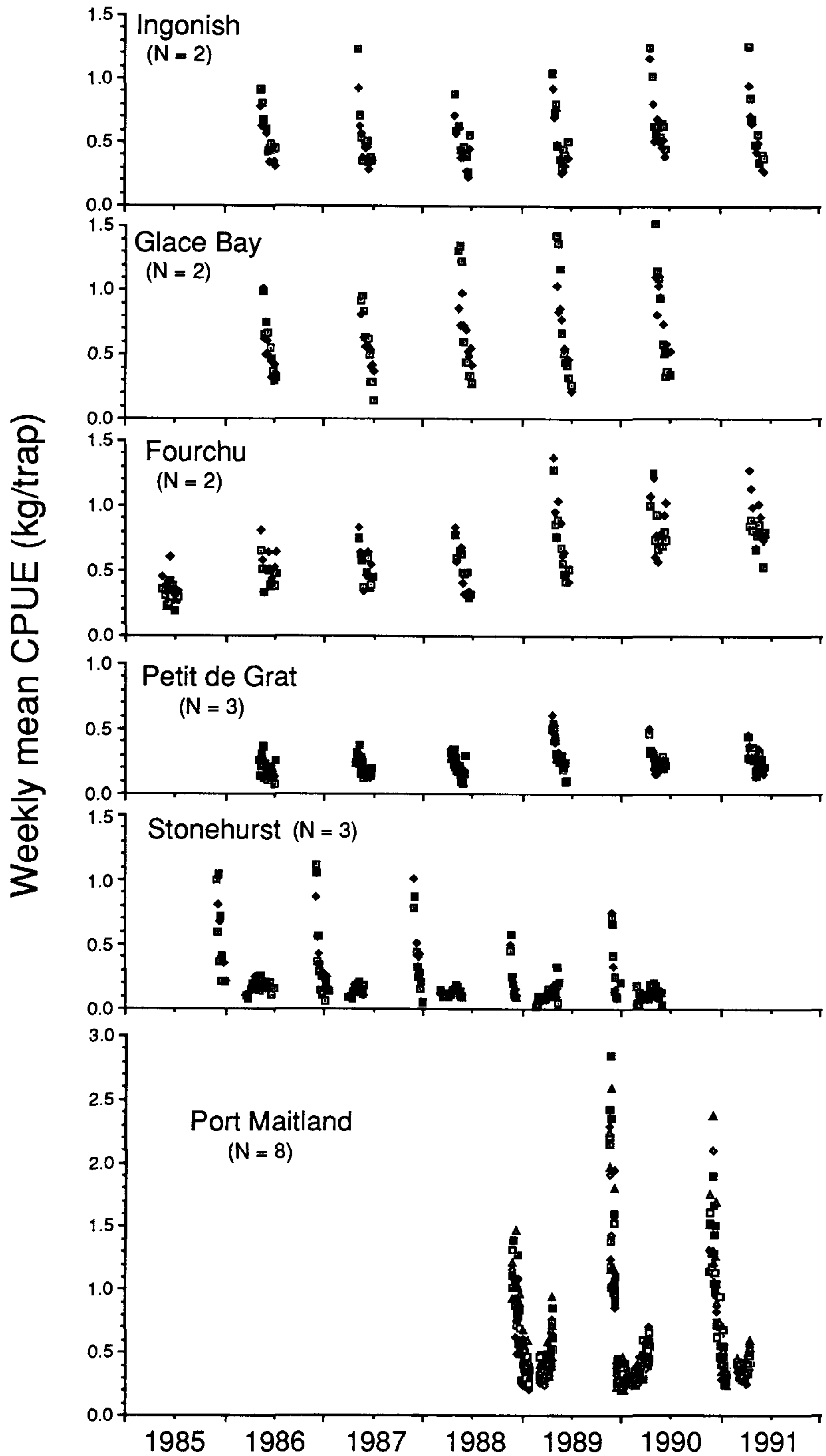

Figure 3. Mean weekly CPUE for individual fishers from different ports. All data shown for each port except Port Maitland. For this port the catch rates of only 8 of 20 fishers are shown.