Analysis of
"Independent Evaluation of Sea Trout Monitoring Programme"

This report reviews the independent evaluation of the Sea Trout Monitoring Programme performed by Dr. Ian G. Cowx, University of Hull Fisheries Institute dated March 1997.

The report could be improved substantially both for the outside reader and for those knowledgeable with the specific program if the technical problem generating the work was clearly articulated. It is our inference that the report addresses the following situation: the population of wild sea trout has evidently declined. This decline is thought to be related to the infestation of wild trout by lice. It is further thought that the infestation of lice on wild trout is increased by salmon raised in aquaculture pens. In order to study this problem there is a need to study infestation rates on wild trout and on aquacultured fish. Studies were undertaken to estimate the rates of infestation of wild sea trout. Concerns were raised regarding the conclusions that devolved from the study and so Dr. Cowx was retained to evaluate these conclusions.

Dr. Cowx prepared a report dealing with seven terms of reference. It is our view that Dr. Cowx responded to the terms of reference in a fair and unbiased manner. Because of this, our report deals mostly with the "future sampling plan," the subject of term of reference "v." Accordingly, we comment on items such as the status of infestation in aquaculture pens and quality assurance even though these issues might not relate to the central theme of Dr. Cowx's terms of reference. This is intended to underpin Dr. Cowx's cogent observations.

The first concern in the program is to estimate the infestation rate of sea trout. We take it that the objective is to estimate various infestation rates for single rivers or groups of rivers. The first concern in any program such as this should be to develop a theoretic sampling plan. Such a plan ensures statistically unbiased estimates and maximizes the efficiency of the sampling by calculating optimal sample sizes taking into account both variance and cost. An additional objective of a sampling theoretic approach is to determine whether sample sizes are sufficient to draw conclusions relevant to the design (e.g. how many fish would need to be sampled in order to detect a difference among years in infestation rates).

It is our finding that the sampling theoretic issues were not addressed and therefore it is difficult to arrive at inferences regarding the statistical bias and precision of the estimates. As we point out below the coefficient of variation is useful for some purposes. However, from a sampling theoretic point of view, one is more concerned with the sampling variance of the mean, the standard error. The standard error always decreases with increasing sample size. We find that the errors in the transmission of data etc. are part of many sampling activities and fortunately they can now be rectified. However it is difficult to determine from this report whether they had a major effect on the specific conclusions regarding sea trout infestation..

Based on our reading of the documentation, the next priorities should relate to the design of an over-all program to address the central issue of the relation of the dynamics of sea trout to aquaculture and the development of a sampling theoretic framework

The details of our review follow: . This review is organized into three sections, corresponding to the evaluation's conclusions and recommendations: the sampling plan, quality assurance procedures, and statistical analysis of the data.

The Sampling Plan

1. The independent evaluation does not describe the sampling protocols that resulted from the efforts of the Sea Trout Working Group and Sea Trout Monitoring and Advisory Group, or evaluate the reasoning that led them to these plans and how the plans were intended to be implemented in the sea farm lice monitoring program and in the wild trout sampling. The report identifies shortcomings in sampling but their impact to the overall intent of the program have not been quantitatively assessed.

The report recommendations concern only the wild trout sampling program. If the intent of the research is to determine the relationships between salmon cage farm lice infestations and wild trout lice infestations, then the sampling at the sea farms needs to be reviewed as well. Any analyses conducted between both data sets should also be scrutinized to determine the best forward course for any sampling revisions. The evaluation does state that only 40% of the variability in trout infestations was explained by an analysis, but model applicability was not assessed.

2. The underlying statistical distributions of the biological properties of sea lice infestations needs to be considered in relation to the sampling plan and data analysis. The evaluation asserts high coefficients of variation are the result of low sample sizes (the coefficient of variation of a sample is computed as the standard deviation/mean x 100; it is usually used to compare the variation among samples independently of the magnitude of their means). An associated statistic is the coefficient of dispersion, calculated as the sample variance/sample mean. If this ratio is > 1, the distribution is considered to be clumped, as opposed to repulsed (< 1) or Poisson (around 1). An evaluation of the adequacy of the sampling plan needs to consider what the form of the underlying distribution of lice counts might be.

In Table A4.2, the revised 1996 database, the abundance statistics are indicative of clumped distributions of lice counts (noting too the larger data sets, such as Invermore and the Dargle, represent pooled samples, and thus could exhibit decreased variability). A plot of variances versus means and sample sizes shows that the variance is a function of the mean. A plot of the coefficient of dispersion of abundance of the samples versus sample size shows no decline with increasing sample size.

Figure A6.2 shows pooled sample frequency distributions of infestations from areas of different infestation levels. They are highly skewed indicating the inherent variability in lice count data.

3. The precise counting of lice on fish may not be the best approach to maximizing the objectives of the program. Would it be sufficient to know that a fish has light or no infestation, medium or heavy infestation (perhaps along with a measure of its condition)? Such an approach could ease the burden on the entire sampling procedure and permit a greater number of fish to be assayed in the field. Categories of infestation could be constructed based on the ranges of infestation (e.g. quartiles) observed in samples taken since 1992 or according to categories reflecting critical biological masses of lice loads. Such specified ranges could be used in the field to insure consistency of results. An average index of fish condition could then be calculated on samples and used in analyses. This approach could be applied to data already collected and its usefulness evaluated.

4. The evaluation did not discuss possible statistical forms for the sampling plan based on the data collected so far; without statistical sampling, it is not possible to make inferences about population parameters. The evaluation does recommend revising the sampling plan to 20 key fisheries, chosen to incorporate ranges in 3 factors (distance from farms, estuarine topography, lice infestation levels), and collecting 30-40 fish from each river. Factors that should be considered in relation to this plan are:

1) In order for population estimates to be made, fishing effort needs to be recorded for the samples. It appears that different fishing gear is used for sampling in different rivers. Are estimates of relative fishing power of gear types available, if it is not feasible to use the same gear in each river in a consistent sampling method ?. (Also, it has been noted that some gear types can affect the number of lice staying on fish. Has this effect of gear been deemed negligible?)

2) The sampling plan should consider the model that will be employed to analyze the data, the ranges of predictor variables (will they be sufficient?) and what sort of biases may be associated with them. For example, how would estuarine topography be quantified or categorized as an analysis variable? Analysis of data already collected could provide some insight into the rationale of the suggested sampling strata. What are the data requirements of the proposed models?

3) Has the categorization of rivers by infestation level changed during the four years of the study? For example, has the Erriff consistently manifested heavy infestation levels?

4) What other variables might be important in elucidating the relationship between caged salmon lice infestations and sea trout? For example, what is the mechanism(s) of transmission? Are there other vectors besides salmon cage farms? Does the mechanism vary by lice stage? What is the significance of the composition of lice stages on a fish?

5) The costs of sampling need be taken into consideration.

6) What is the level of statistical confidence required for management action?

5. The recommendation that data be collected in an *ad hoc* fashion from other studies and used to augment the database would violate the need for samples to be drawn in a prescribed fashion.

Quality Assurance

1. All data collection programs have short-comings. No database is without missing data, keypunch errors, inconsistencies and sampling errors. It is remarkable that so much of the data discrepancies in the wild sea trout databases could be rectified because there were "paper trails".

2. All monitoring programs usually have some level of quality assurance protocols. (It was not clear from the report what were in place.) However, often what problems can occur is not knowable until after a program has been in operation through time and varying environmental and personnel conditions are encountered. In addition to having quality assurance reference procedures and training as recommended in the evaluation, establishing a Quality Assurance Work Group that brings together the data collectors, processors and analysts could be an important asset to the monitoring program in educating data collectors, processors and analysts to the needs, responsibilities and difficulties of each other's role, and promoting collaborative solutions to data quality assurance problems. Such a group would be well-positioned to provide a formal periodic review of program QA procedures to the sponsoring agencies. A work group such as this that is internal to the monitoring program is preferable to the outside annual auditor recommended in the evaluation since participants can be proactive in problem-solving.

3. A centralized, documented database is crucial to the success of a monitoring program. Data users should expect to obtain their data sets from one location. As a government sponsored program, the central database should reside on the government system with documentation of the sampling plan and quality assurance procedures. Copies of any data files constructed under contract for the government, for analysis or other purposes, should also reside on the central system, with appropriate documentation provided by the contractor. In the case of files constructed for data analysis, appropriate documentation would include outlier identification and handling methods, data imputation, etc.

4. The quality of the data collected or available from salmon cages about infestation levels was not addressed.

5. The evaluation was correct in pointing out that omission of non-infested fish from samples in the database will affect the assessment of lice prevalence and abundance measures (number of infested fish/total number of fish in sample; number of lice/number of fish in sample) but not that of the intensity of infestation (number of lice/infested fish). However, it was not clear if these omissions occurred randomly in the data or systematically from some locations, which would have different statistical implications.

6. The revised data forms should be evaluated with respect to data analysis needs. Recording data that is never used (e.g. not keypunched, or considered anecdotal) is a waste of resources.

Statistical Analysis

1. The results of the analytical method of choice, cluster analysis, are used to identify major groups of rivers, 3 or 4 in number. While the variables that are used in the cluster analysis are not specified in the evaluation, the following table shows that a simple classification of rivers by low, medium and high prevalence (percent of infested fish in sample, 0-39%, 40-69%, 70-100%) and low, medium and high infestation rate (based on the distribution of river medians: 0-49, 50-99, >= 100) can achieve a classification similar to cluster analysis that is more straightforward in interpretation and less affected by data difficulties. Cluster analysis is notorious for the instability of groupings—the choice of matching coefficient and type of linkage all affect the dendrogram. That changes in the 1996 data sets affected the number of clusters isn't surprising. A simple, non-parametric approach might be more suited to the data.

	Prevalence Level		
Infestation Level	Low	Medium	High
Low	Palmerstown Eske Dargle Eany Bunowen Roughty Sheem Owenea	Costello Crana Lackagh Southwest Drumcliffe	Aardbear Owenshaugh Dawros Lennan Coomhola Adrigole
Medium	Owengarve	Clifden	Delphi Erriff Invermore Culfin Furnace
High			Gowla Killary

2. The recommendation in the evaluation to reanalyze the corrected data sets is reasonable; however, if there is a "general lack of confidence in the data across the entire scientific and management community" (section 5.1) then perhaps a re-analysis should utilize methods in which data can be weighted according to the level of unease or uncertainty about it. Re-analysis should consider if imputation would be worthwhile. Also sensitivity analysis can be performed to assess the criticality of data to results.

3. High variances of data sets do not mean the data are invalid; however, such a situation may preclude certain types of statistical analyses being performed on the data.

4. The recommendation to conduct trend analysis of lice burdens on sea trout, based on the discussion of the data in the evaluation, seems premature. Only four years of data are available, and these are of varying quality. However, it would certainly be useful to tabulate the level of infestation of the rivers since 1992, to determine whether interannual differences occur.

5. The recommendation to conduct multivariate analyses of the data, given the data characteristics, should be approached with caution.

6. In section 5.5.1, it is suggested that the lice on the sea trout are different morphologically than those in the fish farms and that genetic finger printing techniques be undertaken to determine lice genetic differences among farms and sea trout. It would seem a morphometric analysis could be usefully done prior to the genetic analysis. The assumptions concerning the genetic analysis in terms of lice population isolation (e.g. are salmon cage infestations introduced from the environment outside the cages?) need to be clearly stated. However given the need to focus on infestation estimation and the general nature of the program, the genetic fingerprinting seems like a second order problem at this time.

7. The status of trout stocks and data necessary to undertake analysis is a big issue. It was not clear from the evaluation what role ICES could play and whether a joint project submitted to the European Union would duplicate or enhance the approach to the issue.