The 2001 Assessment of Perch in Lake Erie; A Review

Ransom A. Myers
Killam Memorial Chair in Ocean Studies,
Department of Biology,
Dalhousie University,
Halifax, Nova Scotia,
Canada B3H 4J1,

and

James R. Bence
Department of Fisheries and Wildlife,
Michigan State University,
East Lansing, Michigan,
USA 48824-1222

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1 Summary

The Yellow Perch Task Group (YPTG) is to be congratulated on their excellent recent progress in the assessment of perch in the 4 management areas of Lake Erie. The most recent assessment, i.e. the preliminary assessment provided to us as part of the review, is much better than previous assessments, and uses modern assessment tools. The assessment team has developed a custom assessment within the AD Model Builder environment. This has allowed them to evaluate alternative modeling options and has positioned them for greater flexibility in attempting to more realistically model the population dynamics of perch of western and central Lake Erie. A major advance over previous assessments was the use and evaluation of a wealth of fishery independent (survey) data.

The reviewers have carried out independent analysis of the perch in Lake Erie, and the results from these analyses are in general agreement with the YPTG assessment.

The reviewers have made extensive suggestions about how assessments and management strategies could be improved in the future; however, these should not be interpreted as concluding that the present assessment is incorrect. We emphasize that the following discussions of alternative methods describe suggestions about how to improve the assessments, rather than things that we feel "have" to be done.

A major issue that needs to be addressed in future assessments is to account for changes in catchability due to factors such as changes in growth. This could be done in future assessments by assuming that the catch at age in known without error and that surveys catchabilities are length dependent, and depend upon the mean length observed in the surveys. The use of ADModelbuidler has greatly improved the assessment, and we do not suggest that the YPTG move away from this approach. However, we do believe that it is relatively easy to convert the existing models with the assumption that catch-age-age is known without error. This is much preferable to using a canned program in the long run, e.g. the ICES package (however, the ICES package might be tried to verify the results). This cannot be done without some reprogramming. Alternative assessment approaches that allow for changes in catchability and errors in the catch-at-age might ultimately provide an even better assessment; however, such an approach would be very difficult to

implement.

We note that not all the CPUE series track one another. We suggest taking out each series at a time, to determine how sensitive the results are to each one.

Our analysis leads us to believe that the decision rules presently being used lead to a reasonable exploitation rate. On a longer term basis, we suggest that the task group move away from the $F_{0.1}$ management rule. Such an approach would take into account the spawner recruit relationship. The explanations of this are found in appendix 1 below.

We proceed by listing various concerns that we have.

2 Comparison of Trends

Different trends in abundance are seen using different indices. The most reliable index for older fish appears to be the Ontario partnership surveys. These show an increase in the year 2000 for all regions (in MU4 this increase began in 1998). An increase is also seen in the NY gillnet surveys in MU4, in the Ohio commercial trapnet fisheries and the Ontario trap and gillnet fisheries. This increase is not seen in the recreational CPUE data. This could be caused by a variety of factors, e.g. catch rates are heavily influenced by the seasonal distribution of fish, changes in bag limits and changes in the composition of fishermen caused by the decrease in walleye.

The trawl surveys show the very large 1996 yearclass, but do not show any increase for the older ages. We believe this is caused by the increase in visibility in the lake, combined with trawl avoidance. This should be investigated. We believe that survey data in the southern part of the lake that provides information on older ages would be very useful for future assessments.

It is important to better understand why the different CPUE time series show different patterns. Such discrepancies suggest that some of these series may not be directly proportional to abundance as assumed by the current ADMB model. Of particular concern is the discrepancy between the sport CPUE and other data sources. Commercial CPUE increased from 1998 through 2000 in all areas, but sport CPUE did not. Possible explanations could include changes in catchability, and changes in selectivity that operated differently for the different gear. This needs to be explored. There also appear to be longer-term discrepancies between sport and commercial CPUE. For example, in MU1 the two series show similar patterns through the mid-80s, but after that point diverge. Sport CPUE during the mid-90s was nearly as high as peak levels during the 1980s whereas 1990s commercial CPUE was much lower in the 1990s than peaks seen in the late-1980s. Perhaps some of these patterns are captured in the catchability blocks. But if there are large remaining temporal patterns in residuals, one or more of the fishery data series may be providing a false signal. We recommend alternative approaches be considered.

These approaches include exploring models where catchability varies gradually over time either following a random walk or in response to measured variables or model estimated population density. An alternative and simpler approach is to drop effort from the model, thus acknowledging that nominal effort is providing little information about actual fishing mortality.

3 Discrepancy between ADMB and CAGEAN Estimates

One major concern we have is the large differences between the ADMB and CAGEAN model estimates of recent biomass. At this point we do not understand the reasons for these differences. However we also believe that it will be a simple matter to distinguish two possibilities:

- The differences are due to the use of survey data in fitting the ADMB model.
- The difference are due to differences in how the fishery is modeled or differences in how the fishery data are handled.

To distinguish these possibilities, we suggest re-fitting the ADMB model, giving the survey data very low weights. If the ADMB converges on estimates close to the CAGEAN estimates this would favor the first possibility, whereas little change would favor the second possibility.

In the process of examining the data files to try to understand what was causing the difference, we encountered one aspect of the fishery data that we did not understand and potentially is a problem. The trap net catch data include values of zero. In some areas these are for an entire year and effort is also zero. We presume there was no trap net fishery in those years. However, in other years there is a zero recorded for the age-2 trap net catch. This seemed odd to us. It either suggests that selectivity for age 2 became near zero in some years, or that age-2 catch is either being left out or aggregated with another age in those years. We suspect these zero values could have a significant influence on the model fit.

4 Use of Survey Index Data

We have some concerns regarding the use of the survey data. These surveys are assumed in the model fitting to have constant catchability and selectivity over time, and the indices derived from the survey are assumed to have constant variance. Some of the text of the Yellow Perch Task Groups report, and our examination of data suggest otherwise.

1. The text of the report refers without much detail to changes in survey methodology.

2. In the later years essentially no older fish were caught in the trawl survey in MU1. A similar but not as pronounced phenomenon seems to occur for the partnership survey in MU3. These patterns do not seem to closely match estimated patterns of abundance at age or fishery CPUE at age. In the mu1.dat file, these appear to have been replaced with a large negative number, as a consequence of the constant used for the log-transformation. This may not provide a proper weighting for the observations. At the very least, some alternative weightings for the zero should be investigated. There are alternative approaches to this problem, but they would require a bit more work[1].

3. There have been large changes in survey effort over time. If sampling were at random locations (or could reasonably be treated as such), this could be dealt with by a weighing factor, proportional to the inverse of the variance. This would require very little change in the code.

4. Perhaps the most serious question about the surveys concerns the comparability over time. It appears that the gill net surveys have been at fixed locations,

[1]We note from a closer examination of the code that the older estimates are not actually used in the ADMB assessment.

and the trawl surveys were initially at fixed stations and now follow a stratified random design. Either additions and deletions of fixed stations or change from a fixed station design to the stratified random design could produce artifactual changes in abundance indices (including changes in age-compositions). In particular we are concerned that the stations in the early years may not be representative of the wider survey in later years. For example it looks like the early Ohio surveys may have been the best habitat for older fish. We believe that potential spatial effects need to be accounted for in estimating an overall abundance index for a region, when the spatial sampling design has changed over time. In the case of stations that have been fixed over time, it is a relatively easy matter to investigate this using a generalized linear mixed model. RAM has done a lot of these models, and they may help make the series comparable over time. RAM has offered to help with this if the raw data could be made available.

5. Changes in water clarity may affect survey catchability. The possibility of including water clarity in the index should be investigated. For example, catchability in the trawl surveys is probably caused by changes in water clarity which are known to affect catchability by trawls of *Stizostedion lucioperca* (Buijse 1992).

6. We have some concerns about the log transformation of the geometric means in the model. See below.

7. Not only does the model assume selectivity that is constant in time, it also assumes that the surveys select equally for ages 2-6. We believe this assumption is too strong, and recommend at a minimum that selectivity be estimated for several of the younger ages for each survey type. We carried out an analysis with variable age selectivity, using the ICES VPA package, the result was that older fish were less selective in the gillnets.

5 Recruitment Estimates for Incoming Age 2 Yellow Perch

Recruitment appears to be estimated by a linear regression that does not go through the origin. This may cause problems. The documentation is not sufficient for us to determine exactly how the regressions were done and the resulting output converted into an age-2 recruitment for 2001 in each area.

The yellow perch task group's base assessments did not make use of YoY (or age-1) survey data in the stock assessment. They used these data in a second stage of their analysis to predict future recruitment. We suggest that these two approaches should be combined into one analysis in future assessments.

As a guide to future analysis, one of us (RAM) carried out an analysis of density-dependent mortality at early ages (Appendix 2). If there is density-dependent mortality at younger ages, as is commonly observed for groundfish (Myers and Cadigan 1993), then the assumption that the survey indices are proportional to true abundance will cause recruitment to be overestimated in projections. The current approach used by the yellow perch task group acknowledges this problem by (a) not using survey data in the assessments for ages less than 2, and (b) fitting a nonlinear relationship between YoY and age-2 indices to make short-term projections. We believe that the survey data for younger ages could be incorporated into the assessment directly, but this would require that the density dependent survival of age-0 and possibly age-1 be estimated during model fitting. This has advantages of making more complete use of available data, and also of using tools available within the stock assessment to characterize uncertainty in the short-term projections of recruitment to age-2.

6 Potential Influence of Changes in Growth on Selectivity and Catchability

Evidence is presented in the Task Group Report that growth has changed over time, and has done so differently in different areas. The Task Group Report discusses briefly how these changes might influence selectivity. We are concerned that growth changes could have changed selectivity over time and would have had differential effects on different types of surveys and fisheries. The assessment models do not allow such changes, and if such changes are large they will influence the assessment results. For example, if age-2 fish were smaller than average in 2000, they might be underrepresented in the fishery while still being large enough to be nearly fully selected by the surveys. If so, this could explain discrepancies between assessments that use the survey data (e.g., ADMB) and assessments that do not.

Perhaps the most pressing need in the assessment is to make the selectivity size dependent. Ana Parma did this for Pacific halibut fishery; however, this model is probably too complex to be of practical use here (and in fact may be too complex to be of practical use in any fishery). Perhaps a better approach would be to assume that catch is known without error as in a traditional VPA.

We suggest that an assessment be carried out using the following assumptions:

- catch-at-age is known
- selectivity is constant in the surveys with respect to length (or weight).

During the meeting the YPTG attempted to deal with this by blocking selectivity for different time periods. We believe a direct modeling of selectivity is a preferable approach. The reviewers believe that the issue of size dependent selectivity can be dealt with relatively easily by using a model that assumes that catch-at-age is known, but that the selectivity depends upon the average size in each year at age. This model would be relatively easy to implement. The current approach to modeling changes in catchability is to incorporate time block specific catchability parameters. Changes in selectivity were only modeled for one age and only for the partnership gillnet data by estimating a new parameter starting in 1997.

Changes in catchability are likely to be most pronounced for fishery data but also could be substantial for surveys. Changes in size at age could influence both. Also, regulation changes appear to influence both selectivity and catchability for fishery data.

7 Aging Errors

We are concerned about the influence of aging errors. A potential problem with aging can be seen from the gillnet surveys. The yearclasses adjacent to the strong 1996 year class often had strong positive residuals in the older ages in the gillnet surveys. This suggests a lot of aging error.

We suggest that this issue be evaluated, and if it is a serious problem, there are methods that adjust for the aging error (Fournier and Archibald 1982). However, we do not view this as a major problem at this time.

8 The Weighting of Different Information in the Assessment, i.e. the λ's

In the present model, the weights for each part of the objective function, i.e. the λ's are estimated using an iterative approach in which one enters λ's which are generated and output in the report of the model. After each iteration, the data file is updated with the new λ's until convergence (usually within a small number of iterations), The iterative process is stopped when the λ's remain relatively constant.

A similar method is used in the ICES approach, but the iteration is automatic (and the catch is assumed to be known without error) (Darby and Flatman 1994).

In general it is not required to iterate through the process if a maximum likelihood approach is used. For each part of the objective function, the likelihood is written, and for each survey a separate variance (or CV) is estimated. This eliminates the iteration by hand.

9 Objective Function

There are four components of the objective function used in the perch assessments: 1. the catch 2. the effort 3. the two CPUE series, and 4. a penalty on the average fishing mortality.

In the objective function, a small penalty on the average fishing mortality is added in the last phase. That is,

```
f+= .001*square(log(avg_F/.2)
```

The effect of this this penalty is effective a weak prior on the fishing mortality around $F = 0.2$. It is unclear why this is needed, or if it has a large effect in the end. Perhaps, it would be clearer if this was removed.

The effort and the CPUE data form the main component of the objective function. However, it is impossible for the reader to understand the true relative importance of the different series from the present manuscript. Many of the important details can only be determined from a detailed examination of the tpl files. We have two suggestions on this.

First, it would be useful to have table that specified the final λ values used in the analysis and relative number of terms in the sums of squares. Some of the CPUE series used many ages, and some used only two, while the effort series are effectively only one. This leads to different weightings that need to be well understood.

Second, a robustness analysis needs to always be included. This does not have to be extensive, but should at least include eliminating some of the questionable series. This is particularly needed if different series show different patterns.

One final question, the "ncount" is used in the final line of the objective function. This term does not change with different parameter values; it is not clear why it is included.

10 Differences in the Data

The MU1 gillnet data shows a very different pattern of recruitment from the trawl surveys.

11 Assumptions about the errors

The model assumes that the errors in the catch at age are independent across ages and years, and that the errors are lognormal. Alternative should be investigated, because the residuals show clear year effects. Two alternative ways are described in (Fournier and Archibald 1982; Myers and Cadigan 1995).

12 Plots and Tables

More effort needs to be put into improving the display of model output. Plots should include residual plots, observed and predicted values for the different data sources, and the observed and predicted catch at age data. It would also be useful to include tables of fishing mortality by gear type and years. It would be useful to look at stock assessment documents from other regions to develop standard display plots and tables.

13 The Geometric Mean

In the assessments of Lake Eire perch, the catch-at-age data is combined with indices of abundance. Many of the indices included in the assessment use the geometric mean (with a small constant added to the zeros) to transform the raw data into an index of abundance. The critical assumptions here are that the geometric mean estimates are proportional to the true abundance, and that the log of the geometric mean series has approximately constant variance.

We first note the following property of the geometric mean, G, compared to the arithmetic mean, μ, if the deviations from μ are small compared to the value of the mean:

$$G \approx \mu(1 - \frac{1}{2}\frac{\sigma^2}{\mu^2})$$

(see (Kendall, Stuart, and Ord 1987) page 67). That is, if the the deviations are small, and the CV is constant (which is not a bad assumption for most survey data) then the G will be proportional to μ. This makes the use of G appear reasonable for many purposes. However, we have three potential concerns.

First, the deviations from the mean are not small. We do not know if this will result in a nonlinear relationship between the true abundance and G. Second, (and perhaps more importantly) the addition of a small constant may create real problems and biases. This appears to be the case for the older ages in the Ohio MU1 surveys (see the MU1.dat file, however, these are not used in the assessment at the moment). Third, the geometric mean is log transformed within the model's objective function, this may create unknown problems, because it may"overcorrect" for skewness. These potential problems should be investigated.

We note, that past uses of lognormal based estimators for survey data, do not appear to have been successful in practice (Myers and Pepin 1990; Syrjala 2000). However, the estimators studied by Myers, Pepin, and Syrjala were minimum variance, unbiased estimators (if the lognormal assumption was *exactly* met), and are thus different from the use of a simple geometric mean.

Until the potential problems are understood, we suggest that either the arithmetic mean be used, or that the robustness of the assessments be studied by carrying out alternative assessments where the results of the geometric and arithmetic mean are compared. Note that this comment does not mean that plots of the time-series of the geometric mean abundance indices should not be examined, they may (or may not) be able to pick up trends not apparent in the time-series of the arithmetic means.

14 $F_{0.1}$ Rules

Although the present use of the $F_{0.1}$ seems to produce reasonable results as used by the task group, we have concerns about its use. If it is used, it should be done in a manner consistent with the assessment model. See Appendix 1.

15 Stock Structure

The definitions of stock structure are nearly always compromises from what is practical to manage and what is biologically realistic. Perch in Lake Erie are no different. Several lines of evidence suggests that there are more than 4 stocks in Lake Erie. For example, recruitment on the northern and southern parts of MU4 do not show the same pattern (see next section). On Fig. 2 of the report, there seems to be a large catch from Ontario on the boundary between MU1 and MU2. Could this be MU1 catch taken in MU2?

In practice, it may not be possible to carry out assessments in smaller regions. If this is the case, then it is important to manage in such a way that maximum yield can be archived with minimum risk. If there are several subpopulations in each management unit, then they probably have different catchabilities and, and perhaps different population growth rates. It is thus likely that the more catchable, less productive subpopulations could be eliminated (Ricker 1958; Ricker 1973), with a great loss in total productivity. Further, the spatial dynamics of this stock is likely to be quite complex. Thus, regulations must be such that local stocks are not overexploited.

16 MU4

We did not carry out a detailed assessment of MU4 for several reasons: (1) there will be no short term changes in the management for this region because of a long term agreement not to increase the quota on this region that has 3 years left to run, (2) information was presented at the meeting that suggested that the stock management definitions for this region, (3) the NY surveys show that this stock has increased for the last 3 years, which is a longer term improvement than the other regions.

We note that the research surveys suggest that more than one stock exits in MU4. The New York surveys show an internally consistent pattern of recruitment. That is, strong yearclasses, e.g. the 1996 yearclass, are strong in the age 0 and age 1 surveys. A similar pattern exits for the Ontario surveys. However, strong year classes are not consistent between the northern and southern part of MU4. This is consistent with the general observation that the spatial scale of recruitment for freshwater species is relatively small, i.e. less than 50 km (Myers, Mertz, and Bridson 1997). This suggests that the stock definition in this region should be reconsidered.

17 Estimation of Selectivity from Tagging Data

A major uncertainty in the assessment is caused by changes in selectivity caused by changes in growth. If tagging data exists, the selectivity of the different fisheries could easily be estimated using the available tagging data (Myers and Hoenig 1997).

18 General Code

The computer code as presently used is custom written for each run. It would be easier to work with if it was written in a more general form.

In the code a 1 is added to all calculated numbers before log transformation. This does not seem necessary, as these numbers should all be positive. This may have some small effect of bias.

In the run for the MU1 model, the angling q's are stuck at -15 (log). This may have been fixed on later versions of the code.

19 Broader Questions of Multispecies Management

As a broader question, information on species interactions will be crucial to the longterm multispecies management of the fishery.

For this purpose, it is very useful to have longer time series of abundance of perch, particularly during the period when walleye abundance was low in Lake Erie[2]. The data exists to carry out such an analysis (Shuter, Koonce, and Regier 1979), and would be of great utility in understanding the dynamics of this ecosystem.

20 Why the Fishermen Believe What They Do?

The fishermen are seeing higher catch rates than they have in a decade. This has led them to the belief that the quotas should be much higher. However, the present catch rates are similar to what occurred on a regular basis for the 1970's, and is caused by one yearclass that is large by recent, but not historical standards. It might be important to communicate to the fishermen that the changes in the quotas that have been recommended have been made because of a change in management strategy to improve long-term yield; rather than a drastic change in abundance.

Appendix 1: Reference Points

Your sheet seems to apply the Thompson and Bell approach appropriately under the assumption that only one fishery is operating at a time. Both selectivity and assumed weight-at-age schedule play a role in determining the resulting F0.1. My suspicion is that most of the difference is due to your different weight-at-age schedules, which seem to increase much more with age than those I used for the calculations in the review report. You might want to consider calculating F0.1 for the aggregate fishery as I did instead of for each fishery separately. I am assuming you have access to the spreadsheet I used that was provided earlier. I do not believe that it is the aggregate fishery part of this that produced the very different

[2]Since the meeting, Kevin Kayle has put to together some very useful older data on perch, that should allow stock reconstruction back to the 1960's

results. In these calculations you multiply the nominal F for a reference fishery by a ”weight” to get F's for other fisheries that operate. The appropriate weights would determine the relative values of the fully selected fishing mortality for each source. This would probably require some ”fiddling” in a spreadsheet application as I suspect you want weights so that at F0.1 the resulting YPR is allocated between the fisheries following an agreed upon allocation formula. However, given the similarity in the assumed selectivity patterns and resulting F0.1's for each fishery alone, the exact weight will not be critical and the resulting F0.1 will likely fall some where between the two you calculated.

In case it gets lost in the details I am NOT advocating that you use F0.1 calculated in this way to manage the fishery. Although this is a better way to calculate F0.1 than the method used for the YPTG reports we were given for the review, and F0.1 has often been a conservative measure, there is not guarantee this will be so. We were advocating consideration of alternative reference points and approaches, namely spawning stock biomass per recruit in the short tem and use of simulations based on a Lake Erie stock-recruit relationship in the longer term. However if you must base management on F0.1 it should be calculated in a way like the Thompson and Bell approach, that is consistent with your assessment model.

Currently RAH's are calculated based on an $F_{0.1}$ fishing mortality rate applied to an estimate of the population in 2001. Upper and lower bounds are determined by applying this fishing rate to a population equal to the point estimate of the population plus or minus one standard error. The $F_{0.1}$ rate is calculated using a Beverton-Holt approach, which assumes von Bertalanffy growth and knife-edge recruitment. Age of recruitment was set at 2.5 and weight-at-age was based on a model fit to partnership survey data. The resulting $F_{0.1}$ rate is then applied to fully selected ages, whereas, this rate is multiplied by an assumed selectivity (based on assessment parameter estimates) for other ages. The resulting harvest numbers are then converted to weight using average weight at age in the fishery. Although the resulting fishing mortality rates appear reasonable, and the approach has been applied consistently across areas, we have several concerns regarding the underlying logic of the recommended harvest policy.

First, although $F_{0.1}$ has been widely used in fisheries, it can sometimes provide very bad advice. Second, we believe that $F_{0.1}$ should be calculated using assumptions consistent with the assessment approach, and this is currently not the case.

The basic logic underlying the use of $F_{0.1}$ is that it does not make sense to fish harder over the long-term than the rate that maximizes yield per recruit. Also, fishing at $F_{0.1}$ will provide nearly as high a yield per recruit at substantially less effort, which may make it economically more efficient and also provide some buffer protecting the spawning stock. However, there is no guarantee that such a ”buffer” will be sufficient to avoid recruitment overfishing. Of particular importance is that nothing in the calculations accounts for whether substantial fishing mortality occurs prior to when fish begin entering the reproductive population. Hence $F_{0.1}$ can work well for some populations, but lead to stock collapse in other cases. In the medium-term we recommend a careful analysis of stock-recruitment relationships (including the magnitude of recruitment variability about the underlying relationship). Such stock-recruit relationships could be incorporated into an age-structured population model which could be used to evaluate alternative harvest policies (e.g., constant fishing rates, constant rates used with biomass thresholds and so on - Quinn and Deriso chapter 11). Such analyses can essentially become full scale decision analyses. We suggest that the implied relationship between yield and fishing rate be explored using standard deterministic age-structured fishery models as a first step, including the calculation of F_m (fishing mortality rate that maximizes yield).

On a shorter time-frame we recommend a comparison of the $F_{0.1}$ fishing rate with other reference points that do not require a stock-recruit relationship. One such set of reference points are the $F_{x\%}$ parameters based on spawning population per recruit. For example, $F_{40\%}$, indicates that fishing at this rate will reduce spawning stock per recruit to 40% of the unfished level. Recommendations on the appropriate percentage have ranged from about 20% to 40% although recent recommendations have tended toward the upper end (i.e., lower fishing rates). Even if such reference points were not used for setting RAH's, they are useful for comparative purposes. For example, if current exploitation is well above the $F_{20\%}$ level, this would seem to indicate that current harvest policy is risky in comparison with experiences in other systems. Likewise, current fishing rates below the $F_{40\%}$ would be suggest that current harvest policy may tend toward the conservative side.

As mentioned above, the way that $F_{0.1}$ is calculated is inconsistent with the model used to assess yellow perch. The assessment model allows for gradual recruitment to the fishing gear, and for selectivity less than 1.0 for older fish. In contrast the calculation of $F_{0.1}$ is based on the assumption of knife-edge recruitment. The current approach is conservative in this regard because all ages except age-2 are assumed to be fully recruited in the calculations of YPR. Another area of inconsistency is that the weight-at-age implied by the growth model used in the calculation of $F_{0.1}$ is very different than the average weight-at-age in the fishery.

The Beverton-Holt approach to yield calculations cannot deal with the complications described above. An alternative approach is to use the Thompson and Bell method (Ricker 1975). This essentially involves calculation of a table that follows a single recruit as it passes through the fishery. At each age the numbers alive at the start of the interval, the numbers harvested by each fishery component (trap, gill, sport) and the weight of the harvest are calculated. Overall yield per recruit is then the sum of the weight harvested over all ages. Such an approach can account for the selectivity patterns of each fishery component, the allocation among the components, and differences among the fisheries in the average weight-at-age of harvested fish. An essential assumption of the approach is that weight-at-age of harvested fish will not change much as fishing mortality rates vary. This contrasts with the Beverton-Holt approach, which assumes that at higher fishing mortality rates, a larger proportion of the harvest will occur earlier in the year when fish have grown less. We suspect that the Thompson and Bell approach applied on an annual basis is a reasonable approximation but, if this were of concern, calculations could be done at quarterly or monthly intervals. Calculations of spawning stock per recruit needed for the $F_{x\%}$ reference points can be done by a simple extension of the Thompson and Bell tables, adding information on weight-at-age for spawning fish and maturity-at-age.

Preliminary calculations using the approach described above for MU1 suggest that the resulting $F_{0.1}$ may lead to an unreasonably high exploitation rate. In contrast, exploitation rates set by the current policy may lead to reasonable percentage reductions in spawning stock per recruit, although the actual rate being used appears to be far below a true $F_{0.1}$ rate. In particular, $F_{0.1}$ calculated according to the Thompson and Bell method exceeds 10 and corresponding exploitation of fully selected ages is over 96%. At this level of fishing spawning biomass per recruit is reduced to about 7.5% of the unfished level. In contrast estimated fishing rates in 2000 lead to just under 40% of the unfished spawning biomass per recruit, and the value at the fishing rate corresponding to the current TAC will be higher. The very high $F_{0.1}$ calculated from the Thompson and Bell method reflects the fact that weight-at-age in the harvest is very flat with age, and hence there is little advantage to leaving fish to grow to larger sizes at older ages. At these very high fishing mortality rates the Thompson and Bell calculations are suspect because mean weight-at-age of harvested fish would, contrary to assumptions, decline as larger and larger fractions of the population were harvested. In our view using a yield per recruit reference point is especially problematic when mean at weight in the population and in the harvest show such different patterns.

Information used in these preliminary calculations is provided below. The selectivity and relative fishing mortality for the different gears is based on the "new" assessment results provided by Kevin Kayle during the review. The weight at age information is based on an initial examination of the data provided by Andy Cook during the review.

```
Selectivity (2,3,4,5,pooled 6+)

Trap:   0.00183273 0.118506 1 1 0.999999

Gill:   0.0466275 0.598224 1 0.999999 0.186339

Sport:   0.0814078 0.429677 1 1 0.171292

Relative fishing mortality on fully selected ages (versus gill)
(2,3,4,5,pooled 6+)

Trap:  0.19

Sport: 1.59

Weight at age (2,3,4,5, pooled 6+)

Maturity:    0.029 0.064 0.096 0.101 0.20757519
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Trap:   0.128 0.14 0.142 0.203 0.213

Gill:   0.094 0.111 0.115 0.127 0.133

Sport:  0.91 0.106 0.129 0.138 0.195

Maturity schedule (2,3,4,5,pooled 6+):  0.07 0.59 0.81 0.87 1
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Clarification of issues

The following questions was asked by the YPTG. The page number refer to the original pagination.

“Bell Thompson approach : using selectivity, got a lower $Fi_{0.1}$; F0.1=0.78 to 0.90 (gear dependent)- lower than indicated on page 17; could differences be due to selectivity used or different mean harvested weight at age? please see attached excel file for commercial gillnet and angler fisheries F0.1 ; mean weight at age for Ontario commercial gill net updated in excel sheet (used VBF estimated ages 2 to 12.)”

“Input parameters for Bell Thompson: Maturity indicated on Page 18 looks like Ont partnership survey weight at age MU 1 but for ages 1 to 4 and pooled 5+) not ages 2 to 6+ ”

Our response to the first question is as follows:

Your sheet seems to apply the Thompson and Bell approach appropriately under the assumption that only one fishery is operating at a time. Both selectivity and assumed weight-at-age schedule play a role in determining the resulting F0.1. My suspicion is that most of the difference is due to your different weight-at-age schedules, which seem to increase much more with age than those I used for the calculations in the review report. You might want to consider calculating F0.1 for the aggregate fishery as I did instead of for each fishery separately. I am assuming you have access to the spreadsheet I used that was provided earlier. I do not believe that it is the aggregate fishery part of this that produced the very different results. In these calculations you multiply the nominal F for a reference fishery by a ”weight” to get F’s for other fisheries that operate. The appropriate weights would determine the relative values of the fully selected fishing mortality for each source. This would probably require some ”fiddling” in a spreadsheet application as I suspect you want weights so that at F0.1 the resulting YPR is allocated between the fisheries following an agreed upon allocation formula. However, given the similarity in the assumed selectivity patterns and resulting F0.1’s for each fishery alone, the exact weight will not be critical and the resulting F0.1 will likely fall some where between the two you calculated.

In case it gets lost in the details I am NOT advocating that you use F0.1 calculated in this way to manage the fishery. Although this is a better way to calculate F0.1 than the method used for the YPTG reports we were given for the review, and F0.1 has often been a conservative measure, there is not guarantee this will be so. We were advocating consideration of alternative reference points and approaches, namely spawning stock biomass per recruit in the short tem and use of simulations based on a Lake Erie stock-recruit relationship in the longer term. However if you must base management on F0.1 it should be calculated in a way like the Thompson and Bell approach, that is consistent with your assessment model.

Our response to the second question is as follows:

These were meant to be weight at the time of year when spawning occurs. These rough values were calculated during the review meeting. I believe the assumption I made was that size-at-age at the time of the survey would approximate size of fish incremented one year in age at the time of spawning.

21 Appendix 2: Density-dependent mortality and the accuracy of the juvenile survey estimates

This appendix will be supplied separately.

22 Appendix 3: Alternative Assessments

In this appendix we describe the results of an alternative assessment. We do **NOT** claim that these results are superior to those produced by the YPTG; they are alternatives used to investigate the robustness of the assessment.

We also carried out an alternative assessment using the extended survivors package used in ICES (Darby and Flatman 1994). This method estimates a separate error variance for each survey/age combination, and has a variety of other differences from the assessment teams approach. This method assumes that the catch-at-age is known without error. No shrinkage or downweighting of the time-series were used in the assessments, although this is the default option for this program.

This approach differs from that used by the YPTG in that we assumed that the catch was known without error. However, we did not assume that the selection of the surveys were constant for all ages. However, we did assume that the selection of the surveys did not vary over years for a given age; as stated above this may cause problems if changes in growth rates affect selectivity. This method also allows fishing mortality at age and year to be unconstrained parameters, as opposed to following a fixed selection for each fishery for all years, or a block of years.

Although the assumptions of the method used in this appendix is very different from that used by the YPTG, the results are very similar.

22.1 MU1

The assessment was carried out using Ontario partnership gillnet surveys.

The major results of this alternative assessments is similar to that by the YPTG. The fishing mortality in the most recent year was lower than that estimated by the YPTG.

This version of the assessment does not show the large increase in the biomass of older ages as seen in the Ontario gillnet surveys. This difference is seen in large positive residuals for the last year. In this model run, the recruits at age 1 were estimated as a derived parameter. The estimate of the fishing mortality on all ages in MU1 was slightly lower than the YPTG in 2000. However, we do not believe the difference was very important. The overall results of the two assessments were basically consistent.

We also carried out alternative analyses for MU1, some of which did show a large increase in the year 2000; unfortunately, we cannot be sure that this increase is real.

,	RECRUITS,	TOTALBIO,	TOTSPBIO,	YIELD/SSB,	F (ages 1-5),
,	Age 1				
1975,	61967,	9696,	4905,	.0204,	.5063,

1976,	92710,	9894,	4681,	.0214,	.4553,
1977,	51968,	11064,	6623,	.0151,	.2965,
1978,	193604,	21903,	7864,	.0127,	.3707,
1979,	47530,	18284,	11909,	.0084,	.3548,
1980,	36685,	12655,	9458,	.0106,	.3350,
1981,	66519,	13652,	6873,	.0146,	.3352,
1982,	97955,	13766,	8013,	.0125,	.5356,
1983,	111497,	12701,	5401,	.0413,	.3448,
1984,	22546,	15285,	10175,	.0207,	.3498,
1985,	150721,	9942,	8284,	.0094,	.2524,
1986,	69009,	19266,	10999,	.0127,	.3128,
1987,	45734,	17099,	11874,	.0147,	.2737,
1988,	6217,	11997,	9323,	.0281,	.2826,
1989,	9032,	7069,	5700,	.0332,	.4090,
1990,	16346,	4407,	2687,	.0881,	.5622,
1991,	19857,	3135,	1528,	.1308,	.5819,
1992,	10561,	3156,	1862,	.2574,	.3221,
1993,	17069,	3027,	1630,	.1347,	.4292,
1994,	37671,	4070,	1563,	.0490,	.4418,
1995,	42541,	6040,	2442,	.0627,	.4119,
1996,	45728,	7713,	3585,	.0709,	.3214,
1997,	54331,	8259,	4038,	.0271,	.7014,
1998,	10570,	7097,	4706,	.0459,	.4604,
1999,	35384,	6602,	4009,	.0923,	.3174,
2000,	16228,	5610,	3599,	.0439,	.2390,
Mean ,	52692,	10130,	5913,	.0492,	.3924,
Units,	(Thousands),	(Tonnes),	(Tonnes),		

Log residuals fleet : Ontario Gillnets (g

Age , 1991, 1992, 1993, 1994, 1995, 1996, 1997, 1998, 1999, 2000

1 ,	-.41,	.92,	.27,	1.35,	1.34,	-.70,	-1.60,	-1.52,	.06,	.00
2 ,	.13,	.55,	.25,	.50,	.64,	.18,	-.47,	-.87,	-.14,	-.01
3 ,	-.75,	.10,	.21,	.20,	.50,	.47,	.13,	-.67,	.42,	.43
4 ,	-.77,	.09,	.30,	.46,	-.90,	.12,	.51,	-.20,	.18,	.60
5 ,	.00,	.00,	.00,	.00,	.00,	.00,	.00,	.00,	.00,	.00

We also carried out analyses using the USGS spring and fall surveys for ages 0 and 1.

22.2 MU2

Out alternative asseeessment for MU2 is similar to the results obtained by the YPTG. Again, the fishing mortality has greatly decreased in the last year, with a steady increase in biomass in the last few years.

,	RECRUITS, Age 1	TOTALBIO,	TOTSPBIO,	YIELD/SSB,	F 1- 5,
1975,	46677,	9342,	4246,	.0235,	.5902,
1976,	60164,	8610,	3878,	.0258,	.4976,
1977,	26571,	9066,	4990,	.0200,	.5937,
1978,	132065,	19750,	4559,	.0219,	.5517,
1979,	20160,	13002,	7453,	.0134,	.4268,
1980,	47119,	11546,	6451,	.0155,	.6361,
1981,	94686,	12325,	3599,	.0278,	.7935,
1982,	61090,	10107,	5271,	.0190,	.5654,
1983,	80500,	13509,	5936,	.0376,	.4169,
1984,	9145,	11537,	7599,	.0278,	.4980,
1985,	215131,	18065,	5250,	.0149,	.4990,
1986,	59364,	17344,	10626,	.0132,	.5348,
1987,	68626,	21327,	14313,	.0122,	.3341,
1988,	8150,	15549,	11610,	.0226,	.2967,
1989,	10552,	10295,	8121,	.0233,	.3834,
1990,	37236,	6912,	4054,	.0584,	.4342,

1991,	38816,	6598,	3134,	.0638,	.6046,
1992,	12821,	5993,	3500,	.1370,	.4348,
1993,	30215,	4555,	2356,	.0932,	.5165,
1994,	18322,	5829,	2615,	.0293,	.5157,
1995,	38559,	6488,	2614,	.0586,	.4980,
1996,	35348,	6388,	3213,	.0791,	.4324,
1997,	127735,	9280,	3698,	.0296,	.5404,
1998,	7692,	12149,	7618,	.0284,	.7200,
1999,	53917,	12417,	8074,	.0458,	.4809,
2000,	66565,	15491,	8132,	.0194,	.2213,
Arith. Mean ,	54124,	11287,	5881,	.0370,	.5006,
Units,	(Thousands),	(Tonnes),	(Tonnes),		

22.3 MU3

The assessment for MU3 yielded similar results to the above.

22.4 MU4

As described in the main text, we were not able to obtain a satisfactory alternative assessment for MU4.

References

Buijse, A. D. 1992. Dynamics and exploitation of unstable percid populations. Ph. D. thesis, Agricultural Univ. of Wageningen, Netherlands.

Darby, C. D., and Flatman, S. 1994. Virtual Population Analysis: Version 3.1 Users Guide. Information Technology Series no, 1, Directorate of Fisheries research, Lowestoft, UK.

Fournier, D. A., and Archibald, C. 1982. A general theory for analyzing catch at age data. Can. J. Fish. Aquat. Sci. **39**: 1195–1207.

Kendall, M., Stuart, A., and Ord, J. K. 1987. The advanced theory of statistics. Vol. 1 (Fifth ed.). Macmillan Inc., New York. Distribution Theory.

Myers, R. A., and Cadigan, N. G. 1993. Is juvenile natural mortality in marine demersal fish variable? Can. J. Fish. Aquat. Sci. **50**: 1591–1598.

Myers, R. A., and Cadigan, N. G. 1995. Statistical analysis of catch-at-age data with correlated errors. Can. J. Fish. Aquat. Sci. **52**: 1265–1273.

Myers, R. A., and Hoenig, J. M. 1997. Direct estimates of gear selectivity from multiple tagging experiments. Can. J. Fish. Aquat. Sci. **54**: 1–9.

Myers, R. A., Mertz, G., and Bridson, J. M. 1997. Spatial scales of interannual recruitment variations of marine, anadromous, and freshwater fish. Can. J. Fish. Aquat. Sci. **54**: 1400–1407.

Myers, R. A., and Pepin, P. 1990. The robustness of lognormal-based estimators of abundance. Biometrics **46**: 1185–1192.

Ricker, W. E. 1958. Maximum sustained yields from fluctuating environments and mixed stocks. J. Fish. Res. Bd. Can. **15**: 991–1006.

Ricker, W. E. 1973. Two mechanisms that make it impossible to maintain peak-period yields from stocks of Pacific salmon and other fishes. J. Fish. Res. Bd. Can. **30**: 1275–1286.

Ricker, W. E. 1975. Computation and interpretation of biological statistics of fish populations. Bull. Fish. Res. Bd. Canada. **191**: 1–382.

Shuter, B. J., Koonce, J. F., and Regier, H. A. 1979. Modeling the Western Lake Erie walleye population: A feasibility study. Technical Report 32, Great Lakes Fishery Commission Technical Report.

Syrjala, S. E. 2000. Critique on the use of the delta distribution for the analysis of trawl survey data. ICES J. Mar. Sci. **57**: 831–842.