STOCK ASSESSMENT NEEDS FOR SUSTAINABLE FISHERIES MANAGEMENT

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ABSTRACT

Much of the machinery of modern fish stock assessment is aimed at providing better estimates of current stock size, but the adaptive and feedback policies needed for sustainable long-term management are specified in terms of fishing mortality rate goals in relation to relative stock size, and estimates of optimum fishing mortality rates have dropped sharply since 1990. Should we elect to refocus assessment methods and data gathering more directly on estimation of current and optimum fishing mortality rates, we could probably bypass many of the costs and pitfalls that have plagued traditional assessment approaches. Further, we could concentrate more on providing advice about how to implement safe feedback policies, rather than just contributing to debates about how much is out there to be harvested.

The 1990s were a decade of dramatic change in fisheries stock assessment and harvest management. Much development took place of complex assessment models and methods for fitting models to data and for measuring uncertainty in the results (e.g., AD Model Builder, Fournier and Archibald, 1982). Dramatic stock-assessment failures led us to question some of the most basic assumptions of the models (see, e.g., Walters and Maguire, 1996). Meta-analyses of stock-size trends and recruitment relationships led to considerable downward revision in assessments of safe fishing mortality rates relative to natural mortality rates (Patterson, 1992; Myers et al., 1999). Articulation of the precautionary principle led to a shift in views about the burden of proof in assessment, from the notion that we needed to defend our estimates to the notion that the burden of proof lies with those who would claim it is safe to keep fishing or to fish harder (Dayton, 1998). Deep suspicion developed about the ecosystem impacts of fishing and about the efficacy of single-species assessment methods to provide useful advice about how to manage marine ecosystems sensibly and sustainably, and these concerns drove the development of various new methods for multispecies and ecosystem modeling (Sparre, 1991; Walters et al., 2000).

Here, we review some of the main difficulties that have plagued stock assessment in recent years, and we discuss directions that might be taken to surmount these difficulties. After 30 yrs of experience in the field, C.W. has come to believe that we need radical change in thinking about the aims of assessment, in the kinds of data that should be collected, and in the models that we use to link harvest management to concerns about ecosystem sustainability. In the following sections, we present these concerns and ideas as a series of assertions and discuss each.

THE AIMS OF STOCK ASSESSMENT ARE OFTEN DEFINED TOO NARROWLY

Much of the machinery of modern stock assessment aims to provide estimates of current, absolute exploitable biomass and stock structure (numbers at age, age-specific fishing rates), as though absolute biomass estimates were necessary (and even sufficient) for effective harvest regulation. To yield recommended allowable catches—total allowable

catch (TAC) or quotas—these biomass estimates are then often multiplied by estimated optimum fishing rates F_{opt} (TAC = F_{opt} × biomass), where the estimate of F_{opt} is based on relatively simple calculations such as $F_{0.1}$ (Deriso, 1987) or even simpler assumptions like $F = M$. Inclusion of mean stock-recruit relations in assessment models has become more common and usually leads to lower estimates of F_{opt}, but variability in recruitment data has led to considerable suspicion about the validity of such lower estimates.

If the aim of stock assessment is defined more broadly as the design of effective strategies and tactics for sustainable harvesting in the face of unpredictable environmental variation and other dynamic uncertainty (see, e.g., Hilborn and Walters, 1992), then it is not clear at all whether biomass estimation should be a high priority. We have known since the mid 1970s that strategies for dealing with dynamic variation can generally be expressed in terms of a 'feedback policy' or 'decision rule' relating the optimum fishing rate F_{opt} to current stock size (Mangel, 1985; Walters, 1986) and that constant-F policies are often a good approximation to the optimum rule (Walters and Parma, 1996). That is, the most important strategic focus for assessment should be on F, not on current biomass. Further, even when we see need to vary F with stock size (e.g., to implement a rule with a limit reference point or minimum stock size below which harvesting should be stopped), the decision rule can always be expressed in terms of relative biomass B/B_o, where B_t is present biomass and B_o is unfished biomass. Having a good estimate of the ratio B_t/B_o is not the same as having a good estimate of B_t and a good estimate of B_o. Indeed, we would know immediately something was fundamentally wrong with any assessment that required absolute biomass to prescribe a fishing-rate goal, because this assessment would be saying that this fishing-rate goal depends on the units of measurement of biomass, and that cannot be correct (basic assessment results about F should not depend on the units of measurement that we happen to use in assessment, else we would be saying that we can influence natural productivity by changing those units).

Biomass estimation does become a tactical priority for fisheries that are regulated solely by setting of annual quotas or allowable catches, then use of some simple in-season monitoring procedures to ensure that the allowable catch is not exceeded. TAC or quota-based regulation systems are attractively simple for fishery managers, and it is becoming relatively uncommon to see stock assessments that carefully examine other tactics (Perry et al., 1999) for directly limiting fishing mortality rates or achieving target rates, such as effort limits and space-time closures. We have come to distrust simple effort-limitation tactics because of risks of increasing catchability (proportion of stock caught per unit effort) with decreasing stock size and improving technology. Space-time closures are used very effectively to limit mortality rates in some fisheries, e.g., Pacific salmon and herring, but these are mainly situations where we know annual quota or TAC management would be very dangerous because of high stock variability. Marine protected areas (MPAs) are gaining popularity as a way of limiting fishing rates, but few stock-assessment models have represented spatial dynamics well enough to provide credible estimates of the impacts of alternative MPA options. A resurgence of interest is likely in the classic idea of protecting spawning stocks through more conservative regulation of age selectivity (increasing age at first capture) so as to allow more spawning before fish first become vulnerable to capture; such options can be effectively evaluated with existing age-structured assessment methods.

It is understandable that assessment scientists have tended to avoid much careful analysis of tactical options for limiting fishing rates; such analysis involves a lot of messy, de-

tailed calculations related to seasonal and spatial fish and fishing dynamics. It is easy to shrug off these details by pretending that they can be left to managers as 'implementation problems' rather than assessment problems. Unfortunately, providing only simple assessment results can help promote the use of correspondingly simple and often dangerous regulatory tactics (the 'managers' don't know how to do the detailed calculations either). We should not be letting convenience and simplicity in assessment calculations drive or limit or promote the design of correspondingly simplistic regulatory policies.

Traditional Assessment Methods and Data Have Not Served Us Well

We can examine historical performance of assessment methods in quite a number of cases, using retrospective information on what must have been present at various times in order to produce catches actually observed later. Some of these cases (see, e.g., Parma, 1993; Walters and McGuire, 1996) have revealed really bad estimation errors, particularly overestimation of stock size (and underestimation of F) during declines, which have probably helped to promote or excuse overfishing. That is, the assessment methods are prone to failure under just the circumstances where we most need accurate assessments.

To understand why existing assessment methods have often failed, and will probably continue to do so in the future despite more elaborate estimation methods, we must understand two basic things about these methods. First, the methods are basically just variations on removal or depletion estimators. That is, we use information on absolute removals (catches) in conjunction with information on relative stock response (changes in relative abundance and size/age composition) to infer how large a population had to have been to produce the observed relative responses given the observed absolute removals. Just like simple removal or depletion models, however, the performance of the methods still depends on seeing clear signals in the relative impact data, no matter how elaborate or complex the equations and data used in the analysis might look (and no matter how thick a report we might write to describe the details of the data and calculations).

Second, every assessment method involves two fundamentally different types of models: a 'state-dynamics model' for the ecological processes (growth, mortality, recruitment) leading to the data, and an 'observation-dynamics model' for the relationship between the (unobserved) state dynamics and the response data that we actually have at hand. In principle, errors in the assumptions of either of these models can lead to bad assessment errors. Biologists have typically looked mainly for weaknesses in our state-dynamics (population-dynamics) models, particularly in relation to assumptions about recruitment processes, but in fact, every really severe assessment failure we have seen has been due instead to bad assumptions about the data. Two problems in particular have plagued us.

(1) It is really difficult to find an abundance index that is actually (and strictly) proportional to stock size; commercial or recreational catch per effort routinely fails in this regard for obvious reasons (nonrandom search), and even research surveys often show 'hyperstability' (Hilborn and Walters, 1992) due to range and density changes under stock decline.

(2) Age/size composition data are much less informative than they appear, because of confounding of effects of mortality rate changes with changes caused by recruitment trends and by complex, changing patterns of vulnerability with age and time.

Abundance indexing problems have been well recognized by assessment scientists, but we have been less quick to admit that changes in age/size vulnerability can also be a serious problem. For example, fishers may pursue smaller fish, often by moving to in-shore locations, as stock size declines; when not recognized, this shift appears in the assessment models as the appearance of increasing recruitment.

Modern stock-assessment models typically have a few key, core, or leading popula-tion-dynamics parameters (unfished biomass, natural mortality rate, stock-recruitment curve slope) that largely determine estimates of optimum fishing rate, then a very large number of 'nuisance parameters' to account for recruitment 'anomalies', age dependence in F, and details of the observation process. Indeed, fitting assessment models with doz-ens or hundreds of such parameters is apparently seen by some as a virtue (my model is bigger than your model), as though estimating many nuisance parameters in order to obtain a better fit to the data were somehow letting us 'see' the core parameters more clearly. In fact, all we are really doing is using up most of the information in the data on the nuisance parameters, without necessarily improving the core parameter estimates at all (or even making them worse, an 'overparameterization' problem as discussed by Ludwig and Walters, 1985).

Stock-assessment scientists have contributed to this sad state of affairs in two main ways. First, most of us have not been deeply involved in actually collecting the data, so we are often hesitant to speak out about how bad the data really are or to make sugges-tions about alternative types of information that might be more informative. Second, for obvious psychological and institutional reasons we have worked hard to develop cred-ibility, and that has often meant grossly overselling our results or at least failing to point out some basic problems (e.g., not admitting that getting a good fit to data does not in any way imply that the model is 'correct', especially when we have estimated many nuisance parameters). The credibility issue should become less important now with shifts in the burden of proof (indeed, it should become a virtue to admit just how uncertain we really are), but I suspect that we are a long way from being able to take a leadership role in demands for more useful information.

ESTIMATES OF OPTIMUM FISHING MORTALITY RATES HAVE DROPPED SHARPLY IN RECENT YEARS

Until the early 1990s, most of us would have been quite willing to prescribe $F = M$ (fishing mortality rate equal to natural mortality rate) as a relatively safe objective for fishing mortality, lacking precise information on age selectivity, maturity, and recruit-ment. $F_{0.1}$ calculations (which ignore stock-recruitment effects) were thought to be quite conservative and usually result roughly in $F = M$.

This supposedly conservative starting point was first challenged seriously with real data by Patterson (1992), with an analysis of pelagic fish stock trends from around the world that showed decline or collapse has been common when $F > 0.6M$. At around this time, C.W. started seeing $F_{opt} \approx 0.5$–$0.8M$ in a few assessments where he was accounting for mean stock-recruitment effects (Newfoundland cod, Hudson River shad), but he as-sumed these to be unusual cases where recruitment declines at low spawning stock were possibly due to confounding of spawning stock size and environmental effects.

Then R. A. Myers and his colleagues started to publish their widely cited meta-analy-ses of stock-recruitment data, most recently a dramatic synthesis of information about

stock-recruitment curve slopes expressed as maximum reproductive rates per spawner at low spawner biomass (Myers et al., 1999). When we substitute their results into age-structured population models and calculate optimum equilibrium fishing rates F_{opt}, the results obviously imply $F_{opt} \ll M$ for most species/stocks. The main exceptions are cases where regulation or natural accident has led to age-selectivity patterns such that most fish have at least one chance to spawn before becoming vulnerable to high fishing rates (which may be a key to safer management in the future; see Myers and Mertz, 1998). Two factors conspire to make recruitment overfishing more likely than had been suspected before 1990. First, fishing can have a large effect on mean fecundity per spawner for species that are fished over many ages and become much more fecund with age. Second, recruitment-curve compensation effects, measured as the ratio of maximum juvenile survival rate at low spawner densities (recruitment curve slope) to the juvenile survival rate at unfished densities, are not as large as would be expected on the basis of treating stock-recruitment data as a 'shotgun scatter'; ratios exceeding 10 (10-fold improvement in juvenile survival absent intraspecific competition) are not that common.

Today, any assessment that results in $F_{opt} \gg 0.5M$ must be very carefully justified, either by clear demonstration that higher rates have in fact been sustained for several fish generations or that the age-selectivity schedule permits virtually full replacement of recruits (by the fish that survive to spawn) prior to the age at first capture. As a worrisome point about how fast our views are changing, C.W. has reviewed three stock-assessment documents in the past year where the authors were apparently not even aware of the Patterson and Myers et al. findings, indicating that even very important findings can be slow to spread in the fisheries-assessment community.

WE SHOULD FOCUS ON METHODS FOR DIRECT ASSESSMENT OF FISHING RATES

So we are facing two really frightening challenges in fisheries assessments today: growing evidence that the types of data upon which we base most assessments can be very misleading (yet are already quite expensive to collect) and growing evidence that fishing rates generally should be considerably lower than we had previously assumed. We can deal with these challenges in two ways: we can seek incremental improvement in data and methods, while pretending that our methods are basically sound but need more 'fine tuning', or we can seek more radical changes in information gathering and regulatory policy design. We have come to believe that we need to try the latter approach.

Suppose we had some direct way to measure current fishing mortality rate F_t. By 'direct' we mean a way to measure the probability of a fish's being harvested, not the indirect method of dividing measured catch by a suspect biomass or numbers-at-age estimate. Such a method would actually solve the biomass-estimation problem (biomass = catch/F_t), while giving us immediate information on whether fishing rate goals (F_{opt}) were being achieved and about changes in fishing technology (or stock distribution) that might threaten increasing F under effort regulation tactics. Considering how fast many modern fisheries can change (effort dynamics, technology), suppose further that this direct measurement system were quick to produce results (no long delays for fish aging, convergence of virtual population analysis, etc.). Such a measurement system could be used basically to supplant almost all of the complex data gathering and assessment that we do today, by providing direct and timely information for harvest regulation. At a very minimum, it would give us the ability to use 'input control' (effort regulation) methods more

safely, by providing direct estimates of relative change in fishing rates per unit fishing effort (catchability coefficients) over time. That is, it should provide the ability to 'track' adaptively changes in efficiency that have plagued input control approaches to regulation.

Direct estimation of the fishing mortality rate can be approached in at least three possible ways.

(1) The most obvious and promising approach is to use routine, annual tagging experiments. In this case, F is estimated as the proportion of tags caught within a short time after tagging, corrected for tag loss/mortality.

(2) In some cases we could develop some modern variation on the concept of a 'swept-area method' ($F = q \times$ effort, $q =$ [area swept by unit effort]/[area over which stock is distributed]). To avoid biases due to nonrandom fishing patterns, we would need to use spatial fish-density mapping (from catch per unit effort, surveys) in conjunction with spatially referenced log-book information to determine how the area actually swept by gear is related to the stock distribution. Our experience with this approach (e.g., Walters and Bonfil, 1999) has been almost equally discouraging, mainly because of uncertainty in estimation of the area-over-which-stock-is-distributed part of the method (too many 'black holes' in spatial data, for which we have no idea of even relative density).

(3) In other cases we could use short-term (e.g., seasonal) depletion (decrease in relative abundance) information, again in a spatial statistics/mapping setting. Our experience with this approach has been mainly discouraging, because of confounding effects of a variety of biological factors that cause short-term variation in distribution and catchability (dispersal to or from heavily depleted sites, seasonal movement, behavior changes).

Elsewhere in this issue, we discuss promising simulation experiments suggesting that routine tagging could provide a cost-effective alternative for direct estimation of F (Martell and Walters, this issue). Basically, we have found that F, as a proportion, can often be quite well estimated by surprisingly small tagging programs and can be used to update estimates of time-stock-size-dependent catchability so as to provide a basis for regulating F by controlling fishing effort. The tagging method has to meet three stringent requirements, however: (1) tag loss/mortality rate must be negligible or known; (2) tagging must be representative over exploitable population (every exploitable fish must have roughly equal chance of being tagged, though not necessarily equal chance of being harvested); and (3) every tag caught must be reported (or reporting rate must somehow be known exactly). These are tough requirements. (1) would probably imply expensive, in situ tagging methods for most fish species; (2) implies broad spatial coverage in tagging; and (3) would probably require very substantial rewards for tag returns. In a recent advisory project in Australia, C.W. recommended dealing with (3) by applying a license tax equal to $100 \times$ the expected number of tags caught per license holder and refunding $100 per tag returned, so that on average the honest (or greedy) fishers ought at least to recover their fees.

When we become discouraged about the technical and institutional innovations needed to make tagging methods work, however, we should stop to think about just how much time and money are already being spent on size/age sampling, surveys, and complicated assessments that are not giving the answers needed for safer fishing-rate regulation. These traditional types of data probably would not give much improved assessments even if 10

times as much were spent on them; the problems are coming not from inadequate sample sizes but rather from structural uncertainties about how the data are related to what is actually happening in the stocks. Further, improving precision in traditional types of data today will not help recover better estimates of fishing impact from informative historical periods of rapid stock change; those learning opportunities are now lost to us forever.

At a very minimum, fisheries scientists should consider investing much more in tagging experiments as a cross-check on other assessment methods. I suspect that if we begin to do so routinely and critically, without pretending that the tagging data are just complementing or fine-tuning other methods that are basically sound in the first place, we will find those other methods to be even worse than has been revealed by severe collapses like the Newfoundland cod. We will begin to replace them entirely.

CAPABILITY IS EMERGING TO MOVE BEYOND SINGLE-SPECIES ASSESSMENT

Less than 10 yrs ago, Hilborn and C.W. said, "We believe the food web modeling approach is hopeless as an aid to formulating management advice; the number of parameters and assumptions required are enormous." (Hilborn and Walters, 1992: 448). This is a really good example of how stupid it is for scientists to make negative claims about potential technologies, a fisheries-science equivalent of 'man can't fly'. We are making some really exciting advances today in fisheries ecosystem modeling (Whipple et al., 2000), by using tools like multispecies virtual population analysis and Ecopath to synthesize large bodies of accumulated data on trophic interactions and feeding these syntheses into models ranging from simple biomass-dynamics equations (see, e.g., Collie and DeLong, 1999) to more complex food-web simulators (see, e.g., Walters et al., 2000) to very complex spatial models (Ault et al., 1999). We are also beginning to test and estimate parameters for these models using time-series data, employing the same statistical machinery for large model estimation and error analysis that has become prevalent in single-species assessment. For example, Ecosim II (Walters et al., 2000) is helping us reconstruct historical changes since 1950 in abundances and mortality rates for a wide variety of species in the Georgia Strait, British Columbia, and to interpret alarming declines in some important species as most probably due to a combination of fishing, marine productivity, and hatchery stocking changes (Fig. 1).

Some enthusiasts of ecosystem modeling see this emerging capability as supplanting or replacing traditional stock assessment, by basing assessment predictions more firmly on the trophic processes that lead to single-species production. This is actually a silly reason for moving to ecosystem models; single-species models already incorporate average effects of trophic interactions implicitly, in terms of mortality, growth, and recruitment-compensation parameters. We have not yet found that embedding single-species dynamics in ecosystem models leads to much better single-species predictions or to much improved ability to interpret 'nonstationary' patterns in mortality and recruitment, except in a few cases like Fig. 1, where there have been really gross, persistent changes not expected from single-species modeling (Walters and Kitchell, 2001). Indeed, for many single-species assessments, we should probably be moving in exactly the opposite direction, toward simpler models where the calculations and estimation procedures are developed on spreadsheets for ease in understanding, error checking, and critical review.

A much more important reason for investment in ecosystem modeling exists, however: single-species approaches do not even allow us to ask many of the important policy ques-
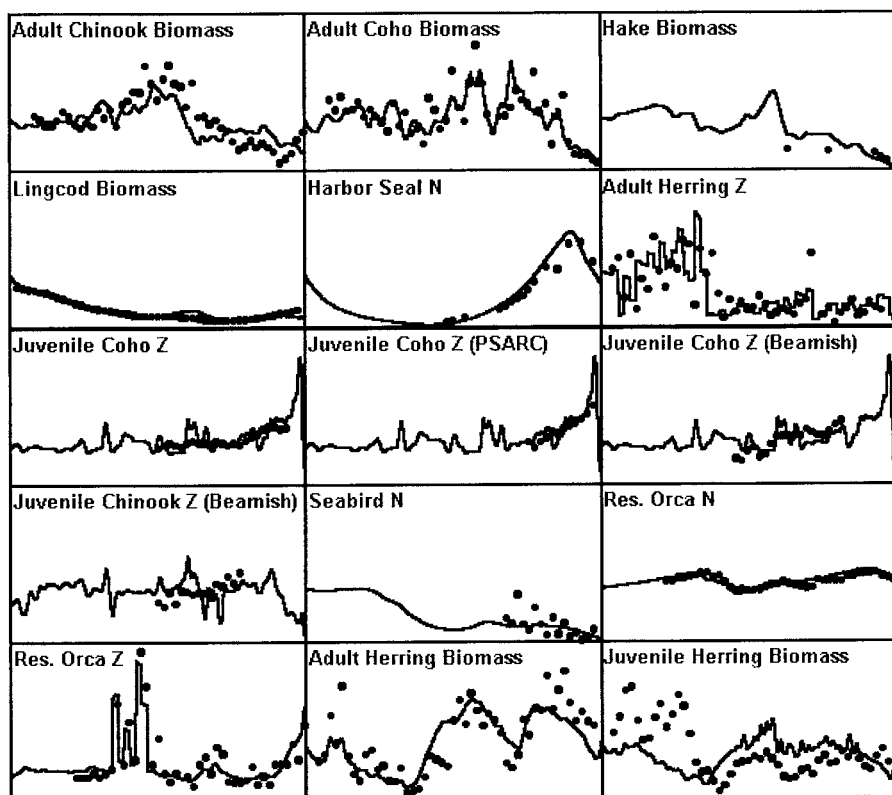
Figure 1. A tantalizing look at some preliminary results from fitting Ecosim II to various long-term data sets from the Georgia Strait, British Columbia. Predicted biomasses from a mixture of linked biomass dynamics and delay-difference age-structured models. Data include relative abundance indices, biomass estimates from surveys and single-species assessments, and estimates of total mortality rates (Z) from tag-recovery rates (salmon), age-composition changes (herring), and even direct observation of disappearances of individuals (orca). Fitting procedure attributes widespread declines in recent years to a combination of declining primary production, overfishing, and increasing competition from hatchery-reared salmon.

tions that are being directed to fisheries scientists and administrators today. These are 'ecosystem-management' questions, like (1) Will changes in primary productivity associated with physical regime shifts driven by climate change be amplified or dampened by food web interactions? (2) Are fisheries seriously affecting the capacity of marine ecosystems to support 'charismatic megafauna', particularly marine mammals, and how can these effects be mitigated? (3) Is overfishing leading to 'perverse' changes in community structure (competitors/predators) that will cause apparent and persistent depensatory recruitment patterns in naturally dominant fish species that have been severely reduced in abundance? (4) How large do marine protected areas need to be to maintain the full structure of marine food webs, and how do food-web interactions affect the performance of MPAs? (5) Will selective fishing practices like by-catch-reduction devices actually help to restore marine community structure, or will they 'backfire' by causing even more severe distortions in food web structure? (6) How much impact will our tendency to fish down marine food webs, shifting more toward harvesting at lower trophic levels, have on

our ability to harvest sustainably and restore abundances of predatory fishes and mammals? (7) How will the physical habitat and epifaunal community changes caused by some fishing practices affect future recruitment and productivity of valued species?

We will probably not be able to answer any of these questions with much quantitative precision by using models like Ecosim, even when we get these models to fit past experience well as in Figure 1. Some real surprises will doubtless arise when we begin to test predictions for questions like (4) and (5) using (even well-planned) adaptive management experiments, but at least we can now claim the ability to make clear predictions based on considerable historical data, a good starting point for designing sound management experiments.

Ecosystem-scale assessment methods and modeling will further increase demands for monitoring and measurement by agencies that are already hard pressed to gather even the data needed for traditional single-species assessments. These demands make it imperative that the data gathered for routine assessment and harvest management be the most informative possible. This is further justification for seeking efficient methods for direct assessment of fishing mortality rates by using methods like tagging experiments. We suspect that we will often find it to be much cheaper and safer to develop ecosystem-scale tagging methods for overall exploitation-impact assessment than to develop ecosystem-scale survey and trend-assessment methods.

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