FURTHER COMMENTS RELATED TO FISHERY "REFERENCE POINTS"/MANAGEMENT

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Cc: michael sissenwine <m.sissenwine@gmail.com>
Subject: RE: [EXTERNAL] RE: Concerns about Bayesian production model assessments being presented - some comments by Mike Sissenwine

Hi all

I have some perspectives on the issues of priors and grids that I won´t get into here. I think this discussion on decisions based on probability might make an interesting paper as a set of case studies.

For me, the most illustrative example of the problems that Doug mentions regarding decisions based on probability was my experience as co-chair of the International Pacific Hake Technical Committee. I think that it illustrates both the technical but also the sociological problems within the fisheries science and fisheries management community that I see elsewhere including with tunas.

To set the context it´s important to understand a bit of the background. Pacific hake is large fishery of 300,000-400,000 tons extending from California to Alaska. It is currently managed by a bilateral treaty between Canada and the US; the treaty defines a default 40:10 harvest control rule and the proportion of the total TAC shared between the two counties. It has all the things tuna fisheries would wish to have but don´t (a biennial acoustic survey, annual proportions at age from age composition information). Because the assessment's estimate of biomass was translated directly into the quota through the harvest control rule in the treaty, Canada and the US fought bitterly over the assessment model from 2004-2011 (see the Figure at the end of this document). In 2010, there was a massive stock assessment error so that when the harvest control rule was applied it predicted the highest quota ever of about 650,000 tons. Luckily only about 250,000 tons of this quota was taken (for a variety of reasons) because in 2011, the survey measured the lowest coastwide biomass ever recorded so that

the 2011 assessment estimated that if quota had been taken it would have removed most of the spawning stock biomass. The crisis in 2011 (plus the coincident desire for MSC certification) led to a set of MSE simulations that illustrated how a few different MPs (for example catch caps plus the existing 40:10 rule) would perform (higher catch, lower AAV, and higher conservation performance). But to my knowledge no MP has been adopted for this fishery. The default process is the best assessment paradigm, with an annual haggle over the quota.

What´s the problem with decisions based on probability in this fishery? Consider the Figure at the end of this document: the shaded interval represents the 95% credibility interval as sampled from the marginal posterior density of the derived parameters in the 2021 assessment. However, as can be plainly seen the probability interval does not even capture the range of median historical biomass trajectories (the other lines) of median historical models - not to mention that nasty pattern of retrospective errors.

Did MSE help? Yes and no. Yes - in that the simulations illustrated that the fishery should expect to see such errors in the assessment model that could result for example in several consecutive years of assessment model errors. Yes also - in that it helped us scientist justify assessment model choices in terms of management performance. But MSE did not result in the eventual adoption of a management procedure. Indeed, while stakeholders liked the MSE in terms of ensuring that the fishery was MSC certified, stakeholders and managers were not keen to adopt an MP.

Why doesn´t the process change? The existing best assessment paradigm is obviously deeply flawed. It is evident that no best assessment was been all that good even in spite of the money spent on the coastwide survey and annual blood letting in the assessment process. In general terms, the problem is more sociological: one of the main problems is that within this community (and others) of stakeholders and managers, fisheries science has lost credibility. Would you trust an assessment given the illustrative retrospective errors? This loss of credibility extends into the MSE process too: would you trust the results of simulation supporting management procedure choice from same community of people that brough you the last 30 years of retrospective error? But as David alluded to, the problem is sociological too, in the sense that the stakeholders and managers (the Advisory Panel and the Joint Management Committee in this fishery) are accustomed to the annual haggle over quota and are distrustful of the MSE to the same degree as they are distrustful of the assessment.

My two cents, albeit lengthy. I think a good think on what to do about is warranted and here too I have some ideas.

NG

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Cc: michael sissenwine <m.sissenwine@gmail.com>
Subject: RE: [EXTERNAL] RE: Concerns about Bayesian production model assessments being presented - some comments by Mike Sissenwine

All,

A very important debate. Thanks Doug and Mike for your observations. I acknowledge that you both raise some important points, however, it will take me some time to digest some of your proposals that suggest that because medians and point estimates are better estimated than the probability distribution around them it would be best to base the advice on those central tendency statistics rather than on decisions based on perceptions of probability.

I think that MSE could shed some light on this by potentially testing CMPs that are not based on probabilities.

I am interested, however, in discussing a related challenge, one that is probably more in the realm of social fishery science than in the realm of ecological fishery science. How is advice provided in the two different ways we are discussing: advice based on point estimates or advice based on probabilities, processed by managers and policy makers? Which type of advice helps them make and better decisions and helps them implement more effective management? There must be information about this within the realm of management science.

David

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Subject: [EXTERNAL] RE: Concerns about Bayesian production model assessments being presented - some comments by Mike Sissenwine

Hi all

Some comments from Mike Sissenwine on my email of yesterday may be of interest.

Cheers

Doug

I think your message is another example of a broader problem of too much inertia in the stock assessment/fishery management system, which gives the impression that we can predict how to achieve targets (rather than iterative approaches to move in a desired direction i.e., typically the nature of empirical management procedures resulting from MSE).

How did we get here? Scientists indoctrinated managers, politicians and lawyers on today's highly demanding approaches. Now scientists are victims of unrealistically demands for predictions and targets, and too many scientists continue to do what they know how to do (inertia) in response to expectations scientists created. Even when they have doubts, they feel obligated to provide the advice managers need. Some of us gripe about these demands, but we seldom acknowledge that they were our idea, and we need to do more to re-engineer the system so we can offer a more robust alternative. MSE is part of the re-engineering, but it is only one stage. One of my concerns is that unrealistic expectations will ultimately discredit the entire process/system.

Doug, I am particularly sympathetic to your concern about probabilistic management decisions for two reasons. First, the probabilities that are selected almost never have a

scientific basis (no one conducts a risk assessment with a specified loss function). Why is 60% better and 70%? The second reason is that probabilities are so poorly estimated, often sensitive to model assumptions that are not addressed by the estimates. This problem is even more severe for the tails of distributions (can we really distinguish between a 5 and 10% probability?). When the p* approach (e.g., TAC corresponding to probability of overfishing less than 25%) was popular in the US, I convinced the New England SSC to reject it because probabilities were so poorly estimated. I thought it was more robust to base TAC advice on a point estimate of a more conservative reference point- 0.75 FMSY). Unfortunately, I have yet to see a critical evaluation of these two alternative approaches for taking account of stock assessment uncertainty, more than a decade since this issue was debated in NE!

Emeritus Professor Doug S Butterworth

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Subject: Concerns about Bayesian production model assessments being presented

Hi all

Earlier in the week I raised concerns about the Bayesian production model assessments of swordfish being driven near entirely by the prior for r, which was hardly (particularly for the south) updated by the data. Later similar results were evident for porbeagle. Today, though it is difficult check this in the report as some key figures one would wish to inspect have not been shown, I have the impression that the same one-way-trip problem applies for the two skipjack stocks, with priors for r little updated – though there is a further problem there as some posteriors for r do show highish densities at larger values for which the prior densities are virtually zero, which is pointing to another problem with these priors which are so influential on the results and advice.

Clearly nothing can be done now, but these features of results raise issues which I suggest should be high priority topics for the ICCAT methods WG to discuss:

1) Obligatory diagnostics for Bayesian assessments, especially where abundance indices are of the one-way-trip type, where priors for r (or its equivalent) are not greatly updated by the assessment (because there happens to be little information on stock productivity in the other information available). There are some suggestions for these below. It is important to understand that in such circumstances aspects such as TAC advice are then being driven almost entirely by the prior chosen for r, so that the assessment report must look carefully and comment upon sensitivities to this choice.

2) Consider different approaches in such circumstances. For example, use assessments with different fixed input values for r, and determine which values correspond to the difference between whether or not overfishing is occurring, and whether or not the stock is overfished. Then see whether the OTHER information available is able to determine on which sides of these threshold values the actual r value is.

3) The JABBA approach converts biological parameters and their uncertainty into a prior for r, and then moves to use a Pella-Tomlinson (or Schaefer) model for the dynamics. An alternative is to use an age-structured production model (recall Hilborn pointing this out in the mid-90s, when computers had progressed to the stage where such computations were readily handled). With the r-prior frequently playing such an important role in determining the TAC advice, this would have the advantage in clarifying directly which of the biological parameters (with their associated uncertainties, and now each as separate priors) is having the greatest impact on management advice and hence requires the greatest scrutiny.

Looking further still, I wonder also whether the current Kobe probability matrix provides the best process to provide a basis for management advice. The problem is the robustness of probability distribution estimation. This is a problem even for best-assessment paradigm assessments. The projected uncertainty typically takes estimation uncertainty and future recruitment variability into account, and in ways which are reasonably straightforwardly standardised across stocks. But what about other uncertainties – e.g. the value used for M – are results to be integrated over a distribution of values for M? – but then the Kobe matrix and recommendations to meet, say PGK= 60%, will change.

It is these considerations which has seen CCSBT move away (if not entirely) from decisions based on probabilities, because of their lack of robustness to what is and isn't taken into account in the probability calculations. Instead, decisions are driven more by the MEDIANS of the distributions forthcoming from assessments, which are much more robustly determined in these circumstances provided (as should be the case) assessment input choices seek balance (between more optimistic and more pessimistic).

Thus if more safety is wanted, rather than asking for 60% instead of 50% (median) probability of achieving, say, Bmsy by some future date, or similarly being 60% certain of not exceeding F0.1, one might give advice based on the median attaining 1.1*Bmsy, or fishing mortality

being at 0.9*F0.1. Also then, advice is given in terms which are much more readily understood by stakeholders than expecting them to fully grasp probability concepts.

Cheers

Doug

Emeritus Professor Doug S Butterworth

The **Figure** below is that referenced in Nathan Taylor's comments that start this document. It shows the varying estimates over time of historical spawning biomass for Pacific hake from annual assessments. The shaded interval shows the 95% credibility interval as sampled from the marginal posterior density of the derived parameters in the 2021 assessment.