

Dear Bill,

Many thanks for your constructive and informative September 3 response to my August 16 e-mail on the “yellowtail” meeting.

I hope our dialogue is continued in a broader context. Your idea of a blue-ribbon panel makes sense, particularly inasmuch as detailed discussion is difficult to cover by correspondence alone.

Based on our correspondence, I wanted to call your attention to three issues: 1) production models, 2) alternative values of  $x$ , and 3) scientific and legal overreach.

**Production Models**—The response to Q2 says, “*surplus production models have much less realism, and much stronger assumptions about the dynamics of the population*”.

This response is not in a direction that I would expect. It is not clear at all that the  $F_{40\%}$  (and its attendant VPA) is more realistic or based on stronger assumptions than the production model. What is the metric for *realism*? What is the metric for “*strength of an assumption*?” Our concern should not focus on good or bad properties of a surplus production model, but how a surplus production model in a particular assessment setting compares with an alternative model.

Conventional scientific approaches for choosing among models, such as explanatory power, parsimony, and variance, rather than discussing the choice of models, seems relegated to arguable criteria. While there are certainly problematic issues that involve surplus production models (e.g., biased parameters and MSY equal to long term average catch), there are as many issues associated with the  $F_{40\%}$  and its attendant VPA. Consider, for example: extensive retrospective patterns, an equilibrium requirement, reliance on stock-and-recruitment assumptions, and use of fully recruited  $F$ , to name a few. For every production model criticism, one can level a criticism of seemingly equivalent import at the  $F_{40\%}$  approach. I really do not know what would be left standing. This is why there is more merit in discussing these issues on a case-by-case, stock-by-stock basis rather than in the abstract.

Regarding the last paragraph, in response to Q2, it would indeed be desirable to have a detailed analysis of underreporting, discards, and environmental predictors of natural mortality so that these properties could be explicitly included in the assessment result. On the other hand, wouldn't it be equally desirable to use stock assessment approaches that did not rely on these difficult-to-measure properties?

**Alternative Values of  $x$** —Now turning to the alternatives associated with the  $F_{x\%}$  approach, we have pointed out in extensive discussions (see references) that the selection of  $x$  and  $M$  to obtain  $F_{x\%}$  is arbitrary. If we use the  $F_{x\%}$  approach, we can arrive at almost any value of  $F_{x\%}$ , depending on our choice of  $x$  and  $M$ .

As you know, the Magnuson-Stevens Act tells us to focus on MSY. Because of this, it would seem appropriate to calculate MSY. The production model is a traditional technique for calculating MSY. So why don't we at least present the MSY calculations to the Council? Clark advocates the  $F_{x\%}$  technique for cases where MSY cannot be computed. However, MSY can

generally be computed. So why is the proxy used instead of MSY? Folks need to understand the answer to this question. What is the answer?

By the way, the fact that  $x$  and  $M$  are arbitrarily selected is not necessarily a problem. However, it is definitely a problem when the choices are not analyzed, explained, or justified.

**Extending Scientific and Legal Reach**—It appears that your response attempts to justify the exclusion of alternative models (not presenting production models and considering only  $x=40\%$ ) by scientific and legal assertions that have not been fully vetted.

Consider the statement, “*...reducing the %MSP would allow us to reclassify an overfished stock as no longer overfished. However, this would not necessarily mean that the stock was in better shape, or that the risks to the stock had been reduced*”.

What is this statement supposed to mean? Does this mean that the goal of eliminating overfishing should be replaced by goals that relate to the “shape” [of] and the “risks” to the stock? How are “shape” and “risks” measured?

Consider other statements:

1) “*...stocks with lower target biomasses will be more sensitive to incoming recruitment because recruitment will constitute a larger fraction of current population size...smaller stocks often oscillate at higher frequencies than larger stocks*”.

What is the evidence for these conjectures? Should we be using hypothetical notions in fisheries management?

There is more. For example, “*...higher  $F$ 's could exceed  $F_{max}$ ...*”. This is true, but any excess could easily be inconsequential.

And more, “*...overall yields from a cohort could increase by reducing  $F$ ...*”. I thought this was a good idea. The discussion of economics is a stretch.

And more, “*There may well be alternatives that perform better than  $F_{40\%}$ , but a simple increase in  $F$  from setting a low %MSP target is unlikely to achieve the desired properties of the fishery. It would be unwise to jump to a new value just because it gives a more optimistic picture of stock status. As noted above, the biological attributes of the stock under higher  $F$ 's and the consequences for harvest variability need to be considered.*”

Regarding the above statement, are we adopting a poor performing management measure over a good performing management measure for reasons that are hypothetical and not scientifically or legally justifiable? Just what are “desired properties of the fishery...”? And how are they measured? What is meant by “unwise,” and how is it measured?

**Conclusion**—Thanks to you and your staff for putting together a constructive response. As you can see, there is a rich difference in viewpoints. The extent of the differences, at least from my point of view, have not been fully explored (for example, I have not discussed the management

response to quadrupling the natural mortality of Georges Bank cod, the analysis of tagging data, or the approaches used in the paper appended to your response). Some of the issues relate to disagreement on technical matters. But other issues relate to how scientific issues are packaged —are the alternatives presented and fully discussed so Council members can fully appreciate them? It certainly appears that there are distinctions between New England stock assessments and more traditional approaches. These distinctions appear to be supported by conjectures that really have not been fully vetted. Should we be making management decisions on conjectures?

As I said in my opening paragraph, a panel discussion of these issues continues to be worthwhile.

Please feel free to distribute my response. I will share it with my colleagues on the MFI and others.

Cordially,  
Brian

## REFERENCES

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