Discussion

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I was very much impressed by this paper. Not being a fisheries' scientist myself, I am not really able to judge the biological relevance and accuracy of the modelling assumptions that are made. But the message that I get from reading this paper is that its authors know very well both the fisheries' management problems that are addressed here, and the two species of fish they consider. And not only that: they are also very competent statisticians, who use skillfully just about the entire statistical repertoire that is available, including two different inferential paradigms, to come up with practical solutions.

On the other hand, I feel rather puzzled about how much, or perhaps rather, little, can actually be accomplished even after a considerable effort has been made to collect both catch and survey data. Even if catch at age data are assumed to be almost without error, it is very difficult to make use of such information in the "inverse problem" of estimating the total stock size in different age groups. Unless the stock has actually already collapsed or is on the verge of doing so, the total catch from recent previous years will nearly coincide with the quotas that were allowed. Therefore, the potential value of the catch information is in how, during consecutive years, the catch is decomposed into the different age classes. But the conclusions that can be drawn from this information are very sensitive to what is assumed about natural mortality in these age classes. Apparently, as is also recognized by Gavaris and Ianelli, natural mortality can be almost anything in the younger age classes that are not being fished as long as the recruitment to age class 4 remains approximately the same. Roughly speaking, from the catch at age data we can get some idea about the total mortality $F_{a,y} + M_{a,y}$ in different age groups a and years y, but there is little information about how such numbers should be split into fishing and natural mortality. But before this is done, it is impossible to convert catch information $C_{a,y}$ into age structured population sizes $N_{a,y}$.

Given that this is so, it is certainly a natural thought to assume that $M_{a,y}$ is a constant across all values of a and y. But then it is actually quite unnerving to see (from Fig. 8) how extremely sensitive the results obtained from such analysis can be to superficially innocent looking assumptions: the other extreme being the similarly innocent looking assumption that the catchability parameter q in the surveys is a constant across older ages. (Such an extreme degree of sensitivity to the assumptions that are made is not least troubling because, in an actual bargaining situation regarding next year's quotas, it allows the various interest groups to reason backwards and choose the assumptions that seem most apt to their desired goals.)

This high degree of uncertainty is reflected also in the two case studies which Gavaris and Ianelli consider in their paper. Rather than just providing point estimates and purporting them as "objective answers obtained by scientific methods", the honest approach to fish stock assessment problem is to acknowledge the large amount of uncertainty which remains, and to try to quantify it. Since fish populations may take a very long time to recover if they collapse, over-exploitation of the resource is a real danger and should not really be to anybody's advantage, including the fishermen who have a long term interest in the viability of their trade.

With my own personal biases to modelling and statistical inference, I cannot resist the temptation to comment a little on the use of the deterministic, classical (frequentist), and the Bayesian approaches to these problems. Apparently, the historically older deterministic models such as VPA are found attractive by many because they are conceptually so easy and mathematically simple. But I doubt that anybody seriously believes that the stock sizes and fishing mortalities computed from such models, on the basis of age class data and after having assigned the natural mortality a fixed value, are of much practical value. Indeed, as Gavaris and Ianelli remark, according to the VPA model it is only necessary to fix for each year class the population size $N_{a,y}$ and the fishing mortality $F_{a,y}$ for a single year y, and the values for all the remaining years will be determined uniquely. In a way, this is too good to be true!

I think that there is a genuine dilemma here: on the one hand, it would be desirable to have as few parameters as possible, because only then is there any hope of being able to "identify" their values from the data. On the other hand, however, models with only few parameters seem to be too "stiff" to really give an adequate description of the degree of complexity and the amount of ecological variation, which are inevitably present in natural fish populations. Although I don't think that there are easy solutions to this problem, I would still think that one should give up using deterministic or nearly deterministic models with only a few parameters, but systematically adopt hierarchical Bayesian models with informative priors. From my perspective, for example, assuming that the natural mortality parameter $M_{a,y}$ is a constant for all values of a and y, let alone giving it a fixed value, imposes a far stronger subjective element on the statistical inference than if it were assumed that $M_{a,y}$ have the form of a product, with one factor depending on a and the other on y, and where both are given informative priors. Fishing mortalities could be assumed to have a similar multiplicative structure, but the factor depending on y being modelled explicitly so that also the total effort spent on fishing during that year would be somehow reflected. Similarly, one could add at least "one layer of stochasticity" to the various abundance index measurement models, accounting for the fact that the fish are not spatially uniformly distributed, and that they actually move from one location to another. I should think that such explicit allowance of ecological variation would provide a more systematic way of handling the sometimes large residuals, than, for example, the least squares method.

Against this background, I am a bit puzzled why Gavaris and Ianelli are so cautious when they comment even on their own use of Bayesian methods. For an outsider like myself, fisheries' management problems would seem a perfect case to apply the Bayesian approach to inference and decision making: they involve a great deal of uncertainty, and the analyst is trying to make best possible use of different bits and pieces of data that may be available, combining this with expert judgement. Finally, there is the concrete goal of deciding on next year's quotas, making the whole exercise an optimal decision problem. I appreciate Gavaris' and Ianelli's careful study using the bootstrap/Monte Carlo techniques, but I cannot really see why they have often preferred, as they call them, *pseudo* Bayes procedures. I find it actually quite difficult to understand what is the probability underlying the empirical frequency distribution of the interest parameter that they obtain from their Monte Carlo simulation. Why not make a head-on attack? The real goal, I believe, is to say something meaningful about how large the present stock is in each age class, and how large it will be next year, after the fishing season is over, if the quota is set at some particular value. The former problem concerns state estimation, and the latter prediction. Since providing exact numbers is not feasible, probabilistic statements based on both empirical data and expert judgement seem to me to be the logical choice for communicating the result of the statistical analysis to the actual decision makers.

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