



agriculture, forestry & fisheries

Department:
Agriculture, Forestry and Fisheries
REPUBLIC OF SOUTH AFRICA

Branch: Fisheries

Private Bag X2, Rogge Bay 8012, Foretrust House, Martin Hammerschlag Way, Foreshore, Cape Town, 8001
Tel : +(27 21) 40213176 Fax: +(27 21) 4023639 E-mail: JanetC@daff.gov.za

Dear Dr Hoggarth

Re: MSC Consultation on Low Trophic Level Fisheries

Thank you for providing an opportunity for those stakeholders who responded to the original consultation to provide any further feedback relating to the Smith *et al.* paper and the extent to which it supports or changes the comments we previously submitted.

In that context, our earlier related comments were as follows:

1) *From the summary provided by the MSC on the "Impacts of fishing on low trophic level species", and also the report of their 24-25 March 2010 workshop in Seattle on this topic, it appears that inferences about impacts of such fishing on natural predators are based on modelling studies only. Before this 75%B₀ target figure, and the 40% figure that is suggested as an appropriate threshold for the extent of impact on a natural predator population, might be accepted:*

a) empirical evidence as well as broad modelling studies need to be presented demonstrating impacts at this level on predators for such a target, and

b) justifying the conclusion that predator reductions of such a magnitude have led to a deleterious effects on ecosystems.

[Requirement a) might have effectively been met if model parameters have been estimated through fitting to time series of abundances of predators and prey in the studies conducted, but the summary does not make clear whether that has been the case.] More importantly though, whether a particular impact is undesirable or deleterious would seem to be a value judgment, and a justification for the selection of 40% as an appropriate threshold on which to base such a judgment needs to be presented. It seems questionable that this 40% figure is firmly proposed in circumstances where the MSC's Seattle workshop report states that the modelling team pointed out "that this was no more than an arbitrary cut-off and that it did not necessarily reflect a biologically significant impact", and that the workshop agreed "that a generic and biologically defensible cut-off point did not exist"

and

II) *The key feature of most LTL species compared to non-LTL species is their greater fluctuations in abundance as a result of environmentally-induced fluctuations in annual recruitment whose impact on relative abundance is amplified as a result of the short lifespan typical of such species.*

In summary, and without detracting from the valuable contribution which the Smith *et al.* paper makes to the topic in question, including strengthening the strategic basis for higher target levels for such fisheries in comparison to higher trophic level fisheries, in the context of the proposal for a 75%B₀ target figure for LTL fisheries as a threshold for certification, consideration of the material in the paper now available to us **exacerbates our concerns and strengthens our view that these specifics of this MSC proposal are premature**, for the reasons following.

1) We note that on the third page of the Smith *et al.* paper its authors state: "For this reason we do not consider that these models should be used to provide tactical management decisions." Yet it seems to us that in advocating a choice of a 75%B₀ target based on the results provided in this paper, the MSC is in direct contradiction of this by putting forward a specific tactical decision concerning threshold choice.

2) We originally asked if the parameters of the models considered "*have been estimated through fitting to time series of abundances of predators and prey in the studies conducted*".

a) The paper comments: "*Each of the models has been validated against time series data from well studied systems*". Although these models may indeed all have had some of their parameters estimated by fitting to such data, the reliability of any inferences to be drawn from the models are dependent on the quality of these fits - the extent to which they at least approach the customary standards required for diagnostic tests for single species model fits that are used in providing fisheries management advice. This we have not been placed in a position to be able to judge adequately. In the short time available we have been able to access publications referenced for only two of the examples considered by Smith *et al.* The one shows no fits of the model to such data, and the other shows only some, a number of which could hardly be considered acceptable under the norms above. In asking consultees to comment on these findings, the MSC should minimally provide all such fits for all the models considered, to allow an overall appraisal of the acceptability of these model fits for the purpose for which MSC wishes to use their results.

b) It is important in framing an assessment of the reliability of the models used to have some idea of the level of rigorous evaluation to which they have been subjected by local scientists responsible for providing fisheries management advice in the regions in question. To indicate towards that end, can we be advised of the extent to which any are taken into account directly in the provision of tactical scientific advice for fisheries controls (catch, effort limitations etc.) in these regions? The sense of this question is not whether any of the models themselves are used to provide such advice directly (we suspect not), but rather whether any have been used in making the choice for overall controls such as medium term biomass target levels.

c) A similar recent model to those considered in the paper - an EwE-like based analysis of the Gulf of Alaska ecosystem: Sarah K Gaichas, Kerim Y Aydin and Robert C Francis: What

drives dynamics in the Gulf of Alaska? Integrating hypotheses of species, fishing and climate relationships using ecosystem modeling (CJFAS, accepted) - has similar difficulties in finding acceptable fits to all the time series of data to which it is fitted. When questioned about this and its implications for the reliability of more tactical inferences from the model following a recent presentation, Aydin responded that it was unrealistic to expect acceptable fits to all series, but that where inferences about specific species were required, it was necessary to fit the model giving much higher weight to series associated with that and closely related species (in the food web) at the expense of other species and the quality of fits to time series for them. This suggests that single overall fits of ecosystem models should not be relied upon for the exercises attempted by studies such as Smith *et al.*, but rather that a series of such fits should be examined, each focussing in turn on fitting more closely to the data for the species (and ones closely related) that are the focus of the study. If this has not yet been done for the Smith *et al.* study, we suggest that it needs to be considered as part of further robustness testing.

d) The Smith *et al.* study most appropriately considers the robustness of results to applications of structurally different ecosystem models to the same ecosystem. In the broadest of terms, results are indeed similar, but at the next level of scrutiny some major differences are apparent. For example for the Southern Benguela, the estimated impact of the depletion of anchovy is substantially different for the EwE and OSMOSE models (Fig. 2 of Smith *et al.*). This must raise serious questions if it is the results of such modelling, as at present, that were to provide, even if in somewhat more of a strategic than a tactical sense, the basis of the MSC's threshold target requirement for certifying the South African fishery on anchovy, were this to be requested for consideration.

3) Empirical evidence of extent of impact [Note our original request that "*empirical evidence as well as broad modelling studies needs to be presented demonstrating impacts at this level on predators for such a (75%B₀) target*"]

a) Convincing evidence to support the extent of impact on predators suggested by these ecosystem models would come most powerfully from the models' fits to the time series data for the prey - prey fishery - predator triplets under particular consideration. Disappointingly however, the "Empirical support" section of the on-line supporting material for the Smith *et al.* paper appears not to offer such but rather primarily qualitative correlations, implying by omission that there is not actual evidence to back the **quantitative** extent of the impacts predicted by these models - is that correct?

b) Examples of support offered can be open to alternative explanation, such as predator reductions reflecting a temporary distributional shift to another area offering improved feeding conditions at the time, rather than that a reduction of predator abundance caused reduction in biomass of a LTL species as the ecosystem models used to project effects are implying. For example, the supporting material for the North Sea models cites broad agreement of EwE model predictions and empirical data that minke whales have declined (would decline?) by more than 40% in response to a 60% reduction in sand eels. However, Norwegian cetacean sightings survey results (Nils Oien, *pers. comm*) do not indicate any decline in the North-east Atlantic minke whale population in question, though the proportion of these whales in the North Sea does vary over time.

4) As pointed out in our original submission, the choice of 40% as the threshold for impact on a predator in the evaluations of impact that lead to the 75%B₀ target recommendation is essentially arbitrary. It is unfortunate that the Smith *et al.* contribution shows results for this choice only, and gives no basis for the choice other than referring to it as reflecting a "severe" impact. Other choices could be defended, and one would then want to see what corresponding LTLF depletion target would be implied.

5) Surplus production function (Fig. 4 of Smith *et al.* Implies a loss of only 20% of yield in targeting 75%B₀ instead of B_{msy})

a) It is the squarish shape of this surplus production function in Fig. 4 that leads to this conclusion of a lesser loss than might normally be expected. No details are given of the calculations that led to this curve, and the question is begged of what particular aspect (input, assumption, ...) of the models examined is leading to this - how robust is this result? Note also that the argument of lesser fishing costs at a higher abundance would not apply in many LTLFs where shoaling behaviour leads to highly non-linear catch-rate vs abundance relationships.

b) For most of the models considered in the Smith *et al.* paper, the impacts on predators of fishing the LTL species were calculated under deterministic assumptions. This might be expected to over-estimate those impacts, as LTL species frequently fluctuate appreciably in abundance at a time scale too short to allow the larger predators with their typically slower dynamics to take full advantage of upward fluctuations in the abundance of their prey. Thus these predators are unable to grow to the level that deterministic analyses would suggest in the absence of fishing, and hence would likely not be impacted as greatly by fishing on their prey.

6) We suspect that there may be problems with the manner in which recruitment fluctuations in LTF species are being generated in some of these models, which could lead to misrepresentative results. Because of the shortness of time afforded for these comments, we have not been able to check this sufficiently. We would not normally comment before completing such checks, but since we would wish to do so to you if these suspicions are confirmed in due course, we mention this now so that our motivation in such possible further submission later is not misunderstood.

In conclusion we consider the Smith *et al.* paper a valuable first step, but certainly not sufficient as a basis for the decisions on target thresholds for certification which the MSC is proposing. The next step should be the organising of a vigorous and wide-ranging review of this work, leading to suggestions to take it further towards the stage where it might provide such sufficiency. The level of rigour for which we are asking may seem high, but the MSC is suggesting this work to be the key basis for novel criteria for LTL fisheries management. If applied and followed, these criteria would quite likely have very heavy negative socio-economic impacts, while the associated predictions of subsequent gains have limited reliability given the current state of development of the field. Decisions should not be taken before the underlying science has been subject to much more careful scrutiny than might more customarily apply, i.e. they need to be taken very carefully and not rushed.

Please note our apologies to the authors of Smith *et al.* in case some of our comments above, made in haste, reflect a misunderstanding of their work, but this was unavoidable in view of the shortness of the period we were given for comment.

Yours sincerely,

Janet Coetzee
Chair: Small Pelagic Scientific Working Group