

24 April 2003

Dr. R.A. Myers  
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Dear Dr. Myers:

Subject: J17408 -- Extirpation of a major Atlantic bluefin tuna (*Thunnus thynnus thynnus*) population?

We have received two reviews that are highly critical of your recent manuscript. Both referees provide you with extensive comments about the points that concern them. There are clearly differences of opinion on the value of some of the data, but the essence of both their criticisms is that you have not provided a sufficiently robust analysis to support your conclusions. We must admit that the style of your manuscript struck us as out of the ordinary in contrast to the work with which we are most familiar.

The subject matter is clearly important and of interest to the readers of CJFAS. However, in order for us to reconsider your manuscript you will have to provide a more quantitative analysis to support your assertions. Part of the problem in providing a convincing argument may have been due to an attempt to satisfy the requirements of the Rapid Communication format for CJFAS. Our advice is that you should attempt to employ the format of a regular article for a more thorough manuscript. A thorough, robust manuscript will have the impact it deserves no matter which part of the Journal it appears in.

At this time, we must decline your manuscript, but we invite you to resubmit a new, revised work that takes into consideration the comments of both referees. Given the extensive comments dealing with the essential substance of your paper, the revised work will have to be sent out for further review. We ask that you provide a detailed reply to the referees to aid us in the evaluation of your new manuscript.

We are sorry we could not bring you better news at this time, but we hope to see a new submission from you in the near future that addresses the referees' concerns.

Yours sincerely,

Moira Ferguson  
Senior Editor

Pierre Pepin  
Editor

## **Referee #1**

Review of J17408 by R. Myers and al.

### ***General Opinion***

This “rapid communication” is an easy to read manuscript and it could be an interesting one; it is dealing with a emblematic species, Atlantic bluefin tuna, and with a problem of major interest: why the bluefin fishing zones have been so variable over time. And surprisingly such major changes in the geographical distribution of Atlantic bluefin are well known, but they have been very seldom studied by scientists in a comprehensive way, for instance in relation with environmental changes, with genetic erosion of subpopulations, or behavioral contraction of the fishing zone following the overfishing of the stock. There is no doubt that the hypothesis of vanishing population is an interesting one, which may be a great source of concern for bluefin tuna.

Taking into account these conclusions, it is clear that this paper would be worth publishing in the Canadian Journal. Unfortunately, this document is still severely hampered by various major problems that should be solved or clarified before its publication.

### ***Major Problems***

1) This paper is too much an “opinion paper” based on personal ideas without quantitative use of the data available or results from the best and most recent ICCAT analysis.

2) The elimination of subpopulations is too quickly the explanation kept by the authors: alternate hypotheses that could well explain the changes in the distribution of BFT are too quickly eliminated by the authors. The analysis by Binet 1988 did suggest that the elimination of the North Sea bluefin could be explained by environmental changes; the work by Ravier and Fromentin is well cited in the paper, but its major conclusion that BFT biomass has been showing during centuries large “natural” variations of its biomass (and probably of its geographical distribution?) has not really been kept in the paper. The recent work by Takeuchi et al 1998 discussing the hypothesis that could explain why bluefin disappeared from Brazil was ignored in the paper. The hypothesis that migration routes of bluefin tunas are variable as a function of the environment has been an hypothesis well accepted by most bluefin experts such as Tiews and Mathers. Following the same point, it is well known among many BFT experts that this species can discover during a given year a concentration of preys in a given strata, and that the same group of tunas will come back year after year to feed on these preys. The refreshing hypothesis of a learned feeding behavior, middle of page 8, is probably a very interesting one for bluefin, and it should be briefly reinforced! But the absence of tunas in the area is hardly proof that a fraction of this tuna population has disappeared from the ocean.

3) A great importance has been given to the Uruguay and Argentina area, south of 25°S. It should be kept in mind that this area has never been significant in the bluefin fishery, for instance with only a total of 230 fishes being caught by the Japanese longline fishery during the entire history of the fishery (some of these fishes being possibly southern bluefin, a different species of bluefin that is dominant in the area, several thousands of this southern bluefin being taken during the same period; as these data are log book ones, this potential bias of possible confusion between the 2 bluefin species during the historical period should be at least recognized).

4) In general, the strong conclusion that overfishing is the sole reason for the vanishing populations should at least be based on the best ICCAT data: for instance, the ICCAT data base shows that the level of catches taken by purse seiners that are targeted by the paper as being the best explanation for the vanishing bluefin populations could be plausible, but they should be strictly based on the best ICCAT data. The ICCAT catch data base do indicate that purse seine catches by purse seiners were still moderate, nearly half recent catches, a majority of these catches being large bluefin (and not juveniles) taken on the Norwegian coasts (very far from Uruguay or Brazil).

5) In relation with this point, the paper should also introduce and discuss the fact that, if bluefin vanished from some areas, it also became quite abundant and fished in various new fishing areas; this was for instance the case:

- for the area NW of Bermuda (35-45°, 60-70°W) , south of Newfoundland, successfully fished by Japan longliners when the nearby Bahamas population vanished, in the Northern Atlantic
- For the area between Island and Faeroe Islands actively exploited by longliners since 1994 .
- Are they new populations? Or simply new feeding concentrations discovered and memorized by a fraction of cohort?
- Similar “mystery” of the recent pop up experiment showing that a large fraction of BFT tagged of the east coast are releasing their tags in the central Atlantic outside any known fishing zone, should be incorporated in the Discussion (see the various papers recently published by Lutcavage).

5) Excessive use of recovery data, without taking into account any quantitative interpretation (as done by ICCAT scientists). A single tagged fish moving between 2 areas cannot describe a migration (as it is labeled in figure 2-i), nor can it simply means that there is a single population independent from other groups. For instance figure 2 shows that tag recovery was observed with apparent migration between Florida and Brasil and Uruguay: there was in fact a single fish moving from Florida to Brazil and another one to Uruguay... These 2 isolated recoveries are interesting to note, but not enough to hypothesize a single mixing population.

### ***Minor or technical problems***

Various small additional small problems could also be noted:

1. in the summary, the 3 year period is valid only for some populations, not for the Norwegian one (actively fished during about 40 years: 1937-1977);
2. Concerning the North Sea bluefin, there is also strong evidence in the Nordic literature that if bluefin has been observed and probably fished for centuries in the area, it was highly variable in its abundance and not at all permanent in the area.
3. Bluefin taken off Norway were predominantly large fishes and giants (at the end of the fishery); the Norwegian fishery used to start yearly in July, not in August;
4. Concerning the present absence of bluefin off Brasil, it could be added that the same area is still actively fished by various fleets' longliners: the absence of bluefin in this area is clear!
5. Concerning these Brazilian bluefin, the more recent Japanese paper by Takeuchi et al 1998 indicates that these fishes were more probably immature, and not spawners.

6. The changes in the levels of catch and cpue by the longline fishery should take into account the ICCAT quota in the early Eighties in the West Atlantic: they simply explain the reduced catches.
7. The max abundance in the Brazilian fishery took place in April, but the fishery did showed 2 seasons, a second season in September and October.
8. The relationship between Norwegian and Mediterranean bluefin are quite clear for all the experts; this point should also appear;
9. The conclusions tend to be too pessimistic: bluefin stock is clearly dangerously overfished and facing a potentially dangerous genetic erosion: it is hard for instance to agree that the species is on the way to extinction, when record levels of catches and cpue are still observed in many fisheries (Medit., for instance). In general the conclusions should be adjusted, taking into account the serious problems previously discussed.
10. Some important literature should usefully be added: Binet 1988 ICCAT paper, the paper by Cury et al ICCAT Symposium 1996 that was among to discuss the problem of potential genetic erosion faced by Atlantic Bluefin, the recent paper by M. Lutcavage showing concentrations of bluefin of Bermudas in an unfished area should also be added... Takeuchi et al 1998 paper on Brazilian bluefin should be added. The thesis by Hamre on Norwegian bluefin would be also interesting to keep in mind in the discussion, but keeping in mind that this thesis was written in Norwegian. The recent US NRC report on Atlantic bluefin does also provide interesting factors and discussions concerning the subject. **[Ed. note: maximum number of references for Rapid Communications is 12].**

### **My final recommendation:**

My recommendation, taking into consideration the positive and original points developed by this paper, but also its serious weaknesses, would be: to strongly encourage its quick resubmission, but after a comprehensive revision of the text, especially taking better into account the real trends & facts upon fisheries, and a more quantitative overview of the recovery data, giving a better balance between the new hypothesis of vanishing subpopulations, with a more complex and more realistic vision of bluefin movements, the natural variability of its stock size and geographical distribution as a function of stock size and environmental variability.

## Referee #2

Review of “Extirpation of a major Atlantic bluefin tuna (*Thunnus thynnus thynnus*) population?” by Ransom A. Myers, Brian R. MacKenzie, and Peter Ward

1. The principal points of this paper are that (1) Atlantic bluefin tuna populations are depressed, (2) “exploitation rates should be reduced to promote recovery and prevent extirpation”, and (3) populations are depressed to lower levels than previously recognized. The first two points are hardly news to the audience served by CJFAS. The third point is based on a speculative interpretation of the data presented.
2. The authors dip heavily into the vocabulary of currently fashionable environmental concerns. This usage only serves to detract from the article and is not appropriate for CJFAS. I will cite examples below.
3. The title uses the word “extirpation”; the abstract refers to “virtually eliminated” from half of its range. In the text of the article, half of its range really means the western south Atlantic off the coast of Brazil and Argentina, the northern North Sea, and a small area surrounding the Bahamas.
4. The introduction is laden with platitude, the worst of which is “maintenance of biodiversity requires that populations be maintained”
5. In the methods section, the authors claim their analysis is “based on scientifically designed longline surveys and tagging studies”. In fact, all of their data are commercial fisheries data from the usual sources. These data were used to calculate catch per unit of effort, which is then interpreted in the most naïve manner as an index of abundance. The only use of scientifically designed longline survey data was to attempt to verify commercial catch rates by comparing them against research data “when possible”. What comparisons were made? How much of the data were compared? What were the results of the comparisons? What does “when possible” mean?
6. What is implied by the phrase “important seasonal component of the marine biodiversity”? It appears to mean that ABT have been seasonally abundant in the past. Why not say it simply? How does one determine which components of biodiversity are important? Are all components equally important? Are some seasonal components not important? Are tunas more important than diatoms – both seasonal?
7. The authors assert the (presumed ABT) prey populations in the North Sea have recovered, but that ABT have not. Given the generation time of ABT, has sufficient time elapsed to span the expected time lag between prey and predator?
8. There is no critical examination of the validity of CPUE as an index of abundance. (To be sure, brief mention is made of the change in targeting practice by some fleets.) The authors state that the Japanese longline fleet “landed over 175 thousand bluefin tuna during the first fifteen years of the fishery” in the South Atlantic. They claim that this catch has reduced the biomass to 1/20 of its pre-exploitation level. A simple back of the envelope calculation puts the pre-exploitation level at fewer than 200 thousand fish – not a very large population. The authors need to address whether the CPUE abundance index and the recorded catch are consistent with population dynamics. A quick application of even the simplest production model would provide such a

test. Instead, the authors offer no quantitative analysis whatsoever. One expects more from scientist with the modeling credentials of the senior author.

9. The authors casually dismiss the truly important paper by Ravier and Fromentin on long-, mid-, and short-term variability in eastern Atlantic bluefin tuna stating that environmental conditions cannot effect tuna populations on such a large scale. In fact, R&F go to some lengths to show that environmentally mediated variations in recruitment can produce large widespread fluctuations in abundance. Furthermore, the trend in ABT abundance published by R&F appears could be interpreted as beginning a period of low abundance at the start of the time series considered by Myers et al. What is the impact of increasing exploitation during a period of low recruitment?
10. On page 10, the authors report signs of recovery of ABT off Brazil. How is that consistent with the assertions that ABT have been virtually eliminated? The authors appear to unaware of reports recovery of ABT fisheries in northern Europe (Olafsdottir & Ingimundardottir, 2000; cited by R&F)
11. There is no doubt that ABT populations are in difficulty and that aggressive management measures are justified. If, as the authors suggest, populations are depressed below previously estimated low levels, the situation is even more severe. However the authors have not made their case, and it is very important to do so. They present no real analysis beyond arm waving about CPUE trends and arrows drawn on maps. Personally, I do not think the catch and CPUE trends are consistent with population dynamics, but then I haven't done any analysis either. At the very least, Myers et al need to apply some simple production models. In the long run, it is necessary to apply an age-structured model that is also spatially structured in order to credibly estimate the status of the ABT population prior to the 1950s. Such models exist and should be applied.
12. A slight digression: Recovery is a recurrent them in this paper. Indeed, much of current fishery management effort is directed toward "recovery of stocks". The very notion of recovery is based on the assumption that in the absence of fishing, fish populations would be at about the same size as they were at the start of fishing. Such an assumption has no basis. There is growing evidence that there are large variations in the productivity of the marine ecosystem. (The Ravier & Fromentin paper is an example that should be kept in mind when considering the status of ABT.) Variations in productivity will undoubtedly mediate the pace at which over-exploited fish populations recover. We should not, however, use potential variability in ecosystem productivity as an excuse for not implementing appropriate restrictions on fishing.
13. Rather we need critically analyze available data to determine, as best we can, whether declines in populations are due to excess fishing, to environmentally mediated recruitment failures, or to exacerbation of one by the other. And that is the reason why this manuscript is such a disappointment. The authors have made no attempt to interpret the CPUE trends in the context of population dynamics or to temper their conclusions in light of what is known about environmental variability.