Dear Dr Benfield,

Please find attached a revised version of our MS ‘Shifting headlines? Size trends of newsworthy fishes’ (MS 2018:07:30093). We have carefully considered all of the reviewers’ comments and revised the MS accordingly. We detail our revisions and responses below.

In particular, we have now implemented the quantile regressions suggested by Reviewer 1 and provided a revised datafile that has the exact data used in the analyses. The new analysis provides a clearer picture of the temporal change in reported relative size of newsworthy fishes, and we have rewritten large parts of the Discussion to reflect these results. However, we do not agree with Reviewer 2’s comments about the validity of the data we extracted from newspaper articles. This comment misunderstands the purpose of our paper, which is not to document actual changes in (relative) sizes of fishes but changes in (relative) sizes reported by journalists. It would, of course, be great to know whether there is a difference between the real and reported relative sizes of newsworthy fishes, but that is not our goal here. The accounts published in the media are what shapes public perception of the status of fish populations, and that is what we focus on. We now make this point explicit in the MS.

We hope that you will now find our MS suitable for publication. We look forward to hearing from you.

Yours sincerely,

Brett Howard & Fiona Francis

On behalf of all co-authors.

REVIEWERS’ COMMENTS

Editor’s comments

As you can see from the reviews, while all three reviewers liked the concept behind your manuscript, their recommendations were quite variable. Two reviewers (1,3) recommend minor revisions while the other reviewer recommended rejection. After reading through all three reviews it's my recommendation that you spend some time considerably revising this manuscript.

Reviewer 2 raises some valid concerns about the underlying quality of the data used in the analysis. I agree, and this may or may not be compatible with a revision. If you can find a reasonable means of assessing the quality of the underlying data then you will have addressed to a large extent, this concern. If the reviewer is correct, and the data are simply too unreliable upon which to base an analysis, then the paper probably cannot be published here. Reviewer 3s comments regarding the absence of supporting data to back up your conclusions may perhaps point to the underlying unreliability of the data upon which the analysis is performed.

Both of the other reviewers raise issues associated with your interpretation of the data, particularly as it applies to the charismatic megafishes. Reviewer 1 also was unable to reproduce your figures using the data you provided.

Please read through the comments of all three reviewers carefully. I look forward to reading a substantially-modified version of this manuscript that addresses the concerns raised in this review.

Reviewer 1 (Craig McClain)

Comments for the Author

Overall I love this paper. The methodology is sound and the paper is overall well written. I have read over the paper several times now looking for any comment or suggestions to make but have nothing to offer. I say this so that this review is not seen a light review but rather the paper is sound. I offer the following as thought and discussion and maybe worth of inclusion?

1. Do you think the lack of pattern reflects low sample sizes and the fact that even in subsets that several species are grouped together? Perhaps this blur any intraspecific patterns?
We do not think that sample sizes are an issue: the charismatic megafish subgroup shows a pattern when this subgroup is the most diverse phylogenetically and in terms of life histories and it has the smallest sample sizes. We have now implemented quantile regressions and this technique reveals stronger, clearer patterns than the previous analysis.

2. The shifting baseline requires two criteria. The first one describes an overall decline in average size a shift in the distribution. The second criteria is focused on extremes, i.e. the rare event. Although the overall shift in the size distribution may be occurring it does not mean that these already rare events, both the existence of a record holder and its capture, will not happen. You need simply a single individual to escape this pressure. Does the fact the two criteria focus on different aspects of the size spectrum suggest they many not align?
Our fisheries model shows that the two criteria are in fact aligned. Maximum size decreases, along with average size, when fishing effort decreases.

3. It does appear in some of the graphs that the frequency of smaller individuals being reported is increasing even if mean reported size is not. I would suggest potentially running a quantile regression to see if these patterns could be recovered in the lower quantiles. Perhaps this is a draw of the recent as social media and internet have allowed us to report more.
This is an excellent suggestion that we have now implemented. Quantile regressions have provided a more nuanced picture of how relative size of newsworthy fishes has changed over time.

***I had gotten excited about doing this and was going to attach r code and the figure to show you how many of your analyses would likely have significant regressions in the lower quantiles (<25%). However, when I downloaded the data, I was unable to replicate Figure 1. According the the text relative size is reported length divided by Fishbase max length (reported.length.cm / fb.max.tl.cm). This needs to be clarified in the manuscript.
Apologies. The variables we used in the analysis were calculated from variables in the datafile that was originally uploaded. We have now uploaded a datafile with these calculated variables. The reviewer should now be able to replicate our results.

Reviewer 2 (Anonymous)

Comments for the Author

This paper is extremely well written and nearly flawless in its presentation. However, the authors used excellent methods and sophisticated statistics and obviously did an exhaustive search of non-scientific literature to analyze questionable, unreliable, dubious, and perhaps incomplete (only literature in English) data. Although the goal of this manuscript is laudable and very important, a manuscript resulting from good methods applied to unreliable data should not be published in a scientific journal. Perhaps some other venue would be more appropriate.

There are potentially two sources of unreliability in our data. The first would be a biased search that led to a biased subsample of all available articles. As the reviewer points out, our search was exhaustive. We documented it thoroughly and it was as systematic as searches of grey or non-scientific literature can be. Our inclusion criteria are explicit and can be replicated by others. An obvious source of bias is that we focused on English articles. This was in part for practical reasons because the databases we had access to are in English. It is possible that articles in other languages might present different patterns, although we do not know a priori why this would be the case. This could form an interesting follow-up to our study. However, in light of this potential bias, we now make it explicit that our realm of inference is English-language journalism.

The second potential source of unreliability is the data extracted from the articles. These data were species ID, lengths and weights. We eliminated records for which species identity was not certain. We acknowledged in the MS that weight data were likely to be less accurate than length data because they were more often estimated rather than measured directly. For this reason, we focused our analyses on length. We believe that we have taken every precaution to decrease the uncertainty inherent to a dataset such as ours.

Despite these precautions, the reviewer states quite confidently that the size data we are using are ‘questionable, unreliable and dubious’. Clearly, we could not be present at every dock and on every boat through the years to verify the accuracy of the sizes reported in newspapers! The reviewer’s comment demonstrates a fundamental misunderstanding of the purpose of our paper, which is not to document actual changes in (relative) sizes of fishes but changes in (relative) sizes reported by journalists. These accounts published in the media are what shapes public perception of the status of fish populations. We now make this point explicit in the revised MS (bottom of page 5).

We have made the changes that were suggested in comment bubbles on the MS.
Line 174 – Changed elasmobranchs to sharks
Lines 375-376 – Rephrased ‘large fishes are shrinking’, with added references to show that this statement is not based on our results but on published empirical trends.

Reviewer 3 (Alistair Dove)

Validity of the findings

Here I do have some concerns with the conclusions, particularly with respect to the charismatic megafish, rather than the gamefishes.

On line 200, the authors report that relative length of gamefish did not change over time, but that mass increased significantly. This is an odd finding since length and mass are so tightly correlated. Similarly, but in reverse, the relative length of charismatic megafish declined significantly over time, but their relative mass did not. No explanation is given for these unusual results, but they really leap out at the reader so it seems some speculation at least is warranted.

We cannot explain the gamefish result but we speculate at the end of the discussion that the increase in relative weight over time of this group might be due to the recent ease of verifying tournament catches against size records – a task facilitated by online access to information. Quantile regressions now show similar declines in length and weight of megafish for the lowest quantiles.

OK, no problem. Thanks.

My greater concern is in the interpretation of the results relative to the charismatic megafishes and, specifically, the generalization of well-established reductions in fish length and mass over time among gamefish (refs on line 270) to apply to the megafaunal species for which no such reductions are yet in evidence. I can only speak out of experience with the megafaunal species, but I'm only aware of one published instance that documents decline in size of whale sharks, and that's the Bradshaw and Holmberg papers cited on line 329-330, which document decrease in length of whale sharks at one site in Australia over time. I don't think there's any other whale shark site we've been following long enough to detect a change in size over time and I'm not aware of any similar reports for molids, basking sharks or manta rays, so I don't think the authors can substantiate a pattern of scientifically proven decline in size among megafaunal species as they can for the gamefishes, and thus interpreting changes in their representation in media is tricky.

It is well documented that fishing results in dramatic declines in old fish (e.g., Barnett et al. 2017), and also that old fish are disproportionately larger than young fish. Thus, our default expectation for all species affected by fishing (whether target or bycatch) is that they are likely to have become smaller over time.

Barnett LAK, Branch TA, Ranasinghe RA, Essington TE (2017) Old-growth fishes become scarce under fishing. Current Biology 27:2843-2848

This is still a “default expectation” (i.e. an assumption) and not an established pattern evidenced by data in hand. Fishing may well result in declines of old/large gamefish, but there’s no evidence of this for the megafaunal species, with the exception of the Bradshaw paper for whale sharks at Ningaloo.

There’s a second problem with interpretation of the results concerns taxonomic confounding among the megafishes, specifically manta rays and Mola spp.. Up until recently, both mantas and sunfishes were considered monospecific, but we now know that there are probably three manta ray species and at least four Mola spp.. In both cases, the body size varies considerably between species, so how do we interpret historical reports of body size of a “manta ray” or “sunfish” without knowing which species we’re talking about? This does not apply as much to the gamefishes, who have been well studied and are on the whole more easily identified.

Thank you for highlighting this potential issue, which we had overlooked. We have now examined our recorded of Mola and Mobula more carefully to see whether any of them can be definitely assigned to one species. This is the case (on the basis of size and catch location) for the two records of *Mobula* that provide lengths. For *Mola*, however, the records could be either *M. mola* or *M. tecta*. We have therefore repeated the analysis, assuming that all records are *M. tecta*. The results are very similar to our initial analysis (where we assumed that they were all *M. mola*). We now explain these new analyses in the last section of the methods and present the results of this sensitivity analysis in the supplement (Figure S1), but have retained the initial analysis in the main text.

OK, I appreciate the authors response but it's not quite there yet. L212 of the revised MS says "Three species of *Mobula* exist: *M. birostris* (maximum length: 910 cm), *M. alfredi* (500 cm) and *M. mobular* (520 cm; *Froese & Pauly 2016*)." but this is not accurate. There are **eight** species of *Mobula* (White et al. https://doi.org/10.1093/zoolinnean/zlx018). Within that genus there are three **manta** rays: *Mobula birostris*, *Mobula alfredi* and an undescribed third species that has been known since Marshall et al (2009) (DOI: 10.5281/zenodo.191734) and is variably called the Caribbean, West Atlantic, or Yucatan manta (Hinojosa et al https://doi.org/10.7717/peerj.2586). *Mobula mobular* is easily distinguished from manta rays and should not have been confused with mantas, although I guess it's possible in some press markets. The same is true for *Mobula tarapacana*, which is also very large.

Similarly, L227 of the revised MS states "Two species of *Mola* are known: *Mola mola* (maximum length: 333 cm) and *M. tecta* (242 cm; *Froese & Pauly 2016*)." This is not accurate; there are at least **three** sunfish: *Mola mola, Mola tecta*, and *Mola alexandrini*. *Mola ramsayi* may be valid too, or it may be a synonym of *M. alexandrini* (https://link.springer.com/article/10.1007%2Fs10228-017-0603-6).

Third, something bugs me about the difference between the way gamefishes and megafaunal species come to be represented in media. Gamefishes are deliberately fished, which is an activity that explicitly prizes and seeks to catch the largest individuals. By contrast, the megafaunal species tend only be reported when they are stranded, entangled or caught as bycatch, all methods that are essential neutral to body size, at least relative to targeted gamefishing. How this difference might be expressed in the way the species are recorded in media isn't clear to me, but it seems like something that should be pointed out.

It is well known in fisheries science (e.g., Barnett et al. 2017) that even if the effects of fishing are not targeted on large fish, the numbers of large fish will decline disproportionately more than the numbers of small fish. This is because large fish are older, and old fish experience the additional mortality that fishing or bycatch constitutes

for more years of their life than small fish do. We do nevertheless highlight the potential difference in targeting between megafishes and the other two groups as a reason for the different trends they exhibit (lines 339-341).

OK

Finally, in the analysis of changing size relative to IUCN RedList status, the authors chose to compare to the current status of each species, but populations of these species might have been considerably different at the times when media reports were made. Even accounting for this, it occurs to me that the RedList status and relative size ought to be negatively correlated because, as the population shrinks, both tails of the distribution move towards the central tendency, reducing the ratio of average size relative to the maximum. I don't know how else you would investigate relationship between size and conservation status, but it seems an inevitable consequence of reducing population size that relative size would decline, even if average size does not.

It is possible that there is a temporal mismatch between the time of capture of some of the fish in our database and the assessment of their threat status by IUCN. We now mention this point in the Methods (lines 184-186). However, we do not believe that this is a big issue since our database is heavily biased towards more recent years.

With the reanalysis of our data using quantile regressions, the reviewer's prediction is now verified, at least for species deemed to be at high risk by the IUCN (see Fig. 3).

I love it when I'm right ☺