

Interactive comment on “Changes in population depth distribution and oxygen stratification explain the current low condition of the Eastern Baltic Sea cod (*Gadus morhua*)” by Michele Casini et al.

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General comments

The manuscript by Casini et al. aims to shed new light on the mechanisms linking the expansion of oxygen minimum zones in the Baltic Sea and the decline in condition of Baltic cod since the 1990s. The manuscript therefore addresses a topic of global interest, the impact of environmental changes and in particular deteriorating oxygen conditions on the status of fish stocks. Moreover, the condition decline of cod has received

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a lot of scientific attention in the Baltic region, and also has applied consequences in affecting mortality and economic value of cod. There has been a large number of publications directly or indirectly addressing the potential mechanisms driving the condition decline, including previous work linking low condition of cod to exposure to low oxygen conditions (Limburg and Casini 2019). At the same time, the implication and relative importance of different mechanisms is still incompletely understood and requires further attention. Since the Baltic has seen very pronounced environmental impacts, including the strong expansion of oxygen minimum zones, it can be considered as model for other coastal areas, and insights gained from the cod story can also be of value for fish stocks under environmental stressors elsewhere. The manuscript therefore addresses questions that are relevant on both the regional Baltic scale and of interest to a general (global) readership.

In terms of the specific analyses, I find the combination of time series on cod depth distribution and on the depth layers at which waters with low oxygen concentrations are present, the resulting calculation of the overlap between these areas, and finally the correlation of overlap with cod condition interesting and relevant. The study provides an independent confirmation of previous results by Limburg and Casini (2019) that implicate exposure to waters with low oxygen concentrations in low cod condition, and provides an important new perspective on the nature of that exposure. This leads to interesting new questions, in particular why the cod depth distribution moves deeper into low oxygen waters in the fall, instead of shallower to avoid such layers.

At the same time, I see a number of major limitations with the present version of the manuscript that should be addressed. I will list these in the following:

—Tendency to oversell the results: this is recurring through the title, abstract and discussion. For example, the study title is not in line with the results. The title implies that the study results alone explain the low condition of Baltic cod, when in reality, the study sheds additional light on one potential mechanism, direct exposure to low oxygen waters, which does not rule out alternative mechanisms (both linked to expanding

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oxygen minimum zones and to other factors) that have been proposed before. Abstract L27-29: point out more clearly that the study is assessing the role of direct exposure to low oxygen waters, not “the processes”. Discussion L223: should be “one mechanism” not “the mechanisms”. Conclusion L293: should be “shown here one mechanism”, not “the mechanisms”.

—Delineation of results from previous work: Exposure to low oxygen water was already previously linked to the Baltic cod condition decline by Limburg and Casini (2019) using otolith microchemistry. This study is cited and referred to by Casini et al., but still, the apparent narrative here is that the exposure to low oxygen waters is shown via the identification of increasing overlap of the depths of low oxygen waters and the cod depth distribution, and that this is then confirmed with otolith microchemistry in this manuscript (e.g., abstract LL29-34, Introduction LL89-93, Discussion LL224-228). This really has it backwards. I suggest to instead clearly lay out key results and conclusions from Limburg and Casini 2019 in the Introduction, and then use this as rationale for the (relevant and interesting) independent confirmation and new insights into the specific patterns of exposure to low oxygen waters in this manuscript.

—Use and presentation of otolith microchemistry dataset from Limburg and Casini 2019 in this study (connected to previous comment): I would strongly recommend the exclusion of these data from the present manuscript. To me, the analysis and results mirror the previous publication by Limburg and Casini too closely to warrant inclusion here. The authors acknowledge the previous study, but without going into details. However, the data set, analyses, discussion points (Section 4.2) and conclusions are largely the same. Also, the results from otolith microchemistry analyses are not formally correlated to the depth distribution analyses, and appear rather like an “afterthought” in this manuscript. The inclusion in the manuscript thus unnecessarily duplicates previous work. If conclusions from the previous work are instead clearly presented in the Introduction, this will provide the rationale for the real strength and novelty of the present study, the depth distribution analyses. New insights from this independent approach

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compared to the insights from the original otolith microchemistry approach could then also be discussed more explicitly in the Discussion. Interestingly, all conclusions in the conclusion section of the manuscript (LL293-306) relate to this aspect of the study anyway.

—Statistical analyses: Right now, the manuscript is lacking in formal statistical assessments. This includes statistical approaches to assess the significance and nature of temporal trends in the depths of low oxygen waters, cod depth distributions and overlap, as well as the formal assessment of the link of overlap and cod condition over time. The Material and Methods should then also include a dedicated section outlining statistical approaches. In this context, looking at Figure 4 of the manuscript, many of the observed temporal changes do not look linear. E.g., for SD26-28, cod mean depth was essentially stable after 1990, and for SD25, neither cod depth distribution nor depth of low oxygen water appears to change significantly between 2008 and 2018. Formal statistical analysis would therefore have the potential to lead to additional insights beyond the points included in the manuscript.

Specific comments

Throughout the entire manuscript, I was waiting for an explanation for the discrepancy of the cod depth distribution trends over time between the very similar data sets and analyses in Orio et al. 2019 (showing cod distributions at least for SD26-28 becoming shallower since the 1990s) and this manuscript. This was then given in the second to last sentence of the conclusions :) I suggest to explicitly explain the difference between the datasets (fall versus other seasons) already in the Material and Methods, and then discuss this interesting difference between seasons in the main part of the Discussion, not just in the Conclusion.

L53: Would cite Chabot and Dutil 1999 here already.

L60: Suggest addition of Reusch et al 2018 as probably best reference for combined strong temporal changes in temperature, eutrophication, oxygen in the Baltic Sea.

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L60-61: to my knowledge, the degradation of benthic communities is NOT well documented in the Baltic Sea, and lack of time series on benthic communities has been one of the issues hampering understanding of consequences of expanding oxygen minimum ones. Rephrase.

L71: see major comments regarding previous results from Limburg and Casini 2019. Suggest to present in much more depth here and explain that link between low condition and exposure to low oxygen water was established in that study.

L73: suggest to mention the actual mechanism connected to this, density dependence.

L73-75: add mechanism proposed by Brandner 2020, mild hypoxia reducing rate of digestion.

L92-96: The otolith works comes in like an afterthought here, since it is not set up in any way in the Introduction section (linked to major comment regarding otolith work)

Section 2.1: more clearly point out that this (or very similar) cod condition time series were previously published and are here updated to 2018?

LL107-109: please explain rationale of using size class 20-29 and 40-49 cm for condition calculations.

Section 2.2: suggest to point out more clearly the key difference between studies, focus on fall here versus all seasons in Orio et al 2019 (see my previous comment above).

LL125-135: I am not a physiologist, but I guess in principle use of oxygen as continuous variable (instead of somewhat arbitrary boundaries) would make sense. I can see that use of specific limits facilitates analysis, but would mention this possible limitation.

L155: Explain the rationale of using a Fulton's k of 0.9. Also give other thresholds (e.g., "very low" used later in L163) here already.

Section 3.2: in the Discussion section (not here), suggest to discuss the patterns observed for fall here compared to the patterns in Orio et al 2019 reporting cod depth

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distribution contraction to shallower water for SD26-28 when looking at the entire year.

Section 3.2, 3.3, 3.4: would all benefit a lot from formal statistics.

L243: I think the discussion of mechanisms that can explain what drives cod into layers with low oxygen levels is quite central, since it relates to the key novel finding of this manuscript. Suggest to therefore not state that "beyond scope" of manuscript, but rather state that you can only speculate and will discuss possible causes as systematically as possible.

LL244-245: The role of temperature was also the first thing that came to my mind, but I then wondered about actual temperature profiles in fall, and whether they would support these considerations. It would be useful to include information on prevailing temperature depth profiles in fall as background for the discussion.

L283: Should read "although we have confirmed here that . . ." and refer to Limburg and Casini 2019.

LL283-291: Discussion of other factors could be more extensive. Cite Brander et al 2020 here as well.

L297: Agree, very interesting future direction, and a question that really results for the first time from the analyses in this manuscript (not possible from Limburg and Casini 2019) – this would be worth pointing out.

Figures: I suggest to add a figure to illustrate key findings regarding the correlation of cod condition and the overlap of cod depth distribution and low oxygen waters.

Related to the general comment regarding the presentation of otolith microchemistry data in this manuscript, Figure 6 of this manuscript appear to be an alternative view of Figure 2 c in Limburg and Casini 2019, i.e., not adding new information here that could not be provided from that manuscript.

Technical corrections

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LL23-24, L62: wording should be more precise – “exponential increase” not really correct, suggest “strong increase”; “largest marine dead zone”, unnecessarily dramatic.

LL26: “elusive” does not really reflect that specific alternative mechanisms have been proposed.

LL29-32: rephrase, confusing wording.

L59-60: Wording in Breitburg et al 2018 is more scientific (“low O₂ areas have become more extensive and severe”) – suggest to follow this approach.

L81: start new paragraph, focusing on effects and not mechanisms from here on.

L82-82: rephrase “lamented”

L194: “in a couple. . .” – word missing?

L254: “hostile waters” – suggest to rephrase

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