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Discussions

Interactive comment on "Short Commentary on Marine Productivity at Arctic Shelf Breaks: Upwelling, Advection and Vertical Mixing" *by* Achim Randelhoff and Arild Sundfjord

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[Below, please find: Your comments preceded by RC, "Reviewer Comment", and our replies preceded by AR, "Author Reply" and MC, "Manuscript Change"]

RC: Review of "Short Commentary on Marine Productivity at Arctic Shelf Breaks: Upwelling, Advection and Vertical Mixing" by Achim Randelhoff and Arild Sundfjord

The authors discuss shelf break upwelling in the Arctic Ocean and argue that it should have different characteristics in different parts of the Arctic Ocean and not necessarily will be pronounced phenomena in the Atlantic sector of the Arctic Ocean in general

and on the Barents shelf break in particular as a result of the climate change.

Major comments It seems that authors try to argue with the opinion expressed in some of the recently published papers, that in the future conditions for the shelf break upwelling in the Atlantic sector of the Arctic will become more favourable. However, from the introduction (or rather "Upwelling in the Arctic" section) it is not clear what are the arguments au- thors fighting against. They mention personal communications and reference several papers, but did not provide any details.

The counter argumentation is very week. It is basically a collection of statements that are not supported by any evidence. It is just author's speculations on the topic with ideas that may or may not be true.

I have a hard time to define the type of this article and the purpose it is written for. The topic of the shelf break upwelling in Atlantic sector is very interesting and it would make a great contribution to our understanding of the Arctic Ocean when investigated properly though numerical modelling or data analysis. Unfortunately, this manuscript lacks any scientific novelty supported by evidence. I also believe it is too shallow to be a review. I do not recommend this manuscript for publishing in "Ocean Science".

AR: > Thank you for taking the time to review our manuscript. Below, find our replies that we hope might help convince you regarding the purpose and quality of our "short commentary".

> First, about the type of article and intended readership. You correctly state that this paper lacks the novelty to be an original research article. It is also not a comprehensive review of every study that has been done on this issue (the choice "review article" in OSD is for practical reasons mostly, and suggested by the Editor). Rather, as the title states, it is a "Short Commentary" - implying we take a stance in a debate. (If you do not believe there is such a debate, please go to a relevant conference, mention Arctic shelf break upwelling, sit back and enjoy the discussion.)

> You mention readers “familiar with Arctic Ocean hydrography” would not hold some of the misconceptions we argue against. We can agree here, maybe pending some clarifications about what constitutes “familiar”. But not everyone interested in the biological productivity in Arctic shelf regions is equally well-versed in the underlying physics. This readership in particular may profit from having all the contributing factors laid out in a single, accessible, quick-to-read piece of text, instead of wading through a lot of more detailed literature to acquire the familiarity with the material. (Something similar goes for physicists wishing to link their research to biology.) With the present manuscript, we tried to provide such an easily accessible text, which has not been published before as far as we know. Following our initial inquiry, we have been encouraged by the handling editor to submit such a text.

> Second, you repeatedly claim that the entirety of the manuscript is based on speculations and statements for which there is no evidence. You must certainly mean “support in the literature” or some such. To this effect, we have in the revised version added a good number of references that will hopefully clarify what we base our reasoning on, even if we initially (and mistakenly) thought these issues were too basic to have to reference them.

> Overall, you criticise us for not writing a different kind of article, which is fair enough, but misses the point of whether this article is useful to large enough an audience.

RC: Minor comments

AR: > [Below, please find: Your comments preceded by RC, our replies preceded by AR, and our edits to the text preceded by MC.]

RC: P 2, L 2 It would be nice to provide references, showing that it “received increased attention”. Now after the sentence you make a reference to the figure, which seem strange and out of place.

AR: > The reference (after the words “Arctic Ocean”) should have been to the left

panel of Fig. 1 only, which shows a map of the Arctic Ocean. We apologize and appreciate that you brought this to our attention so it could be clarified as it should. MC: > The reference now reads “see below for a list of references; for an overview of the geography, see the left panel of Fig. 1”, and a later reference to Fig. 1 now reads “an illustration is also given in the right panel of Fig. 1”.

RC: Fig. 1. Why you illustrate Atlantic Water inflow by snapshot from the model, that can be pretty far from reality (Hattermann et al., 2016 do not use data assimilation)? Why not from climatology or some reanalysis product (e.g MERCATOR OCEAN)?

AR: > As you say yourself, it is an illustration, and it does in fact capture the real-world features and patterns that are relevant for this paper (inflow of near-surface warm and salty water). We could have hand-drawn something (this, too, “pretty far from reality”, but which would still capture the same essence), but for ease of producing the figure and because it looks nicer than what we could have assembled in a graphics editor, we used this data and plotting software that we had at hand. > Note that this picture is confirmed by e.g. Cokelet et al. , 2008, and several other papers going back a few decades, but we believe you will agree that the situation we depict in Fig. 1 is at least qualitatively fully supported by available literature.

RC: P3, L5-7 You should really provide more evidence that this is now a “universal paradigm” and that the paper you mention above are actually directly and unconditionally transfer results obtained for the Pacific sector to the Atlantic Sector.

AR: > No, it is not (yet). That is why we state “it might appear as if [it is] being cemented”, rather than that it “has been” cemented. We also never used the words “directly and unconditionally” (the former only in a different context).

> As for providing evidence that the referenced papers do transfer results from the Pacific side to the Atlantic one, the reader is free to check for themselves; in such a short exposition it would only destroy the flow of the text to quote and paraphrase from all those articles. But see e.g. Våge et al.; “A comparison to hydrographic data from the

Pacific Water boundary current in the Canada Basin under similar atmospheric forcing suggests that upwelling was taking place during the survey." [which took place north of Svalbard, our comment]

> On another note, calling this "a paradigm" does not require that it is occurring everywhere, all the time. Instead, a paradigm is a way of thinking about things. That means that the habit of phrasing shelf break productivity primarily in terms of shelf break upwelling (e.g., Falk-Petersen et al. 2014, Williams&Carmack 2015) we believe qualifies as a paradigm; see also our reply to reviewer 1 on this point. MC: > We amended the sentence to "... currently being cemented as a universal paradigm to conceptualize ..." to make this distinction clearer.

RC: P3, L21 Gradients of what?

AR: > In this context, what matters are the gradients of physical properties, even though there are plenty more. MC: > We added "[large gradients] in physical properties".

RC: Fig. 2 Why you use this transect? Is it typical? Why not climatology or reanalysis?

AR: > Yes, it is typical as it says in the figure caption. Otherwise, it is an illustration, and the same comments apply as for Fig. 1 earlier.

> Just as an example, quoting Falk-Petersen et al. (2014), who were measuring in this area and at the same time of the year, "all transects had very similar hydrographic characteristics, with an upwelling zone of warm Atlantic Water (temperature 3–4 °C, salinity $\sim$35 psu) stretching from west to east along the northern Svalbard Shelf".

> As another illustration, see the attached plot below (showing a transect at a similar location across the shelf break) produced (for wind conditions that people might refer to as neutral in terms of "upwelling-favourability") from the same ROMS 800x800 m model that was used for the right panel of Figure 1. It was made for another (quantitative) manuscript on circumpolar upwelling that is going to be submitted in the not too distant future. We do however choose not to include it in the present manuscript we

are currently discussing in order to not clutter the paper; also based on the available literature (Våge et al. 2016, Cokelet et al. 2008, and others) there should be no doubt that the situation we present in Fig. 2 is representative.

RC: P4, L4-9 It is not clear to me why any reader familiar with Arctic Ocean hydrography must think that Fig. 2 show typical upwelling situation?

AR: > Well, that is exactly one of our points, and we are just as puzzled as you are. Then again, not everyone interested in the biological implications might be sufficiently "familiar" with Arctic Ocean hydrography. Given experiences in other regions of the world ocean, it is at any rate not an abstruse idea to think of upwelling when one sees isolines outcropping at the surface close to a shelf break or coastline.

RC: P4, L13 Ivanov et al., 2016 show that under certain conditions heat from the Atlantic Water can mix up to the surface, but this process is not constant and over the northern Barents Sea shelf the thermal stratification in the upper 100 meters is actually still quite strong most of the time.

AR: > You are right. MC: > We added "potentially [leading to....]" before wintertime convection.

RC: P5 L3-15 Statements in this section need supporting evidence. Now it is pure speculations.

AR: > Assuming by "supporting evidence" you mean references: MC: > We have now inserted a number of references in that section.

RC: P5, L 17 No, we haven't. You just claim it to be true earlier, but you did not show anything to support this claim.

AR: > Specifically, on p.4 l.12-14 (original version) we give two references for how vertical mixing is strong in winter (namely, weak thermal stratification), which directly implies that the mixed layer replenishment of nutrients can happen "without recurrence to wintertime upwelling". That doesn't mean wintertime upwelling is not happening or

cannot ever happen, it just means that “upwelling” is not strictly necessary to replenish nutrients. MC: > We amended the text to “can be replenished” to better express this uncertainty.

RC: I am sorry but most of the rest of the analysis is again just pure speculations and to my opinion have no value as a review.

AR: > See our comments at the outset about intended article type and audience. More details as to what exactly you think is speculation would have been helpful, as certainly not “most” of our statements are. > At any rate, we added many references that you will hopefully find helpful. Also, we did add a sentence to the paragraph explaining why a shelf has to be sufficiently shallow and/or narrow to allow for wind-driven upwelling in order to stress our reasoning there as one of the more central parts of the whole line of arguments.

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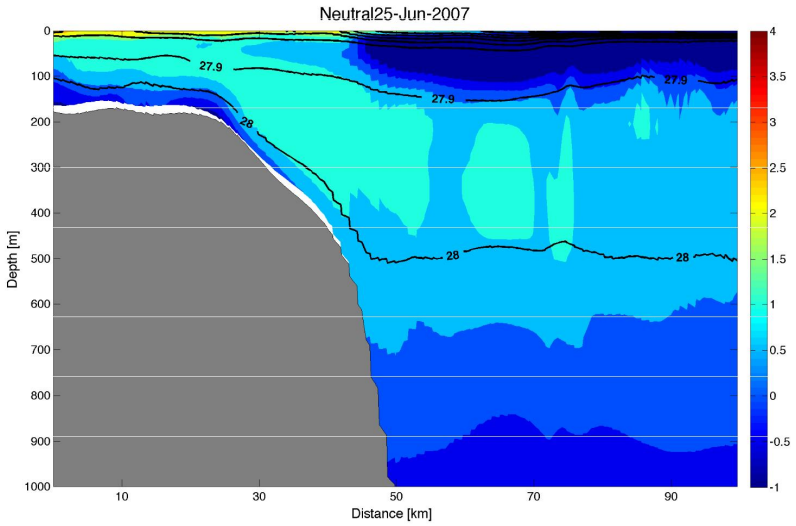

Fig. 1.