



EGUsphere, referee comment RC1  
<https://doi.org/10.5194/egusphere-2022-140-RC1>, 2022  
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## **Comment on egusphere-2022-140**

Anonymous Referee #1

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Referee comment on "The mixed layer depth below the pycnocline (BMLD) as an ecological indicator of subsurface chlorophyll-a" by Arianna Zampollo et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-140-RC1>, 2022

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General:

The paper presents a surprisingly complex statistical approach (GAM) to a large dataset of chlorophyll profiles, breaking the profiles down into key groups based on profile shapes and specifying a large range of parameters based on density and chl contrasts and gradients.

The methods section is incredibly dense and difficult to keep track of. By the start of the Results section, I am afraid I had got so confused with the details I decided to skip to the Discussion and Conclusions to see if I understood what the key, novel results/messages were. I still found it difficult to glean the important findings and their implications in the Discussion. I was left with the view that a surprisingly complex statistical model had been used to analyse the shapes of a lot of chlorophyll profiles (which implicitly have been assumed to be temporally static?) But I did not feel I had learnt anything useful about the general properties of sub-surface chlorophyll layers. There may well be something here that I have missed – but the challenge of deciphering the methods and holding a large number of acronyms in my mind defeated me.

I think the key, interesting point that is being made (though not clearly articulated) is that descriptions of ocean mixed layers are largely informed by starting with the surface ocean and working downwards from there. In shelf seas, particularly where tidal mixing plays an important role, working upwards from the seabed makes more sense. This is a nice way of thinking about the system. Phytoplankton will utilise the stability of the deepest pycnocline where there is still sufficient light, which will set the nitracline position. But pulling this out of the paper was exceptionally difficult – and I may have missed other key points. If you want to make some clear, useful points about chlorophyll layers in shelf seas in a way that a wide audience will be able to understand and use, then I think the paper needs to lift itself out of the statistics and focus much more on the resulting chl shapes and the processes underpinning them. These ideas may well be in there somewhere, but just too buried in the details for me to extract.

The writing overall tends to make things sound overly complicated. It would be useful to go through and simplify the prose and cull some of the repetition – the paper needs to be more concise and clearly argued if you want it to have some impact.

Specific:

Line 38: “Climate change is introducing...” You could also mention the increasing recognition of possible changes associated with large-scale roll-out of renewable energy in deep shelf seas (e.g. Dorrel et al., 2022: <https://www.frontiersin.org/articles/10.3389/fmars.2022.830927/full>).

Line 58: “...where the stratification is maintained by tidal cycles mixing the water column through horizontal circulation...” I think this needs rewording. Stratification is not *maintained* by tidal mixing – the existence and strength of stratification are controlled by a balance between mixing processes (which in NW European shelf seas are generally dominated by tidal mixing) and the source(s) of buoyancy (surface heating and estuarine inputs of low salinity water).

Overall, I get a little confused by the term “deep mixing processes”. Do you mean mixing at the pycnocline or mixing near the seabed?

Line 62: a general statement about ocean productivity and climate change should probably also reference something like Steinacher et al., Biogeosciences, 2010. Clarify that the canonical view is that at low and temperate latitudes in the open ocean productivity will decrease because of strengthening stratification inhibiting vertical mixing of nutrients.

Line 74: What is meant by the nutricline exhibiting positive correlations with MLD? What aspects of the nutricline? The depth, the strength?

Line 107: “Vertical samples...” Do you mean vertical profiles? “Samples” to me implies bottle samples rather than CTD.

Line 109: Is there a particular reason for the choice of 120 metres as the deepest? Is it

simply forced by the data available, or do you have a different reason?

Line 118: What does "426 profiles" mean in the context of a mix of towed and vertical-profiling CTD data? Are the individual undulations of the towed systems each counted as a single profile? Is it clearer later in the paragraph – so maybe the full 1273 profiles needs noting here?

Line 136: "samples' distance" I think should be "sample vertical resolution".

Section 2.2.1: Why was a GAM/spline used instead of a simple spline (or an even simpler moving average)? Some justification/explanation of this choice would be useful. Also, a couple of example profiles in a Fig would help – e.g. one profile where the GAM worked well and another where the visual fixing was required.

Line 194: not strictly "density gradient" – the values you state are densities.

In two of the Methods sections (2.3 – 2.4) I had to work inordinately hard to see what was going on. I think these sections could be clarified with some better ordering. For instance, AMLD is talked about in section 2.3, but the full description of what it is does not occur until 2.4. There is a raft full of HPDs that pops up line 195-200, but it is unclear what they all mean. If you find yourself having to refer to a section further on in the paper (e.g. line 198 you refer to section 2.4 for the explanation of adjusted AMLD) then you need to rethink how you are structuring the material. You need a clear, logical progression of explanations that does not leapfrog – this is really important, as the reader needs to keep track of a large number of different acronyms and their meanings.

Line 213: "transient" – do you mean "transition"? Unclear what you are trying to say.

Line 215: delta-rho is a density difference, not a density gradient. This occurs a few times.

Lines 216 – 227: It is really hard to understand what is meant here (partially, but not wholly, because when you say "this paper" I cannot work out if you mean your paper or the Chu & Fan paper cited in the previous sentence). Clarification needed.

Around this stage I just got very confused with the methods. They appear rather complicated and dense, and I found them difficult to follow. To me this difficulty began to detract from what I thought the paper was aiming to demonstrate. Perhaps consider a Supplementary Material section to deal with the details of the methods (though they

would still need to be clarified) and focus the main paper on the results and implications?

Section 3.1 starts by repeating a lot of the methods. No real results appear until 3.2 and Fig. 4.

I stopped dealing with specific points at this stage - focusing instead on trying to pull out what the key points might be.