Dear reviewer,

We are very thankful for your time and your comments on the paper. According to all the reviewers, we identified some common issues that came across, and we have planned to improve the manuscript following all your advice.

The main points we worked on are: i) better defining the scope of the paper by deleting the Chl-a shapes from the analyses, ii) simplifying the methods, and iii) providing the code to let users trying with the proposed algorithm.

Below, we describe the main changes we introduced into the paper to address the above points.

The scope of the paper has been clarified by focusing on the BMLD (base of the pycnocline) and its use as a proxy for the depth of maximum Chl-a (DMC) in shelf waters. The paper was packed with many details regarding the co-occurrence at the same depth of any density layer (that we renamed "level") (e.g. AMLD, BMLD, DHP and Max N2) and DMC (that we renamed CMd following a comment of reviewer 3). The paper was reporting first the comparison for all the profiles together (section 3.2) and then the comparison for each Chl-a shape (section 3.3). However, the length of the paper and the amount of information were creating confusion among all the reviewers, who struggled to identify the main scope of the paper and often focused mainly on issues referred to Chl-a shapes. On the contrary, we have written this paper to promote a different point of view in investigating subsurface Chl-a by using density profiles. Hence, the main aim of the paper is to highlight the BMLD as a useful tool to predict and investigate CMd in shelf waters. The vertical distribution of CMd nearby BMLDs suggests that this variable has an ecological relevance when we investigate the vertical distribution of Chl-a subsurface patches, and we suggest its use in further research (enlarging these applications in the Discussion). However, this point did not come across easily, and we decided to delete all the analyses related to Chl-a shapes to focus mainly on the use of the BMLD and its potential. The following paragraphs will be deleted: 2.2 in the methods will not include Chl-a shape identification, 3.3 in the results, 4.1 and 4.2 in the discussion. However, understanding the physical processes underpinning the vertical distribution of each Chl-a shape is an open question, and the presented results showed how each shape exhibits a different association of CMd with the pycnocline. We are interested in detailing this question in another paper, to avoid hiding the main scopes of this paper, which are i) proposing a method to extrapolate the base of the pycnocline from density profiles and ii) evaluating its association with the vertical distribution of Chl-a (regardless the Chl-a shape).

The second and third points (*"simplifying the methods"* and *"providing the code to let users trying with the proposed algorithm")* are ensuring that the reader fully understands the method and its potentialities. For this reason, we reduced the number of details regarding the algorithm in paragraph 2.4 and we focused on the requirements, limitations, and circumstances in which the method can be used. We integrated the repetitions in the results into the methods together with figure A1 (now figure 5). Moreover, we uploaded the code of the function on GitHub (https://github.com/azampollo/BMLD), where an example is also provided. The details regarding the structure of the function are reported in the supplementary material to allow people to replicate, improve and use the code. Therefore, the diagram and part of the methods are moved to supplementary materials.

The removal of Chl-a shapes from the paper changed the discussion section, which has been reduced and focused on describing the relationship between density and Chl-a profiles. We reviewed the physical variables that are playing a role in the definition of BMLD and AMLD, and their association with the vertical distribution of maximum Chl-a in the water column.

OVERALL CHANGES

Heading of Section 1.3 was changed from "A new way forward: the base of the pycnocline (BMLD) as an ecological proxy of the vertical distribution of maxima Chl-a (DMC) in shelf waters" to "A new way forward: the base of the pycnocline (BMLD) as a proxy for maxima Chl-a in shelf waters"

Since section 2.2 "Subsurface Chlorophyll-a parameters" was describing the selection of DMC using a portion of the algorithm proposed in this paper, we moved this section after the section describing AMLD and BMLD detection (now 2.2) "AMLD and BMLD detection". This structure allows the reader to have all the information to processes the method used to detect DMC. Hence, Section 2.4 is now describing the chl-a parameters. The sequence was changed from "2.1 Physical and biological oceanographic samples, 2.1.1 Standardized vertical sampling for density and Chl-a, 2.2 Subsurface Chlorophyll-a parameters, 2.3 Common methods identifying Density Layers (DLs), 2.4 AMLD and BMLD detection, 2.5 Evaluating the association between density levels and subsurface Chl-a" to "2.1 Physical and biological oceanographic samples, 2.1.1 Standardized density profiles, 2.2 AMLD and BMLD detection, 2.3 Common methods identifying Density Levels (DLs), 2.4 Subsurface Chlorophyll-a parameters, 2.5 Evaluating the association of density levels with subsurface Chl-a".

#REVIEWER 1

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).

Thank you very much for pointing this out. We changed the sentence that was referring only to climate change, and we added man-made structure as a source of variations in the mixing/stratification balance.

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).

We agreed it was not fully explained and we changed the whole sentence following your comment: "The stratification is generally controlled by mixing processes (tidal mixing and surface wind stress) and sources of buoyancy (surface heating and estuarine inputs of low salinity), whose balance allow primary producers to grow in favourable light and nutrient conditions within the pycnocline. In the North Sea, mixing processes are mostly regulated by strong tidal currents (Glorioso and Simpson, 1994; Loder et al., 1992; Sharples et al., 2006, 2001; Simpson et al., 1980; Zhao et al., 2019b), especially in prolonged stratified conditions, when upward fluxes represent the only source of nutrients intake within the pycnocline."

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

We intend processes occurring below the surface, likely between the pycnocline to the seabed. This has been clarified in the whole paper.

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.

We realised that the sentence was referring (again) only to climate change, while we want to refer to any source of disturbance to physics. Hence, it has been changed as follow: However, despite the clear linkage between SCMLs and tidal mixing in shelf seas, variations on productivity have been largely conducted at oceanic sites by investigating the mixing processes above the pycnocline (within the upper mixed layer) (Somavilla et al., 2017; Steinacher et al., 2010), omitting the effects of processes close to the seabed, e.g. variations of mixing processes below the pycnocline.

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

The depth. This information is now added.

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?

It is forced by the data available. We deleted the information since it may rise doubts as yours, and it is described in section 2.1.

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?

We moved the total amount of profiles at the beginning of the paragraph. We also specified the that 426 profiles were collected using the vertical CTD, and that 847 profiles were obtained from the undulating CTD with the sentence ". The continuous profiles obtained from undulating CTD were converted into 847 single profiles of the water columns."

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

Changed.

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.

I did not know the simple moving average method, and I have read it measure the average of a specified amount of data. I think it would have been a valuable method to fit and smooth the density profiles having a large number of observations, while it may have been limited for those profiles having a few numbers of observations throughout the water column. Here, GAMs were used since it is the method I know better among the smoothing tools, and its application was successful in most of the dataset. Hence, I decided to keep on with this method considering the large number of analyses that the paper was requiring. Hope it is still a valuable tool for the paper. I followed your advice and I added a figure in the Appendix A with two profiles: one where GAM worked and one where the visual fixing was required.

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

We refer to density threshold and hence we fixed the text (kg m^{-3}).

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.

Thank you for suggesting to change the methods. We agreed it was not easy to read, and hence we changed the sequence of sections. The sequence was changed from "2.1 Physical and biological oceanographic samples, 2.1.1 Standardized vertical sampling for density and Chl-a, 2.2 Subsurface Chlorophyll-a parameters, 2.3 Common methods identifying Density Layers (DLs), 2.4 AMLD and BMLD detection, 2.5 Evaluating the association between density levels and subsurface Chl-a" into "2.1 Physical and biological oceanographic samples, 2.1.1 Standardized vertical sampling for density and Chl-a, 2.2 AMLD and BMLD detection, 2.3 Common methods identifying Density Levels (DLs), 2.4 Subsurface Chlorophyll-a parameters, 2.5 Evaluating the association of density levels with subsurface Chl-a".

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

We meant transitional. Transient changed into transitional.

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

Delta-rho was used to refer to a density difference and hence we used the right specification in the whole paper.

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.

We changed the beginning of each sentence for lines 216-227, with "the above assumptions", "such density conditions" and we started a new paragraph after citing Chu&Fun paper. We used "this paper" to refer our paper, and hence we changed the sentence into "In the proposed algorithm, the detection of AMLD.."

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?

We agree with your comment, and we decided to move this paragraph to supplementary materials. Hence, the section 2.4 describes now the definition, shortly the method, and circumstances in which the method can be used. The supplementary materials include now the description of the algorithm, and link to GitLab repository where the function can be downloaded and used. A small description of its use, and an example is reported in GitHub.

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

This section has been moved to methods in the new section 2.2.

#REVIEWER 2

l.14 Abstract and general. The definition/selection of 8 ‘density layers’ instead of other number is not sufficiently justified. These are levels (discrete depths) instead of layers.

All DL were changed from “layers” into density “levels”.

l.36. (also l.57). Specific for shelf seas with strong tides. The authors should notice that many shelf seas have small o no tides.

Thank you for pointing this out. We specified that we refer to the North Sea and that it is characterized by strong tidal mixing.

l.41. Bryden et. al 2005 paper here is not adequate. Scale is too broad and main outcomes are superseded by further results of the rapid array and others.

We changed Bryden et al. 2005 with Orihuela-Pinto et al., 2022 and Bonaduce et al. 2019.

l.89-90. There are no standard methods to MLD identification neither in shelf nor oceanic waters.

Sentence changed.

l.91. BMLD as an “indicator” of the vertical... Indicator or proxy?

Thank you for pointing this out. We changed “indicator” with “proxy”.

l.96 and others. BMLD is indistinctly referred to as ‘bottom mixed layer depth’ and ‘below mld”. Should address this mismatch.

All the “bottom mixed layer depth” are changed into “below mixed layer depth” to be consistent.

l.101 ‘this new level of understanding’ sounds a bit presumptuous, maybe just this new algorithm.

“This new level of understanding” changed with “This approach” since we are talking not only about the method.

l.111. ‘Fig.2’. It is normally requested to cite figures in order, please check.

We deleted this reference, together with the figure – moved to supp. materials.

l.123. ‘standard MSS editing procedure’ requires a reference.

Sentence removed after comment rev. 3.

l.132 and others. Not necessary to specify used functions of TEOS-10, this is too much detail.

Sentence removed from “ In situ conductivity were converted..” which included TEOS-10.

l.135 et.seq. (section 2.1.1). General, I guess the authors are using Chlorophyll fluorescence profiles (from a fluorometer) which is not the same as Chlorophyll-a. Should clarify.

Data were calibrated for Chlorophyll-a from Marine Scotland Science. Hence, we specified always chlorophyll-a instead of fluorescence.

l.138. Understand that smoothing/resampling refers only to undulator.

Correct. We specified it by adding ".. 0.5 to 1 m from undulating CTD".

l.149 'The analyses were run in R v3.6.3...' too much detailed. Again in l.204 etc.

We prefer to refer to the functions and software that were used to allow a perfect replication of the methods. In particular, the new versions of R may not load correctly old packages, and hence functions that are used today may not be used in the future R versions. Therefore, we agreed to specify the version of R that we used.

l.161-162. I do not understand sentence 'and three equal sections were used to divide the difference between the minimum and maximum Chl-a values into three equal sections'

This sentence was deleted with Chl-a shapes.

l.175. Fig.2. why HCL (e) is above HCU (f)? I find this confusing.

This figure was deleted with Chl-a shapes.

l.191 One of the first comprehensive classifications of MLD objective methods available is provided by Thomson and Fine, JAOT, 2003, including curve segmentation aforementioned methods.

Thank you very much for suggesting this paper. I remember I came across it at the beginning of my investigations and then I focused on the Chu and Fan method. I added it to Sect. 2.3 "common methods to identify AMLD". The reference was added.

l.235 et.seq. why these ad hoc parameters? 2-delta and 90% of the entire profile.

We added the following section to justify the selections:

l.240-244. I find confusing that computing the tangent of the angle phi causes issues but computing the angle does not.

Because the tangent of an angle returns positive values for angles between 0 and 90, and negative values from 90 to 180. Moreover, the tangent of the angle increases from 0 to 90 and decreases from 90 to 180. Hence, selecting the maximum angle between ascending positive and descending negative tangent values is not intuitive. Instead of adding a condition for positive and negative tangent values, we decided to calculate the angle, which is also more consistent with the method. Moreover, calculating the angle is more straightforward if someone is interested in analysing the magnitude of the density gradient at each meter depth.

l.299 again density layers vs density levels

Changed.

l.320, Table.2. I miss an explanation for exploring linear regression and 'one-to-one' regression. Should intercept of regression be forced to cross zero for any reason?

We decided to test the simplest hypothesis where the DMC and the density level are at the same depth in the water column. Hence, they locate along a one-to-one linear regression. A forced linear regression with intercept equal to 0 and a linear regression without any limitation are tested. The intercept equal to 0 and a

slope equal to 1 refer to the one-to-one linear regression. If the intercept is 1, and slope is 1, the depths of DMC (CMd) and DL are lagged by 1(e.g. CMd= 1m, DL=2m).

Section 3.3.1. I find too many numeric details and data in the text, should be embodied in tables or figs. Same issue in 4.2.

The sections 3.3.1 and the details referring to Chl-a shapes in 4.2 were deleted with Chl-a shapes.

#REVIEWER 3

L. 19 in the abstract: instead of "<=120 m", consider "depth <=120 m".

Changed.

L. 42: Maybe "processes" instead of "effects".

We agree the right word is "processes".

Some parts that could be removed or significantly reduced in the introduction, since they seem repetitions or they are not very informative with respect of the MS objectives:

L. 46-47: "The vertical […] in the marine environment".

Done.

L. 66-68: "The exclusive […] needs to be investigate further".

Done.

L.82-90.

We believe that this section should be left since the main scope of the paper is the mixed layer depth (MLD). These lines refer to reviews mentioning other methods, and papers describing different approach to measure the MLD.

L. 95: Are you meaning "the distance" instead of "the depth"?

We intended the depth at which e.g. the pycnocline starts and the upper mixed layer ends. Hence, we changed the sentence from "depth between […] the surface mixed layer depth" into "depth between the pycnocline and i) the surface mixed layer".

L. 101: "the performance of these two proposed density layers" can be misleading, since it is not evident what a density layer performance mean. Maybe it could be rephrased with "we compared results with other relationships between density layers and Chl-a proposed in literature".

We changed the whole sentence into "The vertical distribution of density and Chl-a profiles are compared and the ecological relevance of BMLD in investigating subsurface Chl-a is detailed."

L. 112: I suggest to consider to replace "identify" with "to identify".

Changed.

L. 113 “is evaluated by comparing the vertical distribution of subsurface Chl-a”: to clarify the comparison cited in the sentence, I suggest to consider the following rephrasing “is evaluated thanks to comparison of BMLD with the vertical distribution of subsurface Chl-a.

We changed the sentence into “a new method to identify BMLD is proposed, and its potential is evaluated by comparing it with the vertical distribution of subsurface Chl-a”.

L. 121: The indication of the years (from 2000 to 2014) can be moved at L. 118, where the time length of measurements is cited firstly.

Changed.

Some details about instruments could be probably removed:

L. 122-123: “Temperature and conductivity […] editing procedure”.

Removed.

L. 130-133: From “In situ” to the paragraph end.

Removed.

L. 141: “predict” is a word that is usually relate to forecast, in this sentence maybe “interpolate” is more appropriate.

We changed “predict” with “interpolate”.

L. 144 From “The pre-processing” to the paragraph end: my impression is that this sentence can be shortened removing non-necessary details, or delated.

We deleted the sentence to avoid repetitions.

L. 152: Maybe DCM is a more usual way to identify the subsurface (or deep) Chl-a maximum. However, I understand that the authors are aiming at defining an abbreviation for the depth of the Chl-a mximum (that is not strictly DCM, indeed); I suggest to consider CMd (Chl-a maximum depth) to avoid confusion with DCM.

DMC changed into CMd in the whole paper.

L. 154: Here Eq. 1 is cited, but It appears three page later. Usually equations are cited more closely to their appearance in a MS. Consider to move the equation and the first time it is cited closer.

We deleted the reference to the equation here since we prefer it later on. We also moved and changed this section to supplementary materials. We changed the sentence with “, by using the adapted Chu and Fan (2011) method described in Sect 2.4 and Supplementary materials”.

L. 164-165: Consider to rephrase as follows: “Only 2% of the profiles were excluded from the dataset due to unclear subdivision or very different shapes.

The section was deleted together with Chl-a shapes.

Fig. 2: It would be more consistent to indicate with a letter (a, b , etc.) each sub-plot of the figure (the left plot is not labelled with a letter). In the right plots, the “Depth” arrow should point toward the bottom.

The Figure 2 include only a density profile now. Hence, it has been simplified to one plot.

L. 182: I think that “rectangles” is more suitable than “squares”.

The part of the figure with Chl-a shape was deleted.

L. 186: “Among” (capital A) instead of “among”.

Changed.

L. 200: Maybe “maximum squared buoyancy frequency” instead of “maximum buoyancy frequency squared”?

Changed.

L. 210-216 illustrate characteristics of AMLD and BMLD and methods to identify them, however AMLD definition and identification methods are discussed also at lines 189-195. Consider to condensate in a unique paragraph.

We gathered these sections into 2.2.

L. 224-227 seem a repetition of the strategy adopted in the MS.

We fixed this repetition by re-writing section 2.2.

L. 228-292: Please, consider to move detail of this method to an Appendix.

Advice accepted. The section 2.3 and 2.4 were changed and a large portion of 2.4 is moved to supplementary materials.

L. 346-360: these lines contain some repetitions of details provided in Methods section. They can be significantly shortened or removed.

This section was integrated to methods 2.2 to avoid repetition and allow the reader to understand what is considered a correct or a wrong identification in our method. We think that the methods are now eased and support the use of the function. To date, the details of the code are described in the supplementary materials and on the GitLab repository https://github.com/azampollo/BMLD.

L. 392: A bracket is missing after Fig. 4c.

Bracket added.

L. 440: “amount of phytoplankton” is perhaps misleading, since chlorophyll is evaluated here (and not phytoplankton biomass).

This section was deleted.

L. 514-516: “demonstrates” seems quite strong in this context. Please, consider “suggest” or “indicate”.

“demonstrates” changed into “suggests”.

L. 649-651 and L. 655-660 provide valuable discussion points.

This section was deleted together with the Chl-a shapes. The discussion has been changed and we hope now is better structured.