BG-2020-76

Link to previous manuscript

- https://www.biogeosciences-discuss.net/bg-2020-76/

Answers to Reviews

We thank the editors and reviewer team for their insightful comments and precious advice.
Since several comments coincide between reviewers we decided to address their comments in this common document, which is addressed to all reviewers. The reviewer's comments are thus reproduced here in their integrality (in blue), with specific answers including when relevant references to comments from other reviewers. Some of the most critical remarks result from imprecise wording, leading to misunderstandings. Below, we propose some rephrasing of the respective paragraphs, so that these misunderstandings can be avoided. Finally, we'd like to express here our general gratitude to the reviewers and appreciation for the pertinence of their remarks. References are given relatively to the revised manuscript in the form RLxxx.

In summary, to address the main reviewer's comments we have:

- Extended the introduction, for a better positioning of the novelties of our research ->see ***2.3, 2.4, 2.9, 2.11, 3.12, 3.13.***
- Enforced the links between the recent (Argo) period and the longer-term period (1955-present) ***3.12, 3.13.***
- Revised the description of the statistical approach, and streamlined appendixes -> see ***4.6, 4.7***
- Adopted oxygen saturation concentration -> see ***1.7***.
- Clarified technical points, regarding
    - AIC derivation -> ***4.8***
    - Statistical model from atmospheric conditions -> ***1.13***
    - Independence between distinct time-series -> ***1.2***
- Alleviate miscomprehension due to lack of clarity -> ***2.8, 2.9, 2.10, 4.11***
- Restate clearly the new insights of this study, and develop the discussion in that regard -> ***1.3***
- Changed the title ***-> 1.1***

Reviewer #1

The manuscript by Capet and co-authors presents a statistical analysis of cold intermediate layer (CIL) content and formation and its impact on oxygen levels in the Black Sea from the 1960s to 2019 combining different data sources (incl. observations and models). The two key findings are: (1) temporal changes in CIL water can only be described well by a model when taking into account regime shifts; (2) CIL water formation has entered a new warm regime (i.e. low formation) around 2008, which affects oxygen levels (through reduced ventilation) and is likely to also affect the biogeochemistry in the Black Sea. The manuscript is concise, well written and generally easy to follow. The presented statistical analysis seems sensible and appropriate to address the posed research questions.

However, I need to state clearly that I am not very experienced in complex statistical analysis and it would be good if someone more knowledgeable in that field could evaluate this aspect of the study.
-> We believe Reviewer 4 fulfills this role and we thus refer to our answer to Reviewer 4 for statistical considerations.

Most of my comments are only minor, primarily ask for some clarification, and should be easy to address by the authors. I have only a few somewhat bigger points:

1.1 *The study does not isolate the climate change impact (as suggested by the title) on the change in Black Sea ventilation. In my understanding, this cannot be achieved by the applied method, hence, I suggest adapting the title as a simple fix.*
-> This point has been underlined by several reviewers. The title has thus been modified, following the suggestion given by Reviewer1, ie. “A new intermittent regime of convective ventilation threatens the Black Sea oxygenation status”.

1.2 *It is not clear from the descriptions of the datasets whether the models are completely independent from the observational data, i.e. whether the same observations have been used for model calibration. If that’s the case, this needs to be discussed.*
-> Table R2, now added to Method section describes the direct and indirect dependencies that may result from the construction of the 4 times series.

- We now refer explicitly to this discussion in the sentence L151-153.

1.3 *In the discussion, the authors should highlight more explicitly what the new insights of this study are compared to previous work on the topic (I understand it is the regime shift).*
First, following comments from Rev2, we extended the introduction, in order to better position our findings with respect to those of previous studies.

Considering the points addressed in this review, we can reformulate our main findings as follows :

- We issue an unified, synoptic metric to characterize inter-annual variations in CIL formation. The consistency between independent sources demonstrates the accuracy of this metric. Note that some dependencies exist among certain sources, this is discussed explicitly. To our knowledge, no multiple source comparison have been achieved previously over such an extended period. This is now expressed explicitly in RL293-295

- Analysis of the time-series reveals a current restricted ventilation regime that is unprecedented over the last 64 years. Interannual variations appear to be better described by considering abrupt change models than smooth dynamics. This is discussed at length in the discussion section, and has been reinforced with Fig. 7, discussed on RL 304-309
- In particular the actual regime includes years during which **no** new CIL waters are formed, which appears to be a new phenomenon. We highlighted this in conference proceedings (EGU GA, May 2018, Kiel conference on marine deoxygenation, October 2018) . Stanev et al. 2019 later discussed this issue on the basis of Argo observations. Here, we propose a long term multi-source description of the CIL dynamics, to insist on the anomaly of this feature in regards to an extended period.
- The general relationship between CIL formation and oxygenation of intermediate waters has been previously described in the literature. Here, we exploit the dense Argo data sets (7 years) to document in more detail and at a finer scale the response of oxygen conditions to CIL formation at different pycnal levels. In particular, we illustrate the fast decrease in oxygen conditions for years where no CIL water was formed. The fact this detailed illustration of the CIL ventilation effect at different pycnal layers was permitted by the combination of Model and Argo data is explicitly stated in RL363-365

The whole study stresses the current prevalence of a restricted ventilation regime, its impact on the Black Sea oxygenation status, and the urgency of research efforts dedicated to alleviate or at least foresee potential environmental and economic consequences.

1.4 *The appendix seems to be incomplete as there are references to non-existent figures.*
-> Figures A1 and D1 have been erroneously placed at the very end of the pdf, which is probably why the reviewer missed those. Those figures being quite heavy, I've also experienced some lags while scrolling down to that part of the pdf on the Copernicus website. It has been ensured that all figures are distributed coherently in the revised manuscript. However, following the advice of reviewer 4, Appendix D has been removed, and replaced by corresponding addition in the text (RL212-215). The specific issue of Appendix D is further addressed in ***1.7***

Specific comments
1.5 *Title: I suggest changing it to "A new intermittent regime of convective ventilation threatens the Black Sea oxygenation status" as the study cannot really isolate the climate change impact from that of regional atmospheric oscillations.*
-> Agreed. see **1.1.**

1.6 *L58: This should also be mentioned when describing the statistical model using atmospheric forcing in the Methods*
-> Several comments pointed towards a more explicit description of this statistical model. We centralized our answer in **1.13.**

1.7 *L71/72: I am not fully convinced that your analysis actually separates the convective ventilation from the biogeochemical processes (BGC). The observed oxygen from Argo is affected by both physics and BGC. In* *order to isolate the physical (ventilation) component, would it make more sense to analyze oxygen saturation concentration (and AOU for changes in BGC)?*

-> For the sake of simplicity, and according to the suggestion, all oxygen considerations are now re-issued on the basis of saturation concentration (ie. in situ concentration as a percentage of oxygen solubility at in situ conditions). We attempted to indicate with Appendix D and Fig D1 that, for the layer below sigma 14.3 kg/m³ there is a quasi-strict linear relationship between molar concentration and saturation concentration, which is due to the fact that waters below these layers have a very narrow range of thermal and haline variability, so that the oxygen solubility only vary in a very narrow range. The consideration of molar concentration or saturation concentration thus bears quasi-identical results in our analyses. However Rev4 suggested removing appendix D and we agree with this simplification.

Also, the referred paragraph has been rephrased : "*Our analysis thus aims to pursue these investigations, and in particular to focus on the annual convective ventilation as an individual component of the complex Black Sea deoxygenation dynamics (Konovalov and Murray, 2001), in the context of the recent warming trend affecting the Black Sea (Miladinova et al., 2018)*". To our knowledge, a strict distinction of the ventilation and BGC terms should involve a dedicated model study which is left for further work. Please consider also related issue ***2.4.***

1.8 *L73-76: Maybe this "section list" is not needed? If you keep it, the last sentence seems to be incomplete.*

-> We'd like to maintain the section list to support partial reading. It has been completed.

1.9 *L85: Assuming that density increases with depth, this density criterion only defines the upper limit of the CIL. How is the lower limit defined?*

-> On the basis of the temperature threshold. 'CIL points' from a profile are required to meet both temperature and density criterion. While the density criterion is needed to define the upper boundary, the temperature criterion is sufficient to identify the lower boundary, due to the stable higher temperature of deep waters.

We reformulated the sentences LXXX in that sense

: *"" Although the use of a given temperature threshold to define the occurrence of convective mixing is subject to discussion, the existence of a fixed temperature threshold to characterize the CIL as a distinct water mass, and* ***in particular to identify its lower boundary****, is evident given the fixed value of ~ 9 ◦C that characterize the underlying deep waters (Stanev et al., 2019). "*

1.10 *L111: Include the overall number of Argo profiles used and also include the minimum and maximum number for individual years (and state corresponding years) in order to give an idea about sampling error.*

-> We added "*In average, this set includes about 9 floats per year, with a minimum of two floats for 2005 and more than 12 floats from 2013 to 2019.*" RL148-149

1.11 *Given the time series of CIL content rate of change (Fig. 6), I am also wondering whether it would make more sense to only use summer/fall profiles (as rate of change is very small during that period; analogous for the other datasets)?*

-> The answer is technical, and we prefer to avoid overweighting the manuscript. We answer the reviewer's question here, but opted not to include it in the manuscript for the sake of readability, considering that it is sufficient to state clearly that an annual averaging perspective is considered for all sources.

The most difficult time series to compose is probably the one from ship-cast profiles (ie. "Diva"), since it has to compose with (strongly) uneven sampling (both seasonally and spatially). This time series is extensively described in a dedicated publication (doi.org/10.1007/s10236-013-0683-4). In the present manuscript, we consider strictly the time series described in this publication (no temporal extension or revision of the methodology), in order to be able to simply refer to this previous work.

The time series is obtained as annual "trends" which is a by-product of the DIVA detrending methodology. To make it short, it corresponds to annual "additive anomalies" associated with observation points in order to consider these measurements in the construction of a long-term climatology. In particular, those annual trends are identified conjointly with seasonal trends, in the frame of an iterative process. So the notion of "annual average trend" is intimately embedded in the procedure. From thereon, the same "averaging-window" was adopted for each data source, as a requisite for comparability.

1.12 *Table 1: I suggest listing the different datasets in the same order as in the text.*
-> Order of items in the text and in the table are now consistent.

1.13 *I understand from the text description that the statistical model, based on atmospheric predictors, uses more than one (winter air temperature anomalies) predictors, right? If so, please state all of them either in the text, in the table or both.*
The statistical model used for this time series is now explicitly given at RL122

1.14 *For convenience, the time periods covered by the different datasets could be added to the table.*
-> Time periods have been added to Table 1

1.15 *L150-151: What is the smallest number of overlapping years between datasets? Is it appropriate to calculate correlations in the cases of least overlap?*
-> Those numbers are given explicitly in figure A1 that was erroneously transferred at the very end of the pdf. Smallest number is 8 years between "Argo" and "Atmos" time series, providing a p-value of <0.002 for the correlation.

1.16 *Regarding the atmospheric predictor model and the 3D hydrodynamical model, have any of the observational datasets (Argo and ship casts) been used to calibrate/develop either model? If so, those datasets would not be fully independent (during overlapping periods), which would affect the composite C time series (especially, its uncertainty as it may in-/decrease if they were in/dependent) and should be addressed in the discussion.*
-> This point of inter-dependencies is answered in **1.2.** The question of the composite time-series construction is addressed in **4.5** (note that model time-series that may be affected by dependencies are given a smaller weight in this composition). We did not attempt to estimate the uncertainty associated with the composite time series, as deriving uncertainty estimates for individual sources in a way that is consistent and allows a combined estimation of uncertainty for the composite time-series would be too complex.

1.17 *L156: I think Appendix A is incomplete as I couldn't find the information referred to here*
-> Indeed, the information was to be found on FigA1 (at the end of the manuscript).

1.18 *Section 2.3: It would be nice to show a time series similar to Fig. 1a also for oxygen in order to put the more recent development in CIL oxygen into historical context. I understand that this is not possible for the two*

models but perhaps it could be done for the ship casts? This would be particularly useful with respect to climate change.
-> The long term relationship between oxygen and CIL cold content extracted from ship-casts has been explicitly re-illustrated in Capet et al 2016, Biogeosciences, including some of the present Argo data. We believe it is inappropriate to reproduce this here. Yet, following suggestions from Rev3, we now address the CIL-oxygen in large scale historical context (based on World Ocean Database data), using a TS diagram -> see. **3.13**.

1.19 *L174/175: I am wondering whether the recently submitted work by Gordon et al. (https://doi.org/10.5194/bg-2020-119), who suggest a correction of BGC-Argo oxygen observations based on the sensor response time, could help to make use of both descending and ascending profiles?*
-> Thanks for this very interesting reference. In addition to the fact that complication arises to use both ascending and descending profiles, and although we understand that these complications can be technically alleviated on the basis of the proposed reference, we considered the following additional arguments : 1) descending and ascending profiles are very close to each other since they pertain to the same sampling cycle. While those might be relevant when considering very short time scales, they are quite redundant at the time scales of interests, and 2) in the dataset obtained from the coriolis center with the given selection criteria, we obtain 1800 descending O2 profiles and about 250 ascending profiles. We thus consider that including ascending profiles is not worth the burden of technical description in this precise case.

1.20 *L203: It's probably my personal taste but I don't really like the term "routine" regime. Maybe "standard", "normal" or simply "average"?*
-> "Routine Regime" has been replaced by "standard regime". Thanks for the suggestion.

1.21 *L213-215: Would it make sense to show annual (or winter) average surface air temperature as a small panel in Fig. 5 to better demonstrate this link?*
-> In L213-215, we referred to 'warm regime' in terms of CIL cold content. Besides, the "Atmospheric" time series is explicitly based on winter temperature anomaly, in a cumulative way which is more directly relevant to the CIL cold content (cf. **1.13**).

1.22 *L218: More out of interest: would it be possible to also get intra-annual resolution from the statistical model? E.g. by not using winter time averages of the descriptor variables but monthly averages or so?*
-> We believe this level of detail is beyond the scope given to the paper. Detailed works on CIL formation and seasonal development have been done and are referenced within the manuscript. See in particular Capet et al, 2014, Akpinar et al 2017, Miladinova et al 2018. Also in L218 and paragraph, the idea is to quantify CIL formation and destruction rate from the difference between consecutive stocks. This can only be done on the basis of complete inventory such as can only be obtained for the synoptic 3D model product.

1.23 *L225: I don't understand what you mean with "before, during and at the end the thermocline setting". Mainly because I am not sure what you mean with "thermocline setting"; is it the thermocline formation or the period during which a near-surface thermocline exists?*
-> Poor wording indeed. "thermocline setting" has been replaced by "thermocline season".

1.24 *L231: Oxygen is from Argo. Would it make sense to do the intra-annual comparison of CIL formation rates also based on Argo then (or a combined C using 3D model and Argo)?*
-> This relates also to the answer given in **1.22**. CIL formation is not achieved at once, nor in a single location. Rather it proceeds from multiple small scale events in the open part, and in the northwestern shelf (see Akpinar 2017 and Miladinova 2018 for respective importance of both formation pathways). Argo sampling of the CIL is incomplete (important spatial variability and subsampling). Although we assumed to use it to build an annual metric of CIL content (with the caution notice of Sect. 2.1), we wouldn't use Argo to derive CIL formation rates at smaller temporal scales which demands differentiation between complete inventory estimates.

1.25 *Discussion: As stated in the very beginning, it would be good if differences to earlier studies/novelty would be highlighted more explicitly. In my understanding, the main differences in terms of methodology are the longer period and the basin-scale integrated approach, which is needed to detect the regime shifts. This needs to be made more clear and it should also be discussed what the advantages (for the purpose of this study) and possible limitations are.*
-> see **1.3** for novelty. **The assets of considering the regime shift paradigm is discussed as follows : (RLXX)** *"Although, we acknowledge that the statistical advantage (AIC) of the regime shift description is subtle, we consider that it deserves further consideration as this difference in interpretation is fundamental in what regards the expected consequences on the Black Sea oxygenation status and in particular the threat on Black Sea marine populations, whose ecological adaptation (and rate of exploitation) have been built upon a ventilation regime and consequent biogeochemical balance, that may no longer prevail."*
The assets of combining different dataset in our analysis is now underlined in the sentences*:*
RL354-356 : "The consistency between those products, and in particular the close correspondence between observational and mechanistic predictive time series supports the reliability of the composite series and its adequacy to describe the evolution of the Black Sea subsurface convective ventilation during the last 65 years."
And *RL 363-365 "The synoptic CIL formation rates provided by the 3D hydrodynamical model, and the detailed description of oxygenation conditions provided by BGC-Argo floats, allowed us to detail the role of CIL formation in oxygenating, through convective ventilation, the upper part of the Black Sea intermediate layers".*
The asset of considering multiple data sources to extend the temporal extent the of study is mentioned in RLXX "*This composite time series is built from four different data products issued from observations and modelling, so as to optimize its temporal extent in regards to preceding studies \citep[e.g.][]{OGUZ2006}.*"

1.26 *L286/287: If one of the main conclusions focuses on the combined linear-periodic model then this combined model should be included in Figs. 3 and 4.*
-> In fact Fig. 3 and 4 have been removed. However, we completed the "descriptive models" section with explicit inclusion of the linear-periodic model. Please Also consider ***4.9*** and ***4.10***

1.27 *Appendix: There seem to be two figures missing: Fig. A1 (L307/308) and Fig. D1 (L379).*
-> Both figures A1 and D1 appear on the very last page of the pdf, available at the BG discussion website. As D1 is relatively heavy, scrolling down that page is somehow slow, which may explain why it has been overlooked. A1 is now placed consistently, while D1 was removed.

1.28 *The appendix contains a few aspects that would fit into the discussion in most journals (incl. BG), e.g. the discussion of the suitability of the statistical methods. I assume this is owed to the previous submission to GRL. The authors may consider moving parts of it into the discussion (or even into the Methods, e.g. the statement on oxygen data on L378-380). However, it also works the way it is. Maybe the editor can have a say on that.*
-> Appendix A is maintained as was. Appendix D has been suppressed, with essential parts included in the Methods. As concerns appendix B and C, please refer to **4.6** and **4.7** (essential part of the regime shift analysis method has been embedded in the text, details have been maintained within a streamlined appendix).

Technical corrections:

- *L2-3: "from the mid-1970s to the early 1990s"; specify "recent years", e.g. "post-2005"? comma after "Here"* *-> Done*
- *L6: maybe "years without renewal of intermediate water"?-> Done*
- *L7: "density levels"-> Done*
- L15: "While the reduction" *-> Done*
- L20: "at the surface" *-> Done*
- L24" "Strait" *-> Done*
- L27: no comma after "(halocline)"; remove "the" after "prevents"*-> Done*
- L33: "winter time"*-> Done*
- L40: "upper ~100" *-> Done*
- L41: "forcing"; "e.g." (check throughout the manuscript, also for "i.e.", the first "." is often missing) *-> Done*
- L42: give time scale for alterations, e.g. "(order of days)" *-> "relatively fast inter-annual alterations"*
- L53: "Miladinova et al. (2018)" *-> Done*
- L57: "feedback" *-> Done*
- L62: (2005-2018) *-> Done*
- L63: "trend leading to conditions" *-> Done*
- L65: "with increasing trends" *-> Done*
- L67: comma before "in particular" *-> Done*
- L68: remove "extended" *-> Done*
- L71: "an individual component" *-> Done*
- L74: "regime shift analysis"*-> Done*
- L78: use full term for CIL in section heading *-> Done*
- L81: "provides" *-> Done*
- L87: "water mass" *-> Done*
- L89: "for consistency with existing literature" *-> Done*
- L93: "i.e." *-> Done*
- L99: "dataset" *-> Done*
- L100: I'd suggest "errors" instead of "artifacts" since one usually uses the term "sampling error"; space before "Each data"*-> Done*
- L102: "netCDF"*-> Done*
- L107: "This data was" *-> "This dataset was"*

- L110: “Good].”-> *Done*
- L115: Could you provide the total numbers of profiles in the central and peripheral basin regions? -> This would ask to define properly both regions, and appears as ineffective regarding the manuscript’s objectives. However, the sentence has been rephrased : *“As Argo samplings are generally more abundant in the peripheral regions, i.e. outside of the divergent cyclonic gyres, this suggests that C^{Argo} might be slightly biased towards high values.”*
- L123: again I’d suggest “errors” -> *Done*
- Table 1: “guaranteed” instead of “granted” (under drawbacks for atmospheric predictors); “Three-dimensional” instead of “3D” in the rationale for GHER3D-> *Done*
- L124: please cite the “reference study” here-> *Done*
- L126: “consists of” -> *Done*
- L129: remove “defined earlier”-> *replaced by “defined by” + reference*
- L134: please cite the “reference study” here -> *Done*
- L136: “Three-dimensional (3D) hydrodynamic model” -> *Done*
- L136-144: Please include the time period for which the model was run. -> *Done*
- L147: “metric for the” -> *Done*
- L151: specify those series with little overlap ” -> *Done*
- L154: here and throughout the manuscript: no space between “M J” in the unit; just “MJ” -> *Done*
- L159: “inter-annual” -> *Done*
- L161: “i.e.” theme(legend.position ="none")+
- L164: “models’ ability” -> *Done*
- L168: “i.e.” -> *Done*
- L169: “which” instead of “that” -> *Done*
- L171: “shift” -> *Done*
- Fig. 1:
    - add legend; -> *If the reviewer agrees we prefer to describe the legend in the caption*.
    - larger panel and axes labels; -> *Done*
    - “MJ” instead of “M J” in y axes units; -> *Done*
    - no “-“ between quantity and units on y axes (this applies to all figures); -> *Done*
    - caption: “Table” instead of “Tab.”;- > *Done*
    - “time series” instead of “trend”; -> *‘Trend’ refers specifically to the DIVA methodology*.
    - why does the statistical model show a range and what value (e.g. mean) of that model was used for the analysis?-> *Caption completed “confidence bounds (p<0.01)”.*
- L181: “Argo floats were operating” -> *Done*
- L182/183: “Argo floats profiling” -> *Done*
- Fig. 2: larger axes and panel labels; caption: “Argo floats” -> *Done*
- L185: use full term for CIL -> *Done*
- L187: “in Fig.” -> *Done*
- L188: include use latex command “\cdot” instead of “.” In “I1.i”; state what “i” is -> *Done*
- L193: “overestimate” -> *Done*
- Fig. 3:
    - in the legend, you use “Model3d”, while you use “C^3dModel” on L140 (I suggest to use one term consistently); -> *All changed to Model3D.*

    - The figure has been removed.
- L196: “shift”; “i.e.” *-> Done*
- L198: “;” before “not shown” *-> Done*
- L199: “i.e.” *-> Done*
- Fig. 4: caption: “shift”; “point” *-> Done*
- L200: suggest using full term for “CIL” *-> Done*
- Fig. 5:
    - here you use “3D model” in the legend (different to Fig. 3), be consistent; *-> All changed to Model3D.*
    - caption: “blue shaded area” looks more purple; “gray shaded area” very hard to see, perhaps make slightly less transparent; no comma after “i.e.” *-> Done*
- L207: no comma after “e.g.” *-> Done*
- L209: “;” after “oscillation” *-> Done*
- L210: “20th”; “1950s; Ivanov et al., 2000)).” *-> Done*
- L213: “prevails in the Black Sea since about ten years.”; “low cold content”*-> Done*
- L217: suggest using full term for “CIL” *-> Done*
- L219: “before 2008” instead of “in precedent regimes”; specify “the latest period” *-> Done*
- L224: “period P2, P1/3 and P4, respectively.” *-> Done*
- L226: for better readability: “about -1 MJ” *-> Done*
- L227: “in more detail” *-> Done*
- L228: “quasi-absence” and add reference to figure panel *-> Done*
- L229: “during” instead of “among”; “depict” *-> Done*
- L230: “while lower”; “simulated” or “shown” (this figure is not based on observations) *-> Done (“shown”)*
- L231: unit missing for “16.0” *-> Done*
- L232: “increases”; “in the years 2012 and 2015—2017 when CIL formation is significant”*-> Done*
- L234: no space between “~ 14” *-> Done*
- L241: “decreases continuously” *-> Done*
- L242: “remain”; space before unit *-> Done*
- L247: “regime shift model” instead of “first” *-> Done*
- Fig. 6:
    - the yellow dots in P4 panel are difficult to see, suggest using different color; *-> Done*
    - add panel labels (a, b, c, d) for mores specific in-text referencing; *-> Done*
    - add “regime name” to panel titles; *-> Added “Period”*
    - caption: “time series”; “in Fig. 5” *-> Done*
- L250: “built” *-> Done*
- L251: “prevail” instead of “be considered as routine”? I think it’s more important to emphasize that earlier assumptions might not be valid anymore *-> Done*
- L254: “trend” *-> Done*
- Fig. 7:
    - the shaded areas are hard to see, maybe make them a little less transparent? *-> Done*
    - Caption: “areas”; “.” at end of caption *-> Done*
- Fig. 8:
    - significance is expressed by p-value, so why additional log10 of it?*-> Removed*

    - “Pearson Correlation Coefficient”;-> *Done*
    - Labels and legend should be a bit larger
- L265: “i.e.”-> *Done*
- L269: “Sea” -> *Done*
- L271: put “, respectively” at end of sentence -> *Done*
- L284: “which has been” -> *Done*
- L286: space before “Statistical”; “indicate”-> *Done*
- L289: “i.e.”; parenthesis not closed after sigma values -> *Done*
- L297: “feedbacks” -> *Done*
- L301/302: all links should be included in that section, not as footnotes-> *Done*
- L306: “denoted” -> *Done*
- L310-317: equations should be numbered; in the RMS equation, “N^m,n” should be under the square root; in the relative bias equation, use “\cdot” instead of “.” -> *Done*
- L319: “;” before “Fig. 1”; no “.” after “Fig. 1” -> *Done*
- L321: add reference for “published prognostic values” -> *Done*
- L331: “p-value” with cursive “p” -> *Done*
- L334: “six reject”-> *Done*
- L340: “i.e.”; use “Akaike Information Criterion (AIC)” as you refer to this appendix before introducing AIC in the main text -> *Done*
- L341: this should be “Appendix C” (afterwards the numbering of appendix sections B1/B2/C1 seems off) -> Solved.
- L343: “guaranteed” -> *Done*
- L356: “Table C1; the”-> *Done*
- L360: “Table C1” -> *Done*
- Table C1, caption: “second column” and “third column” -> *Done*
- L365: “coverage”; comma after “For instance”-> *Done*
- L367: remove one “identified” -> *Done*
- L370: “in-situ data” -> *Done*
- L371: use “in situ” or “in-situ” consistently throughout the manuscript (preferably the former in cursive letters), here you use both-> *Done*
- L380: no space before the “.”; don’t use “in this study” instead of “in the following” -> *Done, i.e. replaced by “in this study”, unclear comment.*
- L381: The heading for the “Author contributions” section is missing-> *Done*

Reviewer #2

***2.1** The title sounds interesting and attractive. However, for me, the result sounds as follows: There were two cold winters in the period of 2012-2019, and in each of them was not only the amount of cold waters larger, but the concentrations of oxygen were also higher. There is nothing new, except that this has been recorded by BioArgo.*

This appears as a reductive description of the manuscript results.

Result section can be divided in :

- Construction of composite long term (1955-2019) time series for CIL cold content using four sources of different nature (models, observations), and assessment of the coherences and discrepancies between these sources.
- Analysis of the above time-series by comparing different descriptive models, incl. identification of regime shifts and detailing of the various regimes.
- Detailing the contribution of Cold water formation to oxygen ventilation (over Argo period), including vertical (pycnal) distribution of the ventilating action of CIL formation.

All those steps are presented with quantitative considerations that we believe to be relevant for further works addressing 1) Black Sea specific dynamics and 2) interlinks between ocean warming and deoxygenation.

2.2 *Neither, the scientific content of the manuscript supports this title.*
The title has been modified, following the suggestion given by Reviewer1, ie. “A new intermittent regime of convective ventilation threatens the Black Sea oxygenation status”.

2.3 *We read in the Abstract: “Oxygen records from the last decade indicate a clear relationship between cold water formation events and oxygenation status at different isopycnal levels, suggesting a leading role of convective ventilation in the oxygen budget of the upper intermediate layers.” This just repeats what has been known for many years. This finding is just a confirmation of previous knowledge (see several papers of Konovalov et al).*
We acknowledge that the term “suggesting” is inappropriate, and apologize for this. The fact that CIL formation contributes to ventilation is indeed not a novel discovery and wasn’t considered as such. As underlined by reviewer2 this relationship has been considered several times in the past Black Sea litterature, including in the works of S. Konovalov.

This is now explicitly mentioned in the introduction section RL73-80
However,

- considering this dynamics from the standing point of Argo profilers provided an insight at an intra-annual scale (ie. weekly), and enabled an detailed appreciation of the depth (pycnal) penetration of the CIL ventilation effect at an intra-annual time-scale, which is new.
- Here, we used Argo data to illustrate not only the ventilation effect, but mostly the absence of ventilation for the years where CIL ventilation does not take place, ie. in the context of the regime shift highlighted in the first part of the manuscript. This was done not to reveal the already known ventilation role of CIL formation but to highlight the potential consequences of a regime shift in CIL formation by revealing the absence of ventilation during the years where no new CIL waters are formed. This is now highlighted more explicitly in the Discussion Sect. 5 and Abstract.

2.4 *Possible shifts of temperature-oxygen relationships in different periods have also been broadly addressed in these studies. Authors say nothing about that.*
Shifts in the temperature-oxygen relationships is now explicitly mentioned in the introduction part. *(RLXX) :*
“Indeed, (Konovalov and Murray, 2001) evidenced a clear relationship between oxygen conditions in the lower part of the CIL layer, and the temperature in that layer which is directly related to interannual variations in the CIL formation intensity. This relationship explain a large part of the inter-annual fluctuations in

oxygen concentration in that layer, which occur at a time scale of a few years, and are superimposed on the larger scale change in oxygenation condition that is attributed to an increase in the primary production induced by the eutrophication phase of the late 1970s."

Our manuscript does not consider the material needed to address biogeochemical terms (eg. nutrients and production). Instead it focuses on highlighting and documenting the CIL ventilation term of the oxygen budget equation and more precisely the absence of this term for some years of the recent decade. The data considered to this aim (Sect 4.) cover a relatively short period [2011-2019], for which major shifts in biogeochemical regimes, such as the historical Black Sea eutrophication and recovery, can be conservatively ruled out.

Finally, shifts in the long-term oxygen and CIL cold content relationships have been explicitly illustrated in Capet et al 2016, Biogeosciences, and we did not wish to repeat these in the present manuscript. See however request from Rev3 (***3.12*** & ***3.13***), to put the finding of the Argo Period, in relation with historical perspectives. .

2.5 *This brings me to the major criticism. Authors have to make clear what the new knowledge is, which can be gained from their study compared to older ones.*
-> Please refer to ***1.3***

2.6 *One big problem is that there is a lack of balance in the manuscript. Much attention has been given to the long-term variability. On this subject, I cannot find anything new.*
Regarding the long term variability: To our knowledge this is the first time that CIL dynamics is documented using an unified, integrative metric over such an extended period, that multiple sources of information are compared, and that the regime shift analysis paradigm is compared to other descriptive models, which is important 1) to highlight the existence of thresholds in the response of CIL formation to atmospheric temperature and 2) to highlight the exceptional nature of the absence of CIL formation for some years within the currently prevailing regime.

2.7 *The intimate link between analyses of 65-year and 7-year time series is not clear. The good part of the research is the analysis of data after 2012. However, its relation to the previous periods is completely unclear, and neither is it well articulated in the manuscript.*
The last seven year period falls within the identified last regime, and is used to illustrate the impact of this regime shift on oxygenation conditions. The model time-series allows to interlink the long-term framework to the recent period, since it extend over both period while proceeding from a unique model setup (ie. forcings and physical assumptions remains unchanged for the entire simulation time). A dedicated effort has been deployed to better express this in the discussion section, which has been completed in agreement with 3.12 and 3.13

2.8 *In this part* [ie. analysis of data after 2012.]*, authors should clearly describe which floats they use, and how these floats capture the temporal and spatial variability. Important to know is whether what is observed is a clear signal or just noise. The statistics of data, and how representative for the Black Sea state they are, need deeper consideration. Fig. 1 shows perhaps that data used in the analyses are not homogeneous. If the data is not homogenous, the subsequent interpretations of long-term changes are not credible.*

The spatial and seasonal distribution of oxygen Argo floats is easily accessible on numerous platforms including the ones referred to in the manuscript (see for instance the display tool of the Coriolis website). Also, adding maps of floats positions was not considered as essential.
This, of course, does not dismiss the need to properly consider the representativity of the data.
Here the reviewer comments on data analysis after 2012, hence the part on oxygen although the Fig. 1, which is referred to, concerns the CIL cold content.

- Regarding the CIL cold content series extracted from Argo, there is an expected spread between individual floats that is indeed illustrated on Fig 1 and reflects the well known spatial variability in CIL cold content. In the sake of setting this data set on the same level as other data sources annual means were extracted from the Argo dataset (see. RL150). The resulting annual averages were afterwards compared to other data sources (see. Appendix A. and Fig A1) to ensure consistency of the analysis. We also added a sentence giving annual number of floats used to characterize the CIL content (RL 149)
- Regarding oxygen, all available oxygen Argo data were used considering data selection criterion that are described in Sect 2.3. The question of representativeness is considered and explicitly discussed on L. 214-220 and Fig. 2, which now includes the Argo's ID. In particular, it illustrates the spread of oxygen conditions derived from individual floats and shows that the use of a pycnal scale provides a sufficient spatial homogeneity to characterize a basin wide condition. Finally, Fig. 5 also provides the interquartile ranges of monthly-binned Argo-derived oxygen concentrations at different pycnal levels. Considering that Argos profilers are spread across the basin, we would expect much more inconsistency and overlap between the interquartile range areas if, as the reviewer suggests, spatial variability overpasses the temporal signal embedded in this dataset.

2.9 *The statement ". . . suggests that the CIL renewal, that was taking place systematically each year in precedent regimes, has now become occasional." contradicts what is known from earlier studies. They have to at least refer to Lee et al. (2002. Anthropogenic chlorofluorocarbons . . .) who claim that the residence time is ~5yr at 80-120m. I would recommend that they explain what the problem in this earlier estimate is, if any.*
We apologize here for a clear misunderstanding, due to inappropriate wording. Instead of "*complete renewal*" of the CIL layer as seems to have been understood here, we wanted to refer to a "partial renewal", ie. the formation of a certain volume of new cold water, adding to the remaining CIL content of the previous year and contributing to the gradual renewal of the CIL layer. We totally agree with the fact that the residence time of the CIL layer is larger than a year, this is indeed well known and is referred to in the revised RL60.: *"If a well-formed CIL was present during the previous year, subsurface waters exposed to atmospheric cooling are already cold, which increases the amount of newly formed CIL waters (Stanev et al., 2003). Due to this positive feed-back and to the accumulation of CIL waters formed during successive years, the inter-annual CIL dynamics is better described when winter air temperature anomalies are accumulated over 3 to 4 years, rather than considered on a year-to-year basis, which is in agreement with the 5 years upper bound estimate provided by Lee et al (2002) for the residence time in the CIL layer."*

2.10 *The issue of regime shift is not convincing. The question is: can oxygen data over 7 years only identify regime shift? What was the previous oxygen regime?*
There seems to be another misunderstanding here.

- The regime shift paradigm is used to characterize the dynamics of CIL formation, and is based on the analysis of 64 years long time series.
- Oxygen data are used to illustrate the impact that this regime shift in CIL formation may bear on the oxygen dynamics.

2.11 *What I see in the oxygen data are just two ventilation events, not a shift. Authors ignore referring to important works about regime shift (e.g., the review article of Oguz in Front. Mar. Sci., 25 April 2017). They have to study the references in this review. Based on my comments above, I am very sorry that I cannot recommend publishing.*

See ***2.10*** as concerns the remark on regime shift and oxygen. Note that we still want to consider ventilation aspects apart from other drivers. Again, the question of a shift in the biogeochemical terms of the Black Sea oxygenation balance is not directly addressed in the manuscript. We really want to focus on Cold water formation as the main ventilation mechanism, and to highlight a strong, sudden, non-linear reduction in this process, and to illustrate the impact it's likely to bear on Black Sea oxygenation status, regardless of other environmental shifts that may be happening in parallel.

Reviewer #3

This is a very interesting paper. Basically the authors:

1. Assemble a time series data set of temperature data from the Black Sea from about 1955 to the present combining real observations and model results.

2. They integrate cold temperature anomalies in the Cold Intermediate Layer (CIL), relative to a reference temperature of 8.35°C which defines the upper and lower boundary of the CIL, which they call "the cold content".

3. They analyze the (extensive) variability in this "cold content", using several different approaches (linear and sine functions), but settle on a technique they call the "regime shift" paradigm or hypothesis.

4. Using regime shift they identify four periods that characterize different amounts of "cold content"

5. They argue that these periods reflect variable degrees of ventilation of the CIL

6. The last 11 years have been a period with unusually low ventilation and they argue that this is due to ocean warming.

I have a few specific comments,

3.1 *The paper is a little hard to read because of the advanced data analysis techniques used and (though generally well written in English) some awkward word choices. I'm not sure who can fix that.*

-> We refer to exchanges with Rev4 as concerns the technical justification part that needed to be retained as essential to the main text, or that can be directed to the appendixes. A certain level of technical detail can not be avoided. As concerns the english, we hope that our careful revisions and the patient support of Reviewer1 will enhance the reading.

3.2 *L1 Abstract – a ~100 m ventilated surface layer is referred to but does that mean 0-100m No, it means the Cold Intermediate Layer which is more like 50 to 100m I think you should be more specific.*

-> No, in this sentence we indeed refer to the part that is not anoxic so 0-100m, and not 50 to 100m.

3.3 *L20 The early literature (e.g., Tolmazin 1985, Progress in Oceanography 15, 217) argued that as it appeared that the sea surface in the central gyres never got cold enough for replenishment of the CIL by winter convective, that the main source of water to the CIL on an annual basis was from the NW shelf where the key density surface was cooled. I agree that we have much more data now and starting with Gregg and Yakushev (2005), who observed a ventilation event (with real data), we now know that ventilation can occur from the central gyre regions. But the NW shelf hypothesis has been totally left out of all papers since the 1990s, such as those by Akpinar, Ivanov, Oguz and others. I looked back at those papers and they don't even mention the Tolmazin argument, much less argue why it would not play a role. So as far as I can tell, the NW shelf is a possible source of ventilated water for the CIL. If the Tolmazin hypothesis has been disproved, I missed that. I think Capet et al should take that into account. It may not show up in their model, depending on how it is parameterized.*
-> This important point is discussed extensively in ***3.7*** below.

3.4 *L29 Murray et al (1989) discovered the suboxic zone. Stanev et al (2018) is a nice paper but used model results to argue for what causes its origin.*
-> We have updated the reference accordingly. We apologize for this mistake.

3.5 *L104 Why not describe the data sources in the same order as presented in Table 1?*
-> We will reorder items in the table in order to have consistent ordering in the table and text (cf. ***1.12***)

3.6 *L126 How were the Atmospheric Predictors converted into C and CIL temperature variability in the water column?? I don't think anything is said.*
-> The entire procedure, and the resulting statistical model is explicitly detailed in the reference given (Capet et al 2014). We did not restate the procedure and results of this study, because we want to focus the present debate on new investigations and results. However, seeing this comment is coherent with request ***1.13,*** the equation of this lagged regression model has been given explicitly.

3.7 *L136 Does that 3D hydrodynamic model include source water from the NW shelf?*
-> The 5 km 3Dmodel, whose setup is described in the given references, does indeed resolve a shelf source of CIL waters (as well as a source from central gyres), but this is a result of internal dynamics, not a specifically imposed behavior. Because the reviewer seems interested in this question, I may refer to Fig 4 and 5, and Section 6.3 of A. Capet et al. / Deep-Sea Research II 77-80 (2012) 128–142, that uses the same model. It depicts a second EOF of CIL Cold content which could correspond to the relative importance between the two sources of CIL water formation, which relates well to a longitudinal gradient in sea surface temperature.

The more recent works of Miladinova et al 2018, further extends on detailing the relative contribution of the two sources of CIW. I understand and share the reviewer's interest. It would be relevant to investigate whether a trend exists in the respective importance of the two CIL formation mechanisms, and if these two mechanisms imply distinct biogeochemical consequences (they certainly do, as the biogeochemical signature of those water's origin are drastically different). However, we prefer to maintain the focus and the general 'integrated' point of view. Besides, making a distinction between CIW of distinct origins would only be possible for the 3D model. In short, this is a related and relevant question, but we consider it extends beyond the scope of the present manuscript. We've adapted RL134 to state that the CIL dynamics as resolved by the 3D model is explicitly described in this reference.

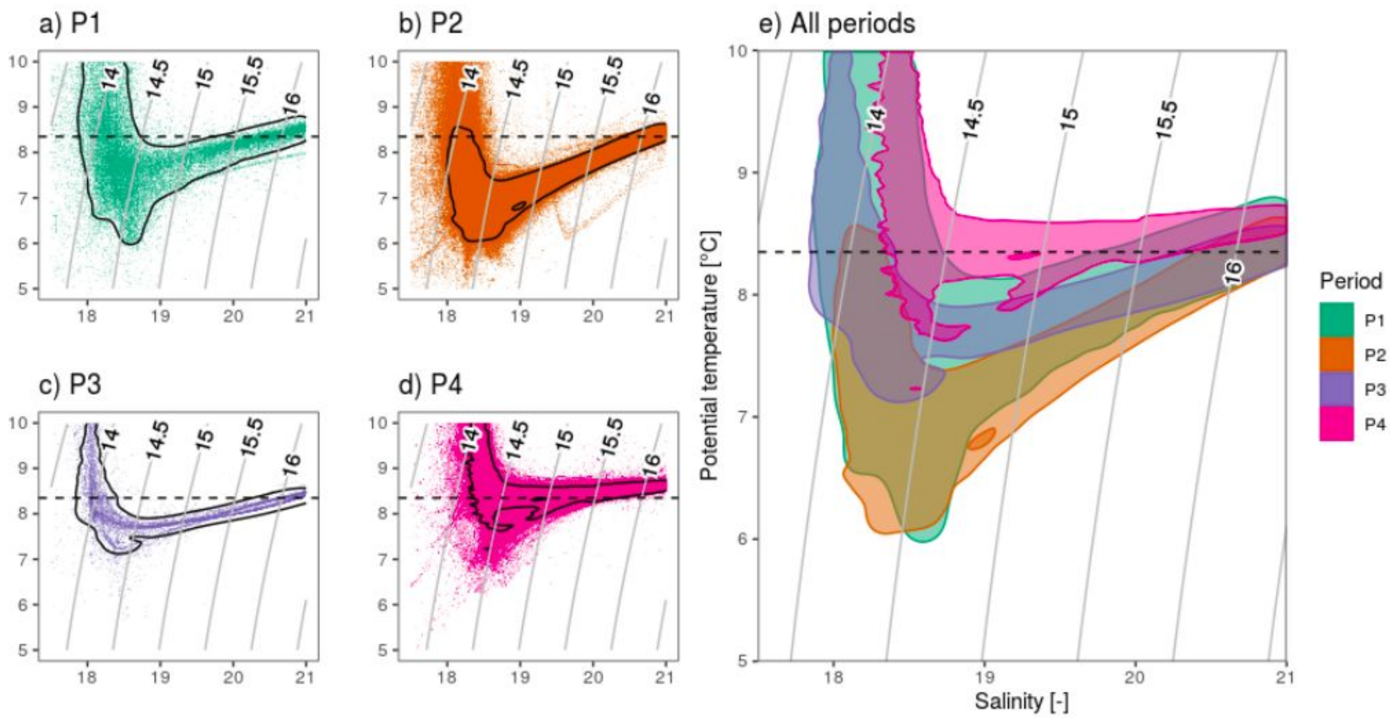
3.8 *Figure 5 with the intervals obtained from regime shift analysis is compelling. But I think back to the geological axiom "If I hadn't believed it, I wouldn't have seen it!". Visually (without the vertical lines) it looks like there is more variability than shows up as the std. deviations. But this is not my area of expertise so I hope other reviewers can evaluate that.*
-> See exchanges with reviewer4.

3.9 *L173 Section 2.3 The absolute values of O2 are uncertain unless the sensors are carefully calibrated. I suspect they were not. The relative changes are probably OK.*
-> Agreed. We only consider relative changes here. Also, we used monthly median values of multiple buoys with different ages we may assume an acceptable overall bias.

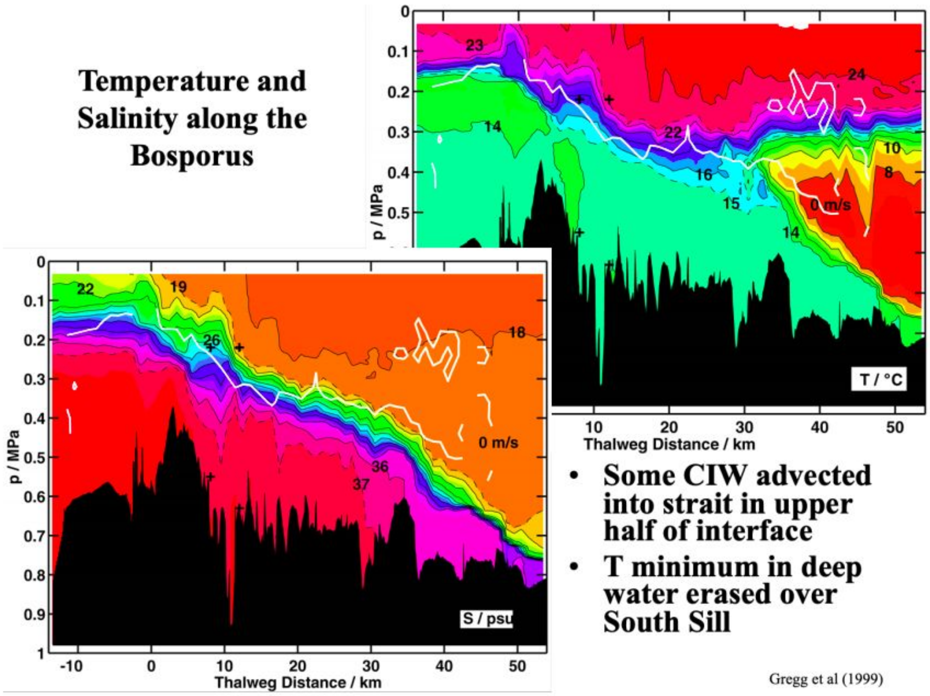
3.10 *L181 Why 2018 and not 2020?*
-> This is a mistake. 2010-2018 has been replaced by 2014-2018, ie. the period where multiple buoys are present at the same time, which better illustrates spatial variability.

3.11 *L289 I think Konovalov and Murray (2001) showed this in a figure.*
-> That is totally true. Please consider our answer in ***2.3***

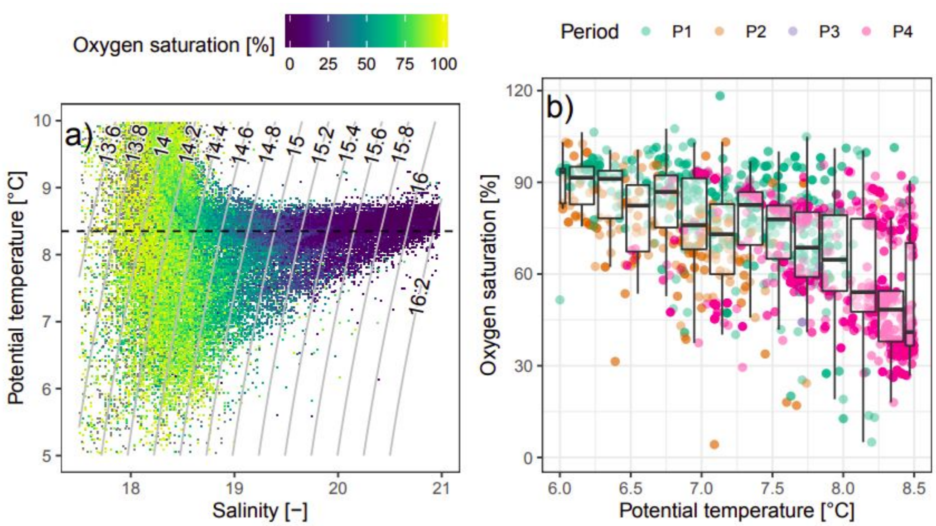
3.12 *I really prefer the real data over the model results, which just reflect what equations and parameters were put into the model. For this reason I would really like to see a T-S plot (with real data) maybe averaged (with std. dev.) for the 3 regimes, blown up to highlight the CIL region. The intervals in Figure 5 should show up clearly.*
-> We added the following figure in the discussion section to address this issue. Please consider the revised discussion section.



3.13 *I think it would be useful to explain another reason why the CIL is important. In my view (see Murray et al., 1991) it is because it plays an important role in the formation of all deep-water in the Black Sea. To a first approximation, all deep water in the Black Sea forms by linear 2-end member mixing between the Bosporus outflow and the CIL. This must be because salinity increases all the way to the bottom and the only source of salinity is the Bosporus. See Figure 12 of Murray et al (1991). This mixing occurs on the SW shelf (Latif et al., 1991). Any curvature in the T-S plots is due to temporal variability in the signature of the CIL endmember. This mixing can be seen in the T and S sections from the Bosporus to the shelf break from Gregg et al (1999). See Murray et al (1991) for more discussion. If the CIL is warmer, and less dense, how will that impact deep water ventilation? I think the deepest layers will be ventilated less frequently.*



-> We thank the reviewer for this very important comment. Keeping the focus on oxygen, we issued the following figure which represents oxygen saturation concentration on the T-S diagram (WOD data, all periods, bottles,CTD and profilers data selected with “good” quality flags)). Taking, for instance, the 14.6 kg/m³ isopycnal as the CIL end-member for mixing of deep waters, it clearly appears that this layer is less oxygenated in the case of low CIL content, which relates to the fact that the CIL layer oxygen signature proceeds from a balance between underlying biogeochemical consumption terms, and fresh oxygen imports associated with annual partial CIL renewal. Although we decline to explore the dynamical implications of a warmer CIL core on deep ventilation ventilation. We believe these considerations (please see revised discussion section) provides a link between the recent (Argo) period (fig 7 & 8), and the longer time scale as is required by Reviewer 2 (see. ***2.7***).



Reviewer #4

I was asked by the editors to review this manuscript with an eye towards an assessment of the statistical methods that were used. Oceanographically, the notion of different convective regimes and their link to Black Sea oxygen dynamics is established using (mainly) observational data. A causal relation, of course, cannot be inferred from this, but the physical linkages are plausible ones and great effort has gone into making sensible metrics for the cold intermediate layer, with a solid understanding of the limitations of each of the data types used. I don't know enough about the literature pertaining to convective processes, nor regional Black Sea oceanography, to comment on significance of this contribution and its novelty within oceanography. For the remainder of this this review, I will focus on the statistics and data analysis aspects.

The paper quantifies the ventilation of the Black Sea intermediate layers through a heat (cold) content metric, i.e. the CIL defined in eqn (1). Estimates for the values for the CIL over time are computed from 4 sources: the first two are observational data (ARGO floats and CTD casts); the next is output from a statistical model that relates air temperature to CIL; and the last is numerical ocean model output. The different CIL estimates have very good correspondence to one another, and a composite CIL time series is produced for further analysis. The central feature of the paper is a changepoint analysis of the CIL series was undertaken to isolate different convective regimes. This is then followed by correlation analysis to relate CIL changes to oxygen dynamics.

***4.1** From an visual examination of the data, I doubt that the 4 regimes found exist as distinctive equilibrium states (if that is how one defines a regime), but are rather part of a continuously varying process with cycles and trends like we see in all climate data. These CIL data do clearly show underlying long period cycles and a decreasing trend since 1980. As for the existence of regimes, looking at the graphs without the regimes (Fig 3), and with the regimes superimposed (Fig 5) – one's eye is drawn to the regimes in Fig 5, but these are subtle at best. A statistical change point analysis will always pick up regimes, and in my experience the AIC criteria used here tends to choose overly complex model (here, implying more changepoints and regimes). But the above is a subjective assessment. Quantitatively, the analysis undertaken is clearly spelled out, its assumptions addressed, its application done properly, with the result that 4 regimes are statistically identified.*

-> We will stick with the last sentence. We acknowledge the fact that the regime shift perception is one among others, and the discussion section has adapted in this regard (RL297). In particular, what appears most important to us is that the recent change in CIL dynamics, as depicted by the regime change approach, is not a slow trending one, but includes a step change towards a new phenology of Black Sea ventilation. This is why we insist on this regime shift description, and on highlighting its potential consequences. In order to underline the difference between the actual and former regimes, besides statistical considerations, we followed the advice of Rev3, and produced the more direct illustration proposed in ***3.12***.

***4.2** These regimes here are changes in the level (mean) of the CIL over time; there appears to be no changes in the higher order statistical moments. Note that there are many different ways to do change point analyses, and each need not be only based on the mean level, but could use other metrics (variance, autocorrelation, skewness) to break up the series.*

-> It appeared to us that considering only the first order moment would already appear as a complex approach (see other reviewer comments in that sense), yet it is sufficient to make our point. This is why we discarded the option to consider other higher order metrics.

4.3 *I feel there is value in putting forth this regime analysis and the results for discussion to the wider community, and hence support publication of the paper. I would, however, downplay the ambitious claims of the title. This is an analysis that brings out the recent decline in the oxygenation state, its relation the ventilation and the CIL, and suggests the possibility of CIL regimes. But these regimes are subtle at best and not easily separable from the general downward trend in CIL. The last regime might be different from those in the past (less cold), but further work would be needed to verify/validate this as a new regime.*
-> The main idea is to oppose non-linear regime dynamics to smooth linear and sine trends models. Yes, this is gladly open to criticism, and future observations will tell more about the relevance of this paradigm. Yet, it appears worthy of presentation to the interested audience.

4.5 *My main criticism is that I found it difficult to follow the methodology and how it was applied. The only reason that I was able to do so was since I have used most all these techniques before, and so could 'read between the lines' as to what was being done. The latter comment is important since it means that a**dequate and understandable descriptions of the statistical approaches used need to be included in the main body of the manuscript (not just buried the appendix details)**. Important points are glossed over (the composite time series, the atmospheric predictors model for CIL, and in particular the regime shift model). To compound this, basic statistics are discussed at length (like overfitting, AIC, and model selection, and fitting the curves of Fig 3), but in a way that is not clearly linked to the problem. Below I provide specific suggestions. Overall, however, it should be straightforward to insert the necessary methodological detail into the text, and streamline the appendices, so that the results would be understandable and reproducible by an educated reader. I view these as relatively minor changes (since everything is there, if you read carefully with an adequate background).*
-> See specific answers below.

SPECIFIC:
4.6 *Title: a bit over-reaching. There is no explicit link made to climate. But the paper clearly demonstrates the CIL decline and de-oxygenation since 1980 whether or not these are really new regimes.*
-> Title has been changed: see **1.1.**

4.7 *Describe the "Atmospheric Predictors" model in the text (line 126). It looks like a lagged regression model. Important to be specific. Why not a basic equation?*
-> The lagged regression model equation has been given in addition to the reference (RL122) . See similar comments in **1.13** and **3.6.**

4.5 *Describe the composite CIL time series (line 145) in more detail. It is a central quantity for the paper.*

- The construction of the time series was kept very simple. It simply consists of a weighted average of the 4 time-series, restricted to available sources for years during which all sources weren't available. In order to emphasize the value of direct observations, we used relative weights of 1 for observation and 0.5 for model time-series. We've ensured any ambiguity is alleviated in the text (cf RL165)

- Describing the time series itself is the object of Sect. 3.1.
- The composite time series is depicted in Fig5 (stated in caption but now added to the color legend).

4.6 *Describe the regime shift model in text. It is a change-point model. These are easy to explain, if tricky to code. Be clear in the text the R-package you used, what it is based on, and that there are lots of different kinds of changepoint analysis and algorithms. Because of this lack of description, it is unclear why the authors then talk about model selection in the next paragraph (because different numbers of changepoints imply distinct statistical models).*

-> We answer here several comments related to this issue.

- We enforced the description of the regime shift analysis and change-point model in the main text (Sect2.2). This paragraph aims for accessibility and to allow non-specialized readers to get the basic notions of change-point models.
- The appendix have been streamlined, and are now organized as follows :
    - Appendix A is maintained as in the previous version : Comparison of C time series from multiple sources and definitions of metrics used, including Fig A1, that provides important information (see for instance request **1.15**).
    - Appendix B is now restricted to technical details for the change point model and regime shift analysis and validity of underlying assumptions. It has been compiled from the actual appendixes (“ Regime shift analysis”, “Statistics”, “Normality”,”Autocorrelation” and ”Biases between components of the composite time series”).
    - Appendix D has been removed, and the discussion on oxygen absolute or saturation concentration has been simplified and embedded in the main text.

4.7 *I think this paragraph on model selection and fit/complexity metrics to be too general and elementary – best put in the appendix.*

-> We would like to maintain the discussion about the regime shift approach, and it’s relevance to describe the C time series as compared to other paradigms (linear,etc..). Mostly because this discussion highlights different conceptions of the response of CIL ventilation to atmospheric trends, and potential future evolution of this ventilation (see L244-248). We acknowledge that the statistical arguments in favor of the regime shift paradigm, although objective, are subtle at best. Our aim is mostly to propose this paradigm as a plausible point of view, which is now stressed with the sentence “*Although, we acknowledge that the statistical advantage (AIC) of the regime shift description is subtle, we consider that it deserves further consideration as this difference in interpretation is fundamental in what regards the expected consequences on the Black Sea oxygenation status and in particular the threat on Black Sea marine populations, whose ecological adaptation (and rate of exploitation) have been built upon a ventilation regime and consequent biogeochemical balance, that may no longer prevail.*”.(RL297).

Because we want to maintain this discussion, it appears essential to keep a few lines on model selection in the Methods section. In our opinion, the fact that it is too elementary (and we are unsure that it will appear so to all readers) does not justify relegate it to appendix. We thus maintained a streamlined version of this paragraph in the Method section 2.2.

4.8 *Regime shift analysis and number of changepoints. I am unclear on whether the Rpackage you used here computes the number of breakpoints as well as their time locations. The reason I bring this up is since you*

have used an AIC criterion to choose the number of breakpoints – is this your addition to the analysis, or is it part of the R-package? State clearly as this is the central step that determines the 4 regimes (and hence underpins your conclusions).
-> The R package strucchange fits change-point models by computing their time location and section-specific averages (homoscedastic approach) for a selected number of breakpoints (1 to 5 in our case). It provides RSS and BIC for each of those change points models. Here, we decided to use AIC for selection, following advice from colleagues, although both BIC and AIC provide the same ranking between the different change point models (ie. different by the number of change points) and between the optimal change point model and the linear, periodic and linear+periodic models. Of course, AIC computations for change point models accounts for the fact that mean for each period AND time locations of the change points are evaluated. This has been made more explicit in RL203.
There is a correction to mention here, although it does not impact much on the results. Upon data manipulation (successive re-computation upon resubmission of this work) the time series used for AIC computation of the change point model was limited to 2017, while it is now extended to include 2019.
The complete time-series was correctly used for the design of the model and to get the means associated with segments, however, the AIC were derived in another version of the same scripts where the time series was erroneously limited to 2017 (and therefore shorter than the series used for the other models obviously leading to smaller AIC). So the only consequence is that AIC for the 4-segments model is corrected to 752, instead 730. 752 remains smaller than other models, so that no major changes in the conclusions arise, besides that the subtlety of this statistical support of the regime shift paradigm further needs to be stressed (cf. **4.1**).

4.9 *Figure 3 and supporting text is not needed. Fitting a linear trend and a sinusoid to the CIL doesn't add anything to the paper (and the data is repeated in Fig 5 for the regime analysis).*
->We removed Fig 3, but maintained the discussion part, following arguments given in **4.7** and following the request **1.26**).

4.10 F*igure 4. Similarly, all the model selection stuff like AIC versus changepoints is over-explained and too generic. It is enough to say in the text that AIC identified 3 breakpoints as optimal, then refer to appendix.*
-> Along successive submissions of this work, these parts have been displaced between supplementary materials, appendix and main body, several times, way and back. Here again, reviewer 1 asks instead to add a model to Fig. 3 (cf. **1.26**), while Rev4 proposes to minimize this section and send it back to appendixes. We can only conclude that this is a matter of personal appreciation. What matters to us here is to stress the fundamental difference between slow-trending/periodic variability, as opposed to the “abrupt” change in ventilation regimes which is a consequence of the non-linear dynamics of CIL formation. To us, the regime shift analysis better corresponds to this type of dynamics. Of course, as stressed above, this will always remain a matter of personal appreciation, even if statistical considerations objectivate the approach (see **4.1**). For this reason, and because linear trends and periodic descriptive models have been applied to describe Black Sea time series, we'd like to maintain this section in the main body. Yet, in agreement with the reviewer, the section has been streamlined and the importance of model selection minimized. It only serves the purpose to oppose two discussion points of view : the slow-trending or periodic routine to the new unprecedented regime, as will be stated more clearly. **We thus agree that the Fig4 gave unnecessary details, and removed it.**
AIC of the models (optimal 3 change point models, linear, periodic, linear + periodic) are now given in the text RLXX

4.11 *Figure 6. Define dC/dt (difference between median oxygen concentrations between successive years). Is this the annual oxygenation index you refer to in line 239?*

- Caption has been completed : "Weekly averaged basin wide CIL cold content formation and destruction rates (dC/dt), obtained as differences between the weekly integrated CIL cold content provided by the 3D model.". So dc/dt on Fig 4 (former 6) is a **Weekly CIL** formation/destruction index.
- The **Annual oxygenation** index is defined on Lines RL283 (precising the "Annual" there and for the CIL formation mentioned below).
- On Fig 6, we assess the correlation between this Annual Oxygenation index, and a corresponding **Annual CIL formation** index, obtained as the differences between annual values of the CIL composite time series.

4.12 *Figure 8. No need for both significance (colours) and p-values (size) since they give the same information. Significance values are thresholds for p-values.*

-> We totally agree this is redundant information. We saw no harm in this, but agree now that this may be confusing. This has been redrawn with equal size for each point, removing the point size legend, and thus considering only the significance color scale.

4.13 *Rationalize the material in appendices. It is rather a strange grab bag of material, and perhaps is a consequence of a previous review process?*

-> See answer in ***4.6***

4.14 *Appendix A: Put a basic understandable description of the regime analysis the main body of text (as noted). Details here. Note also that there are many changepoint analysis of various levels of statistical sophistication. You've picked one of many.*

-> See answer in **4.6** (proposition of appendix structure) and **4.2** (selected of changepoint analysis)

4.15 *Appendix B. I think this is a discussion of how the statistical assumptions of this regime analysis – which is based on linear regression - could be violated, and their possible consequences. Not sure you need most of this, and the important parts should be folded into Appendix A. Overall, I'll agree that within each regime, using average annual values, the normal iid assumption of OLS regression is OK (and hence the inference underlying the changepoint detection).*

-> We agree with the proposition and have integrated only the most important aspects in the dedicated Appendix (see **4.6**).

4.16 *Appendix D. This appendix could be omitted and a brief statement in the text made as to how and why oxygen concentration vs saturation were used.*

-> We have removed Appendix D and inserted this important issue in the methods. Please refer to **1.7** for a full answer on the question of the selected oxygen variable.

~~Climate change induced a~~ A new intermittent regime of convective ventilation ~~that~~ threatens the Black Sea oxygenation status

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Abstract. The Black Sea is entirely anoxic, except for a thin (∼100 m) ventilated surface layer. Since 1955, the oxygen content of this upper layer has decreased by 44 %. The reasons hypothesized for this decrease are, first, a period of eutrophication from ~~mid 70's to early 90's~~ the mid-1970s to the early 1990s and, second, a reduction in the ventilation processes, suspected for the recent years ~~. Here~~ (post-2005). Here, we show that the Black Sea convective ventilation regime has been drastically altered by atmospheric warming during the last decade. Since 2008, the prevailing regime is below the range of variability recorded since 1955, and is characterized by consecutive years during which the usual partial renewal of intermediate ~~waters~~ water does not occur. Oxygen records from the last decade ~~indicate a clear~~ are used to detail the relationship between cold water formation events and oxygenation ~~status at different pycnal levels, suggesting a leading~~ at different density levels, to highlight the role of convective ventilation in the oxygen budget of the ~~upper intermediate layers. We thus suggest that this regime shift has a significant impact~~ intermediate layers, and to emphasize the impact that a persistence of the reduced ventilation regime would bear on the oxygenation structure of the Black Sea and on its biogeochemical balance.

1 Introduction

By reducing water density and increasing vertical stratification, global warming is expected to impede ventilation mechanisms in the world ocean and regional seas with potential consequences for the oxygenation of the subsurface layer (Bopp et al., 2002; Keeling et al., 2010; Breitburg et al., 2018). On a global scale, the reduction of ventilation processes constitutes a larger contribution to marine deoxygenation than the warming-induced reduction of oxygen solubility (Bopp et al., 2013). ~~If~~ While the reduction in ventilation mechanisms is often evidenced, it remains challenging to determine whether such changes are the signal of natural variability or rather witness a significant regime change attributed to global warming (Long et al., 2016).

The Black Sea provides a miniature global ocean framework where processes of global interest occur at a scale more amendable for investigation. Its deep basin is permanently stratified and the ventilation of the subsurface layer relies in substantial parts on the convective transport of cold, oxygen-rich water formed each winter ~~in~~ at the surface. Between 1955 and 2015, the

Black Sea oxygen inventory has declined by 40% (Capet et al., 2016), which echoes the significant deoxygenation trend that affected the world ocean over a similar period (Schmidtko et al., 2017).

The permanent stratification of the Black Sea results from two external inflows (Öszoy and Ünlüata, 1997). The saline Mediterranean inflow enters the Black Sea by the lower part of the Bosporus straitStrait, the sole and narrow opening of the Black Sea towards the global ocean. The terrestrial fresh water inflow, for its greatest part, enters the Black Sea on its northwestern shelf. The contrast in density (salinity) between these two inflows maintains a permanent stratification in the open basin (halocline) , that prevents the that prevents ventilation of the deep layers. This lack of ventilation induces the permanent anoxic conditions that characterize 90% of the Black Sea waters. Between the oxic and anoxic (euxinic) layers, a suboxic zone, where both dissolved oxygen and hydrogen sulphide are below reliable detection limits (?), is maintained by biogeochemical processes(Stanev et al., 2018).

Just above the main halocline, the ventilation of the Black Sea subsurface waters (~ 50–100m), is ensured by convective circulation. It proceeds from the sinking of surface waters, made colder and denser by loss of heat towards the atmosphere in wintertime winter time (Ivanov et al., 2000). A similar ventilation process is observed, for instance, in the Mediterranean Gulf of Lion (e.g., MEDOC group et al., 1970; Coppola et al., 2017; Testor et al., 2017). In the Black Sea, however, the dense oxygenated waters never reach the deepest parts, as their sinking is restrained at intermediate depth by the permanent halocline. The accumulation of cold waters at intermediate depth forms the so-called Cold Intermediate Layer (CIL). The process of CIL formation thus provides an annual ventilating mechanism that structures the vertical distribution of oxygen (Gregg and Yakushev, 2005; Capet et al., 2016) (Konovalov and Murray, 2001; Gregg and Yakushev, 2005; Capet et al., 2016) and, by extension, that of nutrients (Pakhomova et al., 2014) (Konovalov and Murray, 2001; Pakhomova et al., 2014) and living components of the ecosystem (Sakınan and Gücü, 2017).

The semi-enclosed character of the Black Sea, superimposed with the fact that ventilation is limited to the first upper $\sim$100 m, makes it highly sensitive to variations in external forcingsforcing. In particular, the variations of atmospheric conditions (ege.g. air temperature, wind curl) result in pronounced and relatively fast inter-annual alterations of the Black Sea physical structure (Oguz et al., 2006; Capet et al., 2012; Kubryakov et al., 2016).

While several studies evidenced a warming trend in the Black Sea surface temperature (Belkin, 2009; von Schuckmann et al., 2018), Miladinova et al. (2017) showed that the Black Sea intermediate waters present an even stronger warming trend. This difference between the surface and subsurface temperature trends can be explained by the fact that the CIL dynamics buffers the atmospheric warming trends and minimize its signature in sea surface temperature (Nardelli et al., 2010).

The inter-annual variability in CIL formation can be explained for its larger part on the basis of winter air temperature anomalies (Oguz and Besiktepe, 1999; Capet et al., 2014), although intensity of the basin-wide cyclonic circulation (Staneva and Stanev, 1997; Capet et al., 2012; Korotaev et al., 2014), fresh water budget (Belokopytov, 2011) and the intensity of short-term meso-scale intrusions also play a role (Gregg and Yakushev, 2005; Ostrovskii and Zatsepin, 2016; Akpinar et al., 2017). An extensive description of the CIL dynamics, detailing the contributions and variability of the mechanisms mentioned above was recently provided by (Miladinova et al., 2018)Miladinova et al. (2018). One aspect is particularly relevant to our study: in winter time, the deepening of the mixed layer and the uplifting of isopycnals in the basin center (as the cyclonic circulation intensifies), expose subsurface waters to atmospheric cooling. If a well-formed CIL was present during the previous year, subsurface waters exposed to atmospheric cooling are already cold, which increases the amount of newly formed CIL waters (Stanev et al., 2003). Due to this positive feed-back feedback and to the accumulation of CIL waters formed during successive years, the inter-annual CIL dynamics is better described when winter air temperature anomalies are accumulated over 3 to 4 years, rather than considered on a year-to-year basis (Capet et al., 2014), which is in agreement with the 5 years upper estimate provided by **?** for the residence time within the CIL layer.

Given this non-linear context, there are reasons to suspect that global warming, by increasing the average air temperature around which annual fluctuations occur, may induce a persistent shift in the regime of the Black Sea subsurface ventilation. Indeed, Stanev et al. (2019) used Argo float data (2005–2018) to highlight a recent constriction of the CIL layer, following a trends leading to the trend leading to conditions where the CIL, as a layer colder than the underlying waters, would no longer exist, The authors further indicate implications on the Black Sea thermohaline properties, as this recent weakening of the CIL layer goes along with an increasing trends in surface and subsurface salinity, indicative of diapycnal mixing at the basis of the former CIL layer.

Here, we combine different data sources to analyze the variability in the Black Sea intermediate layer ventilation over the last 65 years and, in particular, investigate the existence of a statistically significant shift in the CIL formation regime, in regards to the variability observed over this extended period. The hypothesis of a significant regime shift is tested against the more traditional linear and periodic interpretation of the observed trends (e.g. Belokopytov, 2011), as the consequence for Black Sea ventilation and the future of the Black Sea oxygenation status in particular, are drastically different.

Indeed, Konovalov and Murray (2001) evidenced a clear relationship between oxygen conditions in the lower part of the CIL layer, and the temperature in that layer which is directly related to interannual variations in the CIL formation intensity. This relationship explain a large part of the inter-annual fluctuations in oxygen concentration in that layer, which occur at a time scale of a few years, and are superimposed on the larger scale change in oxygenation condition that is attributed to an increase in the primary production induced by the eutrophication phase of the late 1970s.

Our analysis thus aims to isolate pursue these investigations, and in particular to focus on the annual convective ventilation as a particular an individual component of the complex Black Sea deoxygenation dynamics (Konovalov and Murray, 2001; Capet et al., 2016) typically involves intricate biogeochemical and physical processes(Konovalov and Murray, 2001), in the context of the recent warming trend affecting the Black Sea (Miladinova et al., 2018).

Section 2 details the datasets considered to characterize the Black Sea CIL and oxygenation dynamics and the method of regime shifts shift analysis. In section 3, we analyse the long-term dynamics of the CIL through the lense CIL dynamics through the lens of regime shift analysis. In section 4, we use the most detailled datasets outputs from a three-dimensional hydrodynamic model and recent Argo records to relate CIL formation rates to changes in the Black Sea oxygenation conditions. Section 5, then concludes with a short discussion on In section 5, we discuss those results in the frame of larger time scales, while we conclude in Sect. 6.

2 Material and methods

2.1 The ~~CIL~~ cold intermediate layer cold content

While annual CIL formation rates are difficult to assess directly from observations, the status of the CIL can be quantified locally on the basis of vertical profiles of temperature and salinity. This simple indicator, based on routinely monitored variables, ~~provide~~ provides a suitable metric to combine various sources of data while summarizing an essential aspect of the thermo-haline conditions. The CIL cold content C is defined as the heat deficit within the CIL, integrated along the vertical:

$$C = -c_p \int_{CIL} \rho(z)[T(z) - T_{CIL}]\, dz, \tag{1}$$

~~with~~ where z is depth, ρ, the ~~in-situ~~ *in situ* density, c_p, the heat capacity of sea water and $T_{CIL} = 8.35$ °C, the temperature threshold which together with a density criterion $\rho > 1014.5\ \mathrm{kg\,m^{-3}}$, defines the CIL layer over which the integration is performed (Stanev et al., 2003, 2014; Capet et al., 2014). Although the use of a given temperature threshold to define the ~~occurence~~ occurrence of convective mixing is subject to discussion, the existence of a fixed ~~threshold to define~~ temperature threshold to characterize the CIL as a distinct ~~watermass, and as a signature of its formation process~~water mass, and in particular to identify its lower boundary, is evident given the fixed value of ~ 9°C that characterize the underlying deep waters (Stanev et al., 2019). The above definition has been chosen for ~~correspondence with past litterature~~consistency with previous literature.

C is expressed in units of $\mathrm{J\,m^{-2}}$ and provides a vertically integrated diagnostic which is more informative than, for instance, the temperature at a fixed depth or the depth of a given iso-thermal surface. Although C is a deficit, we inverted the sign of C in comparison with previous literature (Stanev et al., 2003; Piotukh et al., 2011; Capet et al., 2014) for the convenience of working with a positive quantity. Large C values thus correspond to large heat deficit in the CIL, ~~ie~~i.e. to low temperature in a well-formed CIL layer, which is characteristic of cold years. A decrease in C corresponds to a weakening of cold water formation (typically for warm years), an increase in the intermediate water temperature and/or a decrease in the vertical extent of the CIL.

C has been estimated for each year over the ~~1955-2019~~ 1955–2019 period using four data sources summarized in Table 1. These sources include ~~in-situ~~ *in situ* historical (ship-casts) and modern (Argo) observations, as well as empirical and mechanistic modelling (Fig. 1). Annual and spatial average values for the deep sea (depth > 50 m) were derived from each ~~data set~~dataset, while considering the ~~artifacts~~ errors induced by uneven sampling in the context of pronounced seasonal and spatial variability. Each data source has particular assets and drawbacks, and involves specific data processing to reach estimates of annual and spatial C averages as described below. All processed annual time-series are made available in ~~netcdf~~ netCDF format on a public repository (see 'Data availability').

~~***Argo profilers***: The assets of autonomous Argo profilers are a high temporal resolution and the continuous coverage of recent years, which offers unprecedented means to explore the CIL dynamics at fine spatial and temporal scales (Akpinar et al., 2017; Stanev et al., The drawbacks are the mingled spatial and temporal variability inherent to Argo data, the incomplete spatial coverage, and the lack of data prior to 2005. These data were collected and made freely available by the Coriolis project and programmes that contribute to it (http://www.coriolis.eu.org). The request criteria used were Bounding box : 40-47N;27-42E; Period~~

Table 1. Overview of the four ~~data sets~~ datasets used to characterize the CIL inter annual variability. Details are provided for each ~~data source~~ dataset in Sect. 2.1.

~~Data Set~~ Dataset (Period)	Rationale	Assets (+) & Drawbacks (-)	References
Ship casts (1956–2011)	~~In-situ~~ *In situ* profiles analyzed with the DIVA detrending methodology to disentangle spatial and temporal variability.	+ Large time cover + Direct observation - Uneven spatial and seasonal sampling - Annual gaps	Boyer et al. (2009) Capet et al. (2014)
Atmospheric Predictors (1956–2012)	Empirical combination of atmospheric descriptors (winter air temperature anomalies) calibrated to reproduce the above time-series.	+ Full time cover - Not observation - Validity of statistical model not ~~granted~~ guaranteed outside its range of calibration.	Dee et al. (2011) Capet et al. (2014)
GHER3D (1981–2017)	~~3D~~ Three-dimensional hydrodynamic model (GHER). Unconstrained simulation (no data assimilation). 5km resolution. ERA-interim atmospheric ~~forcings~~forcing.	+ Synopticity + Underlying mechanistic understanding - Not observation	Stanev and Beckers (1999) Vandenbulcke et al. (2010) Capet et al. (2012)
Argo (2005–2019)	Drifting autonomous profilers. Average of synchronous profiles.	+ Direct observation + Intra-annual resolution - Uneven spatial sampling - Recent years only	Stanev et al. (2013) Akpinar et al. (2017) Stanev et al. (2019)

(DD/MM/YYYY) : between '01/01/2005' and '31/12/2019'; Data type(s) : ('Argo profiles','Argo trajectory'); Required Physical parameters : ('Sea temperature','Practical salinity'),Quality : Good.C values were derived from individual Argo profiles (Fig. 1). All available profiles in a given year were averaged to produce the annual Argo time series C^{Argo}. Although homogeneous seasonal sampling can be assumed, we note that the uneven spatial coverage of Argo profiles might induce a bias in the inferred trends. This potential bias stems from the horizontal gradient in C, that is structured radially from the central (lower C) to the peripheral (higher C) regions of the Black Sea (Stanev et al., 2003; Capet et al., 2014). Argo samplings are more abundant in the peripheral regions, which suggests that C^{Argo} might be slightly biased towards high values.

In-situ ship-cast profiles***In situ ship-cast profiles***: The asset of ship-based profiles is their extended temporal coverage. The drawbacks are the difficulty to untangle spatial and temporal variability (as for any non-synoptic data source), the uneven sampling effort and the low data availability posterior to 2000. The C^{Ships} time series was provided by the application of the DIVA detrending methodology on ship-cast profiles extracted from the World Ocean Database (Boyer et al., 2009) in the box 40°–47°30' N, 27°–42°E for the period 1955–2011. DIVA is a sophisticated data interpolation software (Troupin et al., 2012) based on a variational approach. The detrending methodology (Capet et al., 2014) provided inter-annual trends, here representative for the central basin, cleared from the artifacts errors induced by the combination of uneven sampling and pronounced variability along the seasonal and spatial dimensions. We redirect the reader to the reference study Capet et al. (2014) for further details on data sources, data distribution and methodology.

Atmospheric predictors: The statistical model considered here consists in a linear combination of time-lagged of a lagged regression model based on winter air temperature anomalies., i.e. using the form $C_i^{Atmos} = a_0 + a_1 \cdot ATW_i + a_2 \cdot ATW_{i-1} + a_3 \cdot ATW_{i-}$ where i is a year index, and ATW_i stands for the anomaly of the preceding winter air temperature (December-March).

This model was obtained by a stepwise selection amongst potential descriptor variables (incl. summer and winter air temperature, winds and fresh water discharge), in order to reproduce the inter-annual variability of C^{Ships} (Capet et al., 2014) and proposed as an alternative to the winter severity index defined earlier (Simonov and Altman, 1991)by Simonov and Altman (1991). C^{Atmos} is thus naturally representative of the same quantity, i.e. annually and spatially averaged C. The asset of this approach is the opportunity to fill the gaps between observations in past years, using atmospheric reanalysis of 2m air temperature provided by the European Centre for Medium-Range Weather Forecasts (ECMWF) for the period 1980–2013. Its drawbacks lie in its empirical nature and indirect relationship to observable sea conditions. C^{Atmos} was only extracted for the years covered in the reference studyCapet et al. (2014), considering that the potential non-linearity in the air temperature-C relationship may be exacerbated for the low C values typical of recent years.

3D hydrodynamic model***Three-dimensional (3D) hydrodynamic model***: The Black Sea implementation of the 3D hydrodynamic model GHER has been used in several studies (?Stanev and Beckers, 1999; Vandenbulcke et al., 2010; Capet et al., 2012). The present simulation uses the set-up presented in Capet et al. (2012) and (?Stanev and Beckers, 1999; Vandenbulcke et al., 2010). In particular, Capet et al. (2012) presents the model setup used in this study and analyze the simulated CIL dynamics. This simulation, extending over the period 1981–2017, has been produced without any form of data assimilation, on the basis of the ERA-Interim set of atmospheric forcing provided by the ECMWF data center (Dee et al., 2011). Aggregated weekly outputs of the GHER3D model are made available on a public repository (see 'Data availability'). $C^{3dModel}$ $C^{Model3D}$ was derived from synoptic weekly model outputs and averaged for each year and spatially over the deep basin (depth > 50m). The assets of this approach are the synoptic coverage in time and space and the mechanistic nature of the model, that implies a reproducible understanding of the process of CIL formation. The drawback lies in the numerical and conceptual error that might affect unconstrained model outputs.

Argo profilers: The assets of autonomous Argo profilers are a high temporal resolution and the continuous coverage of recent years, which offers unprecedented means to explore the CIL dynamics at fine spatial and temporal scales (Akpinar et al., 2017; Stanev et al., The drawbacks are the mingled spatial and temporal variability inherent to Argo data, the incomplete spatial coverage, and the lack of data prior to 2005. This dataset was collected and made freely available by the Coriolis project and programmes that contribute to it (http://www.coriolis.eu.org). The request criteria used were [Bounding box : 40-47N;27-42E; Period (DD/MM/YYYY) : between '01/01/2005' and '31/12/2019'; Data type(s) : ('Argo profiles','Argo trajectory'); Required Physical parameters : ('Sea temperature','Practical salinity'),Quality : Good]. In average, this set includes about 9 floats per year, with a minimum of two floats for 2005 and more than 12 floats from 2013 to 2019. C values were derived from individual Argo profiles (Fig. 1). All available profiles in a given year were averaged to produce the annual Argo time series C^{Argo}. Although homogeneous seasonal sampling can be assumed, we note that the uneven spatial coverage of Argo profiles might induce a bias in the inferred trends. This potential bias stems from the horizontal gradient in C, that is structured radially from the central (lower C) to the peripheral (higher C) regions of the Black Sea (Stanev et al., 2003; Capet et al., 2014). As Argo samplings are generally more abundant in the peripheral regions, i.e. outside of the divergent cyclonic gyres, this suggests that C^{Argo} might be slightly biased towards high values.

Table 2 give specific comments on the dependence relationship between the different time series presented above. Only C_i^{Atmos} and C_i^{Ships} can be considered as strictly dependant. $C_i^{Model3D}$ is influenced by the same datasets that were used to build C_i^{Atmos} and C_i^{Ships}, but through drastically different processing pathways and can thus be practically considered as independent.

A composite time series was computed as the average of available source-specific series for each year . Given their indirect nature, the two model time series were given a smaller weight when computing this average (0.5 for the model timeseries, 1 for the others) . A composite time series was constructed as the weighted average of the 4 time-series, restricted to available sources for years during which all sources were not available:

$$C_i = \frac{\sum_j w_i^j \cdot C_i^j}{\sum_j w_i^j}, \tag{2}$$

where i is an annual index, j stands for a source index ($j \in \{Model3D, Atmos, Argo, Ships\}$). In order to emphasize the value of direct observations, the weights w_i^{Argo} (resp. w_i^{Ships}) equals to 1 if C_i^{Argo} (resp. w_i^{Ships}) is defined (i.e. the time series covers the year i) , and 0 otherwise, while $w_i^{Model3D}$ (resp. w_i^{Atmos}) equals to 0.5 if $C_i^{Model3D}$ (resp. w_i^{Atmos}) is defined, and 0 otherwise.

Table 2. Dependence relationships between the different time series.

	Ship Casts	Model3D	Argo
Atmos	The statistical model providing C_i^{Atmos} is built on the basis of C_i^{Ships}. So, even if C_i^{Atmos} is more homogeneous and complete than C_i^{Ships}, it can not be considered as independent.	Atmospheric conditions used to build C_i^{Atmos} are issued from the same data sets (ECMWF) that were used to force the 3D model. So, formally, both approaches are influenced by a common data-set, but through drastically different processing pathways. We consider no direct dependency in this case.	Strictly independent.
Ship Casts	-	The 3D model simulations involve no data assimilation. The model has been calibrated by testing different parameterizations of the atmospheric fluxes bulk formulations, using T and S $insitu$ data from the same set that has been used to build C_i^{Ships}. However, this calibration was not based on C_i^{Ships} itself. Also, the selected parameterization remains fixed for the whole simulation time. So, although both times series are influenced by a common data sets, we consider there is no direct dependency.	Strictly independent.
Model3D	-	-	Strictly independent.

The composite time series was then used as a synoptic metric ~~indicating on~~ for the inter-annual variability of the convective ventilation of the Black Sea intermediate layers.

The consistency of the different CIL cold content data sources is demonstrated by the high correlations obtained between the annual time series (from 0.91 to 0.98, see detailed comparative statistics in appendix A). Despite the small number of overlapping years between certain series (eg. seven years between C_i^{Ship} C_i^{Argo}, see Fig. A1), all correlations are significant ($p < 0.05$). The close correspondence between independent time series (see discussion above), issued respectively from strictly observational and purely mechanistic modeling approaches, provides a high confidence in their accuracy and ensures the robustness of the forthcoming analysis.

More precisely, the standard deviations estimated from the different series are similar (~~$\sim 100\,\mathrm{MJ\,m^{-2}}$~~$\sim 100\,\mathrm{MJ\,m^{-2}}$), despite their distinct temporal coverage. The root mean square errors that characterize the disagreement between the different

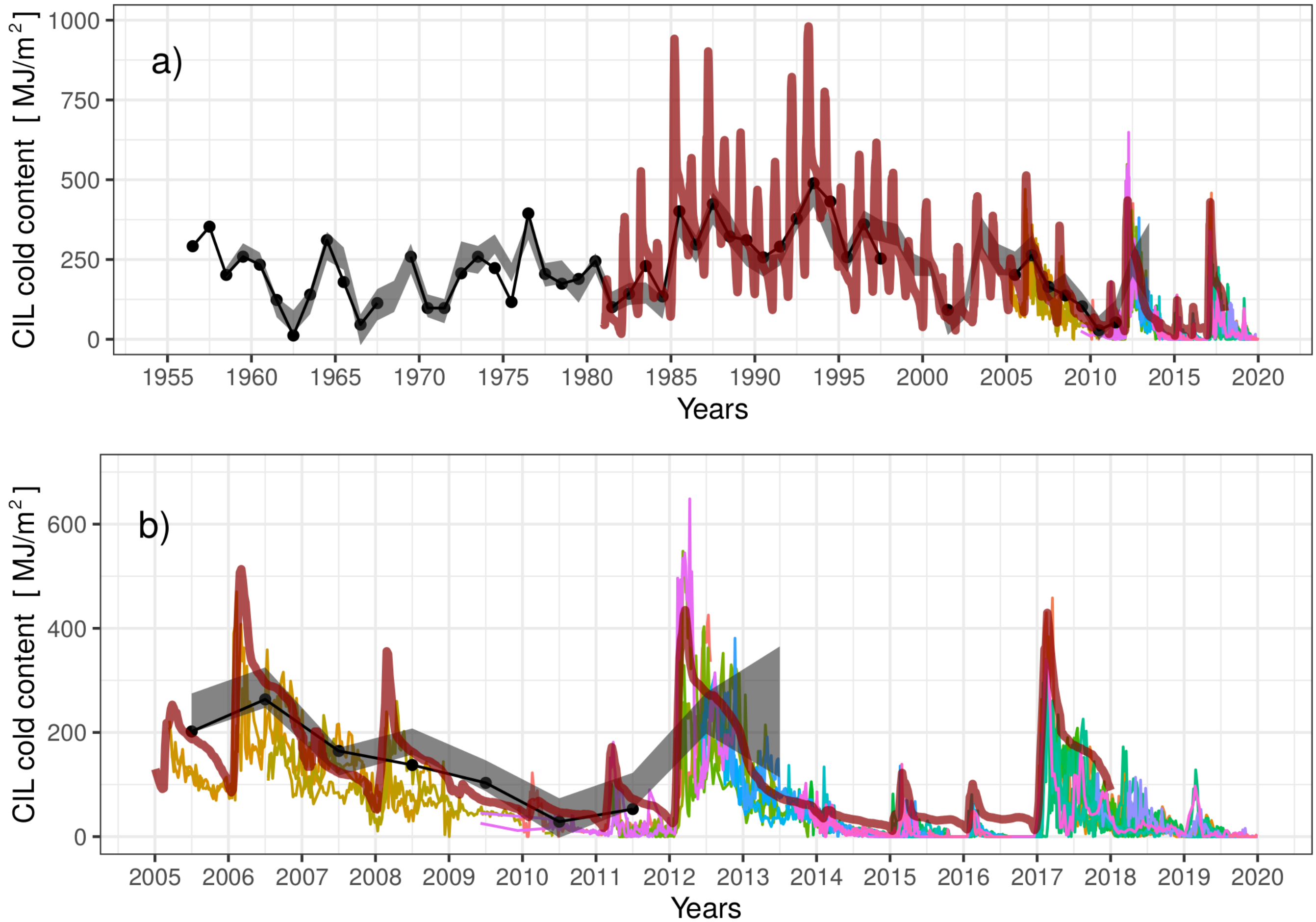

Figure 1. Time series of the Black Sea CIL cold content (C) originated from various data sources (Table 1), displayed at original temporal resolution: (black dots) inter-annual trend derived from ship casts; (gray shaded area) confidence bounds (p<0.01) of the statistical model based on winter air temperature anomalies; (dark red thick line) GHER3D model; (thin colored lines) Individual Argo floats. a) Complete period of analysis, b) focus on the recent years.

data sources remain below this temporal standard deviation (in all but one case, see appendix A for details). This justifies to merge the different sources in a unique composite time series, enabling a robust long term analysis of the variablity variability in the Black Sea CIL formation.

2.2 Regime shift analysis and descriptive model selection

The inter annual

The inter-annual variability of the Black Sea CIL formation is analyzed in the framework of regime shift analysis(Zeileis et al., 2003). The rationale behind this approach . The natural first step towards identification of a regime shift in a time series is the identification of change points (Andersen et al., 2009).

The rationale behind change point models is to identify periods over which the times a time series depicts statistically distinct regimes, ie. In its simplest form, a change point model will aim to identify distinct regimes that differs in terms of their mean, i.e. during which annual fluctuations take place around distinct averages. Statistical tools are used to judge the relevance of such segmented descriptionNote that other type of change point analyses can be done, which would consider other metrics (variance, autocorrelation, skewness) instead of the mean to break up the series. For the sake of simplicity, only the first moment (mean) is considered in this study.

The change point model used for this regime shift analysis has been derived and verified following the methodology described in the documentation of the R package `strucchange` (Zeileis et al., 2003). The procedure includes the following steps.

First, the presence of at least one significant change point in the time series was tested, against the null hypothesis that considers annual fluctuations around a fixed average value for the entire time series. More details are given in appendix B.

The problem when choosing among different models to describe a time series, To this aim, the `strucchange` package provides different methods based on the model's ability to reproduce observations, is that any additional parameter involved in a descriptive model automatically reduces generalized fluctuation test framework as well as from the F test (Chow test) framework.

Second, the locations of the most likely change points in the time series were identified. Assuming that N change points separates $N+1$ periods, this step thus consists in identifying the locations of the change points and the specific mean value specific to each period. This identification proceeds from an optimization procedure aiming to minimize the residual sum of squares (RSS) used to assess its descriptive skill. This is particularly clear in the context of regime shift model, where "considering additional parameters" means segmenting the entire period into more distinct periods with specific averages. To decide until which point the addition of a new change point between the time series and the change point model (ie. a new distinct period)in the regime shift modelremains informative, we considered constant mean value for each specific period).

Five change points models were derived for the composite time series, considering from one ($N=1$) to five ($N=5$) change points. The final step consists in selecting, among those five models, the one that 'best' describes the time series. Obviously, considering additional change points can only reduce the RSS. This is generally true for any descriptive model, and has led to the definition of the Akaike Information Criterion (AIC) , that assesses for model selection. Basically, the AIC consider the RSS of each model but gives includes a penalty for the introduction of additional parameters (Akaike, 1974). number of parameters (Akaike, 1974), such that if two models bear the same RSS, the one involving less parameters will be favored. Note that in our case, the parameters identified for change point models includes both the locations of change points and the specific mean for each period.

The model with the smallest AIC should be favored for interpretation. The same approach was In section 3.1, the AIC is also used to compare the regime shifts models to the shift models to linear and periodic models of C_i.

Time series of the Black Sea CIL cold content (C) originated from various data sources (Tab. 1), displayed at original temporal resolution: (black dots) inter-annual trend derived from ship casts; (gray shaded area) statistical model based on atmospheric predictors; (dark red thick line) 3D GHER model; (thin colored lines) Individual Argo floats. a) Complete period of analysis, b) focus on the recent years. More technical details and verification of underlying assumptions are given in appendix B.

2.3 Oxygen

BGC-Argo oxygen observations were obtained from the Coriolis data center for a period extending from 01/01/2010 to 01/01/2020. Only descending Argo profiles were considered, to minimize discrepancies associated with oxygen sensors response time (**?**). To minimize the impact of spatial variability, oxygen concentrations were saturation was considered using a potential density anomaly (σ) vertical scale and the year 2010 was discarded for lack of observations. While both oxygen concentrations concentration [μM] and oxygen saturation level [%] were considered in our first analyses, the narrow range of thermo-haline variability in the layers considered allow to present only results obtained for one of those aspects and concentration was retained (see Appendix **??**)of interest results in very small variations of the oxygen solubility. As a consequence, considering one or the other of these two variables led to very similar results, and we opted for oxygen saturation in the following.

Figure 2 indicates that the use of σ vertical coordinates indeed minimizes the range of spatial variability (see years 2010-20182014–2018, when more Argo are were operating) and gives sense to the use of monthly medians as an integrated indicator of the basin-wide oxygenation status at different layers. For deeper density layers (Fig. 2c), a larger interquartile range is induced by Argo 's floats profiling in the vicinity of Bosporus influenced area, as plumes of Bosporus ventilation introduce a larger horizontal variability in oxygen concentrationsaturation.

3 The Black Sea CIL cold intermediate layer dynamics over 1955–2019

3.1 Descriptive models

The composite time series C_i is depicted on Fig. **??** with calibrated linear and periodic models (with i as a year index)in Fig. 3, along with individual components.

The CIL cold content composite time series (light blue) is obtained as a weighted average of the different data sources. Calibrated linear and periodic models (red dotted lines and area) are considered for discussion.

The poor statistics associated with a linear model , $C_i = l_1.i + l_2$ (description of C_i, in the form $C_i \sim l_1 \cdot i + l_2$ (i stands for an annual index, adjusted $R^2 = 0.05$, AIC=794, with $l_1 = -1.59 \pm 0.78 \mathrm{MJ\,m^{-2}\,yr^{-1}}$), dismiss the perception of a linear trend extending over the entire period. Using the periodic model, $C_i = p_1 + p_2 \sin(\frac{2\pi}{p_3} i)$$C_i \sim p_1 + p_2 \sin(\frac{2\pi}{p_3} \cdot i)$, gives a better representation of the cold content inter-annual variability (AIC=763), and provides broad characteristics of C_i C_i : the mean value,

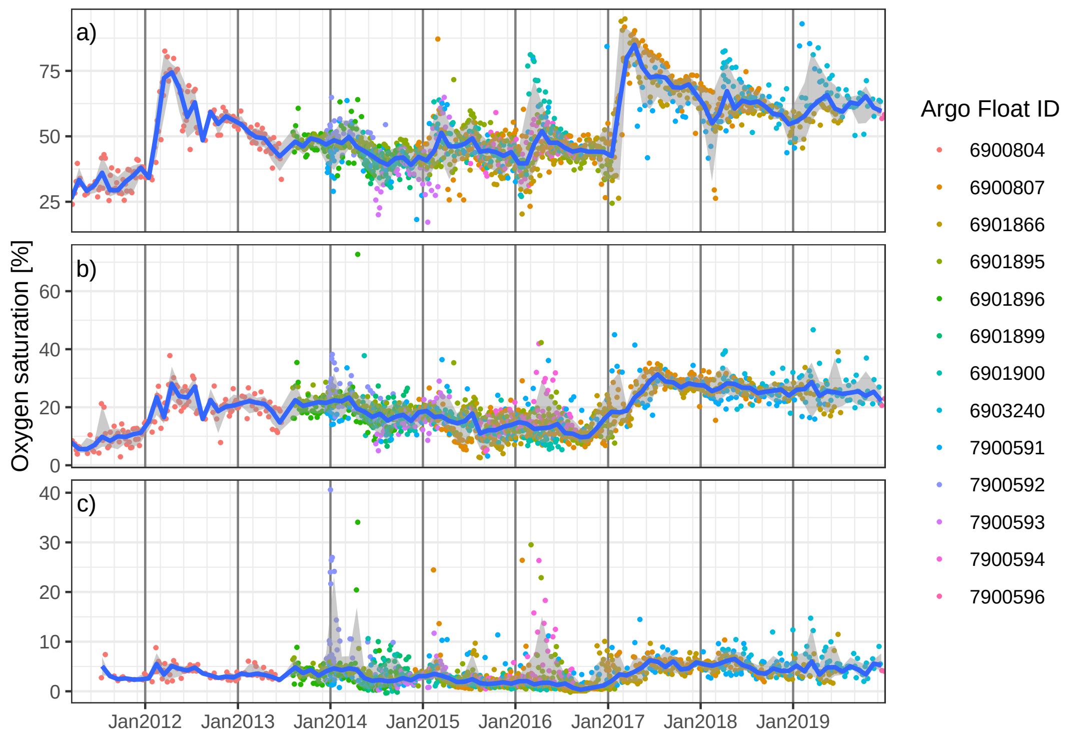

Figure 2. Oxygen concentrations saturation levels derived from individual BGC-Argo profiles at σ values of a) 14.5, b) 15.0 and c) 15.5 $\mathrm{kg\,m^{-3}}$. Colored points correspond to different Argo platformsfloats. The blue line represents monthly medians and while the shaded area covers monthly interquartile ranges.

$p_1 = 222 \pm 12\ \mathrm{MJm^{-2}}$$p_1 = 222 \pm 12\ \mathrm{MJ\,m^{-2}}$, the amplitude of inter-annual variability, $p_2 = 114 \pm 16\ \mathrm{MJm^{-2}}$$p_2 = 114 \pm 16\ \mathrm{MJ\,m^{-2}}$, and the periodicity of pseudo-oscillations, $p_3 = 43.04 \pm 0.02$ years.

However, both the A combination of linear and periodic models overestimates model, with the form $C_i \sim lp_1 + lp_2 \sin(\frac{2\pi}{lp_3} \cdot i + lp_4 \cdot i)$ slightly improves the descriptive statistics (AIC=758.5). However, all of the above descriptive models overestimate C in the recent years, as the composite time series C_i shows a departure from its usual range of variability during the last decade. This is evidenced by ranking the 65 years of C_i on the basis of their cold content. It is remarkable that, among the ten years with the least cold content, eight occurred after 2010.

Figure ?? gives the AIC obtained for regime shifts models considering from 1 to 5 change points (ie. partitioning the entire period into 2 to 6 segments). All of those regime shift descriptions appear Each of the change point models appears as statistically more informative, *sensu* AIC, than a linear or periodic interpretation of the time series(and than a combination of linear and periodic model, AIC=758, not shown). In particular the 4-segments model (iei.e. 3 change points, AIC=752) should be favored for interpretation.

The residual sum of squares (RSS, left scale) of regime shifts models decrease with each additional change points. The Akaike Information Content (AIC, right scale) penalizes additional parameters to support model selection. For comparison, the RSS and AIC obtained with the calibrated periodic model are depicted with dashed lines.

3.2 Regimes Regime shifts in the CIL cold intermediate layer cold content

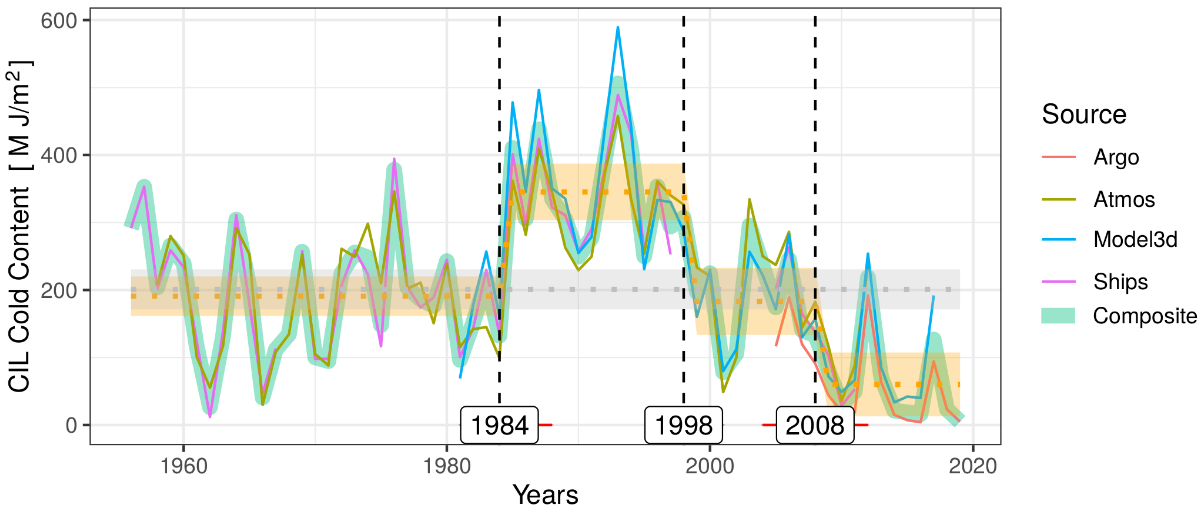

Figure 3. Multi-decadal variability of the Black Sea CIL cold content and distinct periods identified by the regime shift analysis. Confidence intervals on mean C values are indicated by blue the orange shaded area for each period, and by the gray shaded area for the null hypothesis (i.e. , considering no regime shifts). Confidence intervals on the time limits of each period are indicated with red ranges.

The evolution of C_i over 1955-2019 is thus best described by discriminating four periods (P_1–P_4, Fig. 3), objectively identified through regime shift analysis.

A "routinestandard" regime is identified that is consistent for periods P_1 (1955–1984) and P_3 (1998–2006), which depict averages $\langle C \rangle_{P_1} = 191 \pm 15\ \mathrm{MJ\,m^{-2}}$ and $\langle C \rangle_{P_3} = 183 \pm 29\ \mathrm{MJ\,m^{-2}}$ $\langle C \rangle_{P_1} = 191 \pm 15\ \mathrm{MJ\,m^{-2}}$ and $\langle C \rangle_{P_3} = 183 \pm 29\ \mathrm{MJ\,m^{-2}}$, respectively. This routine regime is also consistent with the average C obtained without considering any change points, $\langle C \rangle = 201 \pm 15\ \mathrm{MJ\,m^{-2}}$ $\langle C \rangle = 201 \pm 15\ \mathrm{MJ\,m^{-2}}$ (Fig. 3).

Departing from this routine, a cold period (P_2) is visible from 1984 to 1998, during which C fluctuates around a larger average value, $\langle C \rangle_{P_2} = 345 \pm 26\ \mathrm{MJ\,m^{-2}}$ $\langle C \rangle_{P_2} = 345 \pm 26\ \mathrm{MJ\,m^{-2}}$. This cold period has been described in numerous studies (e.g., Ivanov et al., 2000; Oguz et al., 2006)(e.g. Ivanov et al., 2000; Oguz et al., 2006), and is attributed to strong and persistent anomalies in atmospheric teleconnection patterns (East Asia/West Russia and North Atlantic oscillations, ; Kazmin and Zatsepin (2007); Capet et al. (2012)). Noteworthy, similar cold periods were identified earlier in the XXth 20th century (late 1920s–early 1930s and early 1950s, ; Ivanov et al. (2000)).

From 2008 to 2019, a warmer period (P_4) is identified during which C varies around a lower average $\langle C \rangle_{P_4} = 60 \pm 28\ \mathrm{MJ\,m^{-2}}$ $\langle C \rangle_{P_4} = ($

The regime shift analysis thus evidences that a general weakening of the cold water formation and associated ventilation prevails ~~since 10 years~~ in the Black Sea since about ten years. Warm years and ~~weak~~ low cold content were also observed during the years 1961 and 1963, but those were not identified as part of a statistically distinct "warm" regime and should be considered as strong fluctuations within P_1. The regime shift analysis thus indicates that the current restricted ventilation conditions have no precedent in modern history.

4 ~~CIL~~ Cold intermediate layer formation as ~~a basin-wide ventilation~~ an oxygenation process

The intra-annual resolution provided by the 3D model and Argo time series (Fig. 1) suggests that the partial CIL renewal, that was taking place systematically each year ~~in precedent regimes~~before 2008, has now become occasional. Here we focus on ~~the latest period~~ period P4, better detailed in our datasets, to characterize the CIL formation as a basin wide ventilation process and its relationship with changes in oxygen ~~concentration~~ saturation at different σ levels.

Basin wide CIL formation and destruction rates were computed from the synoptic 3D model outputs, as differences between weekly C values (Fig. 4). The seasonal sequence depicts CIL formation peaks from late December to March, typically reaching C formation rates of ~~10,~~ 5, 10 and 1 ~~$\mathrm{MJ\,m^{-2}\,d^{-1}}$ respectively~~ $\mathrm{MJ\,m^{-2}\,d^{-1}}$ for the period ~~P2, P1-3~~ P1/3, P2 and P4, respectively (Fig. 4a,b,c). The CIL cold content is then eroded at different rates before, during and at the end the thermocline ~~setting~~season, with a damped erosion rate through the thermocline season between 0 and ~~$\sim$~~ about -1 ~~$\mathrm{MJ\,m^{-2}\,d^{-1}}$~~$\mathrm{MJ\,m^{-2}\,d^{-1}}$. CIL formation processes have been described extensively in the past (e.g. Akpinar et al., 2017; Miladinova et al., 2018), in ~~better details~~ more detail than allowed by the integrated perspective adopted here. This integrated point of view, however, serves to point out the striking ~~quasi absence~~ quasi-absence of CIL formation ~~peak~~ peaks for the years 2001, 2007, 2009, 2010, 2013 and ~~2014.~~ 2014 (Fig. 4d). In fact, ~~among~~ during the period of Argo oxygen sampling, only 2012 and 2017 ~~depicts~~ depict important CIL formation events, while ~~smaller but still perceptible formation rates are observed~~ minor CIL formation events are shown for 2015 and 2016.

Oxygen ~~concentration~~ saturation in this period varies in concordance with CIL formation until σ layers of about ~~–~~16.0 $\mathrm{kg\,m^{-3}}$ (Fig. 5). Large ~~increase in oxygen concentration~~ increases can be observed from December to March ~~for the years for which~~ in the years 2012, 2015, 2016 and 2017 when CIL formation is significant, which denotes the impact of convective ventilation. ~~While the impact of 2012 and 2017 CIL formation can be seen~~ The narrow interquartile ranges depicted on Fig. 5, denote the efficiency of the isopycnal diffusion of oxygen : the amount of oxygen imported with the newly formed CIL waters is distributed horizontally and contributes to increase the average oxygen saturation of a given σ layer.

While major CIL formation events in 2012 and 2017 induced a significant increase in oxygen saturation through the whole oxygenated water column, the ~~smaller CIL formation of~~ minor events in 2015 and 2016 ~~only penetrates until $\sim$ 14.6 $\mathrm{kg\,m^{-3}}$~~seems to have a limited penetration depth. For instance, oxygen saturation at 14.6 $\mathrm{kg\,m^{-3}}$ only stagnates during 2015 and 2016 as compared to 2014, while oxygen saturation at 15.1 $\mathrm{kg\,m^{-3}}$ keeps decreasing during these two years, indicating that the amount of oxygen brought to this layer during minor ventilation events is not sufficient to counter-balance the biogeochemical oxygen consumption terms (i.e. respiration and oxidation of reduced substances diffusing upwards).

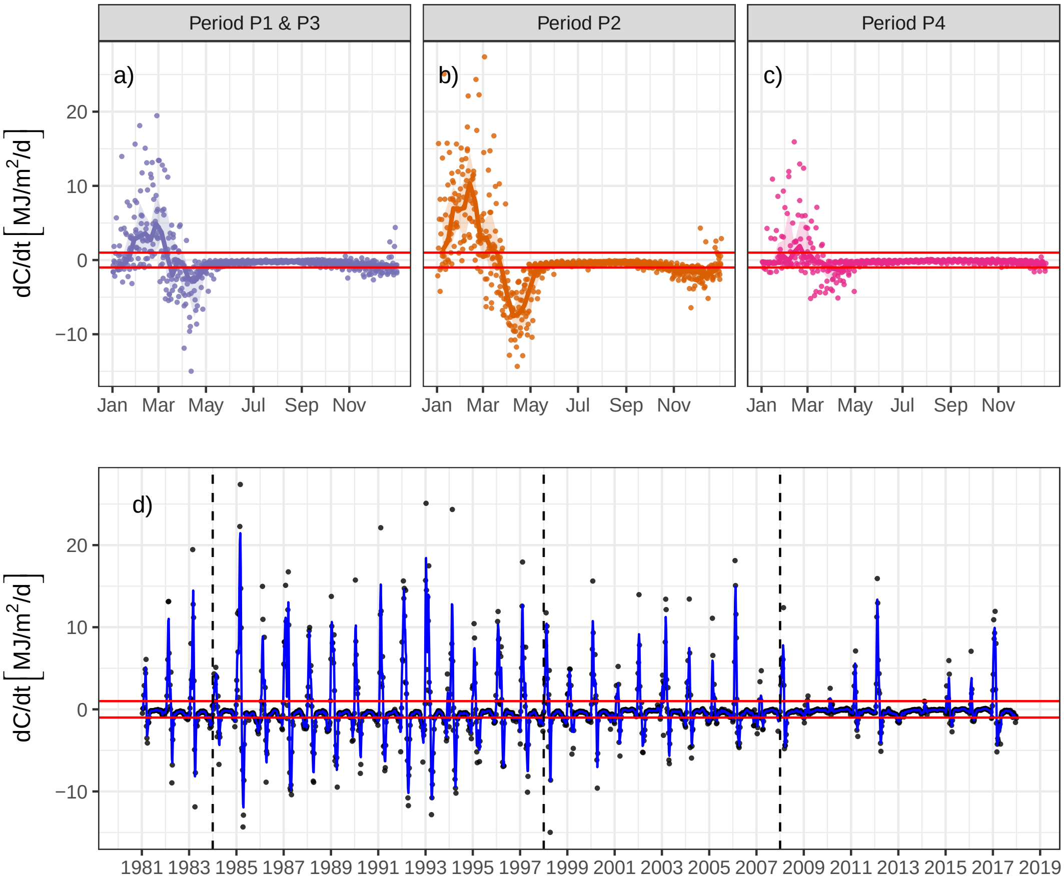

Figure 4. ~~Basin~~ Weekly averaged basin wide CIL cold content formation and destruction rates ~~inferred from the 3D model time-series~~(dC/dt), obtained as differences between the weekly ~~averaged values. Upper panels~~integrated CIL cold content provided by the GHER3D model: a), b), c) on a seasonal frame with weekly medians (line) and interquartile range (shaded area), merging years from the periods P1 and P3 (considered together), P2 and P4~~. Lower panel:~~ ; d) on an ~~interannual~~ inter-annual scale, with 3-weeks running average (blue line). Vertical dotted lines separates the four periods evidenced ~~on Fig. 3~~by the regime shift model. The red lines delineate the thresholds of ± 1 ~~$\mathrm{MJ\,m^{-2}\,d^{-1}}$~~$\mathrm{MJ\,m^{-2}\,d^{-1}}$, corresponding to the lower bound of CIL erosion rate during summers.

Following our attempt to summarize large datasets and to characterize a basin-scale annual oxygenation rate, we computed for each layer an annual oxygenation index as the difference between the median oxygen ~~concentration~~ saturation in November between successive years. The rationale behind this approach is that CIL formation typically extends from December to March (Fig. 4a,b,c).

In order to obtain a general indication on the (pycnal) ~~depth of penetration~~ penetration depth of the convective ventilation associated with CIL formation, we assessed the Pearson correlation coefficient between this annual oxygenation index, and a first order assessment of annual CIL formation, obtained as the annual difference in the C composite time series. The correlation between oxygenation and CIL formation is high near the surface and ~~decrease regularly~~ decreases continuously as

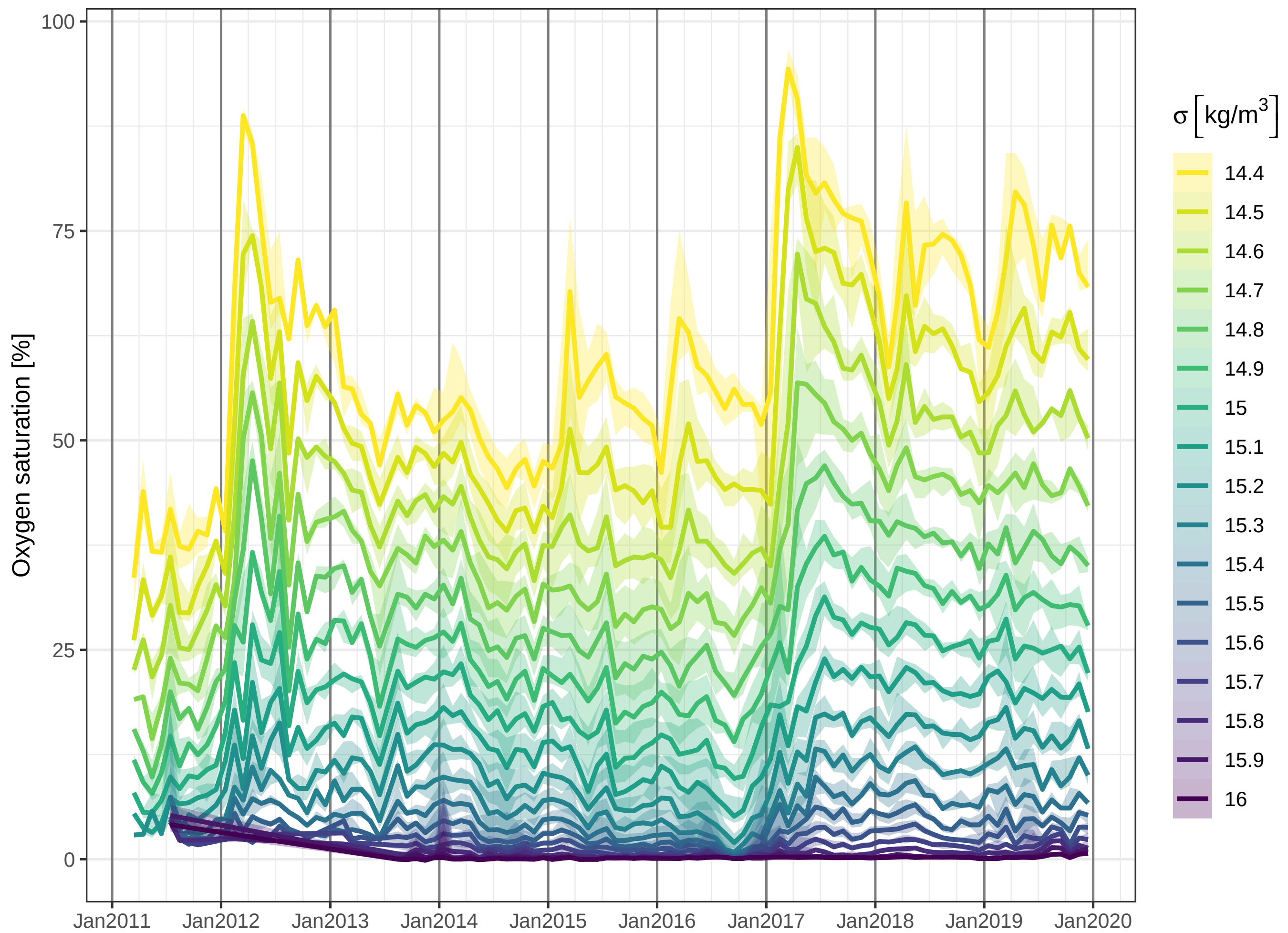

Figure 5. Monthly medians of oxygen ~~concentration~~ saturation at different σ layers. Shaded ~~area~~ areas indicate the monthly interquartile range (Fig. 2).

deeper pycnal levels are considered. Those correlations ~~remains~~ remain significant ($p < 0.1$) until ~~$\sigma = 15.4\,\mathrm{kg\,m^{-3}}$~~ $\sigma = 15.4$ $\mathrm{kg\,m^{-3}}$ (Fig. 6).

5 Discussion

The regime shift paradigm describes an abrupt and significant change in the observable outcome of a non linear-system, as could result from a threshold in this system response to an external forcing. A periodic model, on the other hand, supposes either a linear response to periodically varying external forcing, or an oscillation resulting from internal dynamics. It is our hypothesis, supported by the quantitative consideration presented above, that the ~~first~~ regime shift model should be favored for interpreting the recent evolution of the Black Sea CIL dynamics. ~~This~~ The prerequisite for the regime shift analysis was

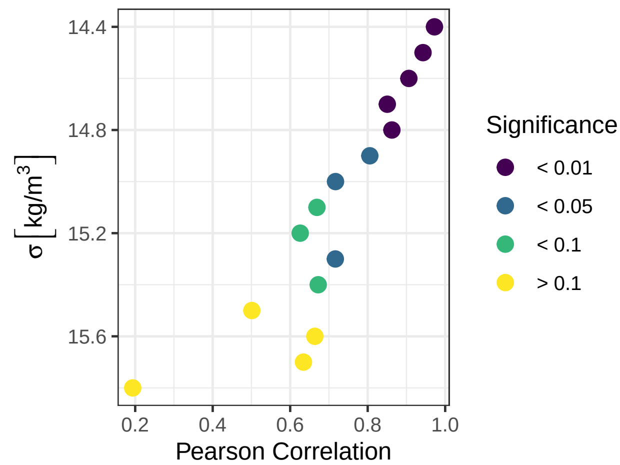

Figure 6. Pearson ~~correlation~~ Correlation Coefficient between basin wide annual oxygenation and CIL formation estimates, for different σ layers. Size of the points relates to the order of magnitude of associated p-values, while colors classify those correlations among classes of significance.

first to issue an unified, synoptic metric to characterize inter-annual variations in the CIL content. The consistency between the different data sources demonstrates the robustness of this metric. To our knowledge, no multiple source comparison have been achieved previously over such an extended period. Note that some dependencies exist among certain sources, as has been discussed explicitly in Sect. 2.

Although, we acknowledge that the statistical advantage (AIC) of the regime shift description is subtle, we consider that it deserves further consideration as this difference in interpretation is fundamental in what regards the expected consequences on the Black Sea oxygenation status and in particular the threat on Black Sea marine populations, whose ecological adaptation (and rate of exploitation) have been ~~build~~ built upon a ventilation regime and consequent biogeochemical balance, that may no longer ~~be considered as routine~~prevail.

~~It so~~ Indeed, it appears that the intermittency of ~~CIL renewal~~ significant CIL formation events characterizes the new ventilation regime: the ventilation of the Black Sea intermediate layers does not occur each year anymore but is occasionally absent for one or two consecutive years. Moreveover, major CIL formation events, which bear the potential of a deeper oxygenation, appears as significantly less frequent.

The extent to which the current regime differs from the previous ventilation regimes is clearly illustrated on a T-S diagram (Fig. 7) : not only are in-situ measurements from the P4 period found in a range of the T-S diagram that was almost never recorded before (temperature above 8.35°C, and σ within $[14.5–15]\mathrm{kg\,m^{-3}}$; Fig. 7a,b,c,d), but two-dimensional density estimates (obtained with R function MASS:kde2) also indicate that such occurrences are now the rule rather than the exception.

As indicated by Stanev et al. (2019), this ~~trends~~ trend may lead to the disappearance of a characteristic layer of the Black Sea, that constituted a major component of its thermo-haline structure and constrained the exchanges between surface, subsurface

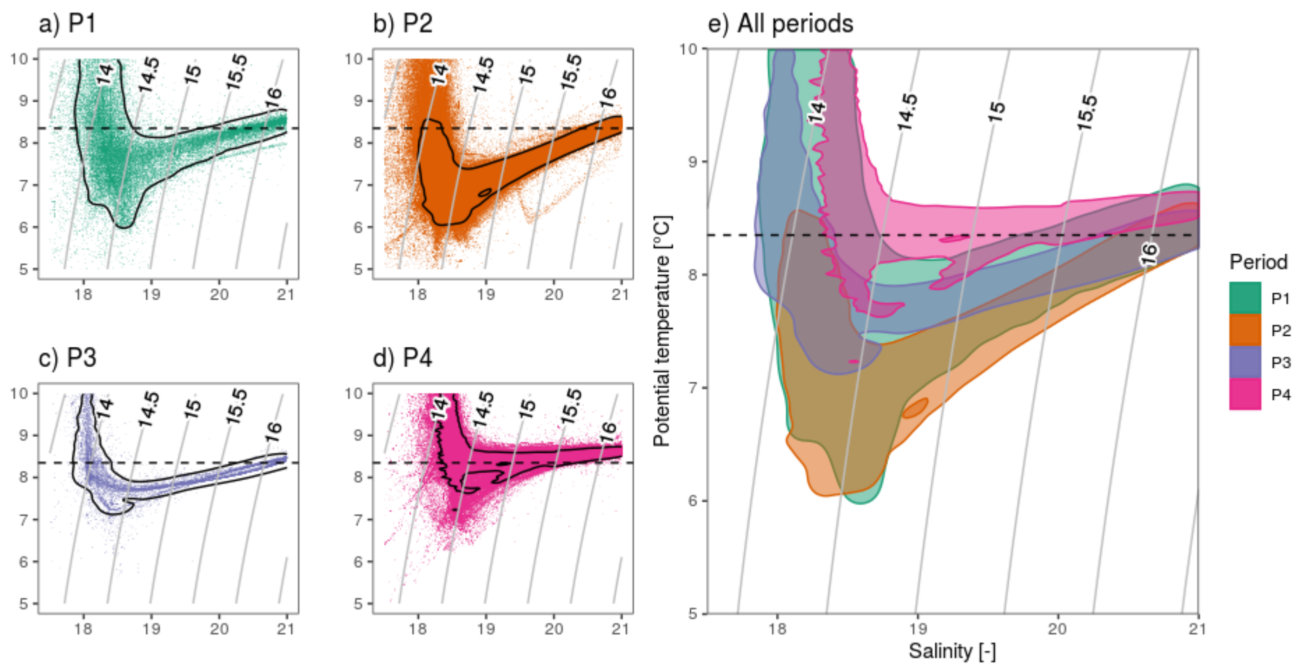

Figure 7. Potential temperature versus salinity (T-S diagram) for bottle, CTD, and Argo in-situ data available from the World Ocean Database for the period 1955–2020 (**?**). Data from periods P1, P2, P3 and P4 are shown on panels a), b), c) and d), respectively. Black contours delineates 75% of the observations for each period, and are repeated with colors in e) for comparison between periods. The black dotted line locates the T_{CIL} =8.35°Ccriterion used to identify CIL waters.

and intermediate layers. In particular, the authors highlight surface and subsurface salinity trends that indicate recent occurrences of diapycnal mixing at the lower base of the intermediate layer, where waters are characterized by a strong reduction potential due to the presence of reduced iron and manganese species, ammonium and finally hydrogen sulfide (Pakhomova et al., 2014).

On a decadal time scale, the average oxygen signature of a given isopycnal layer within the CIL depends on the frequency of CIL formation events of sufficient intensity (Sect. 4), which is in line with the ventilation dynamics depicted by Ivanov et al. (2000) for the upper pycnocline. Although inter-annual fluctuations in CIL formation rate still occur, the regime shift analysis specifically describes a reduction in the frequency of significant ventilation events, and therefore a potential decrease in the oxygen saturation signature in the lower part of the CIL.

A deeper consequence of this reduction relates to the fact that the lower CIL layer also acts as the upper member of the two end-member mixing line that characterizes the Black mid-pycnocline (**?**Ivanov et al., 2000), i.e. between density of about 14.6 to 16 $\mathrm{kg\,m^{-3}}$, following entrainment of CIL waters by the Bosphorus inflow and subsequent lateral ventilation (**?**), the lower end-member being composed by Bosporus waters. Considering the characteristic residence time for the upper (about 5 yr; **?**)

and intermediate pycnocline (9-15 yr; Ivanov et al. (2000)), it is appropriate to consider such temporal average to characterize the oxygen signature of the CIL member composing the mixture of pycnocline waters.

The display of historical oxygen saturation data (1955–2020) on a T-S diagram (Fig 8a), indeed evidences in general a deeper oxygenation during high CIL formation regimes (see lower part of the diagram, characteristic to P2), than in regimes during which significant CIL formation events are rare, or the extreme cases were no CIL waters are visible (see upper part of the diagram, characteristic to P4). This indicates that the analysis linking oxygenation and CIL formation for the recent period (Sect. 4), can be extended to larger time scales by considering changes in the frequency of significant CIL formation events. Thus, the depth until which the reduction in CIL formation may impact on biogeochemical balance of the Black Sea (by affecting oxygenation level) will depend on the period over which the actual ventilation regime will remain.

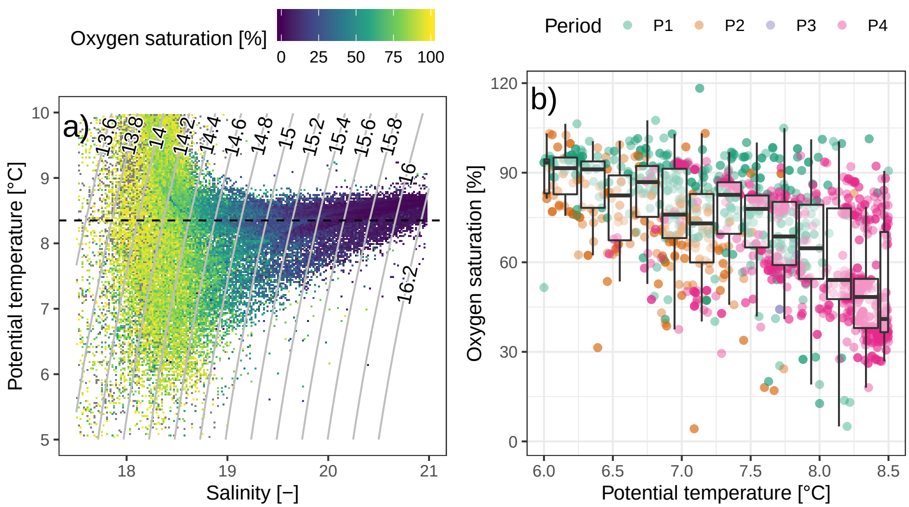

Figure 8. a) Historical oxygen records displayed on the TS diagram. Data collected from the World Ocean Data base for the period 1955–2020 (?). Isopycnal σ layers are indicated by the curved grey lines. b) Highlight of the oxygen saturation conditions along the 14.55 – 14.65 $\mathrm{kg\,m^{-3}}$ σ range of the T-S diagram, i.e. at the core of the CIL layer.

Beyond the changes in convective ventilation highlighted above, it thus appears as a lead priority to assess the biogeochemical consequences of this new thermo-haline dynamics of the Black Sea. In particular, the influence of CIL formation on the biogeochemical components of the oxygen budget should be addressed in more details, asking for instance how the presence or absence of CIL formation influences on planktonic growth, trophic interactions, and organic matter respiration rates. We voluntarily adopted here a wide integrative point of view, so as to highlight the large scale relevance of the depicted regime shift on Black Sea oxygenation. However, we still consider that a detailed assessment of all components of the oxygen budget, ~~ie.~~ i.e. considering ventilating processes and biogeochemical production/consumption terms, is required in order to infer the future evolution of the Black Sea oxygenation status (e.g. **?**).

Although, there are clear indications of a long-term warming trend in the Black Sea (Belkin, 2009), it remains a delicate task to strictly dissociate the contributions of global warming from that of regional atmospheric oscillations (Kazmin and Zatsepin, 2007; Capet et al., 2012). One such assessment in the neighboring Mediterranean ~~sea~~ Sea (Macias et al., 2013) concluded that global warming trend and regional oscillation contributed to the recent regional sea surface temperature trend (1985–2009), for ~~respectively~~ 42% and 58%, respectively. While corresponding assessment will have to be routinely reevaluated for the Black Sea as time series expand, it may conservatively be considered that global warming had a significant contribution to warming winters in the Black Sea. This contribution is expected to increase in the next decades (Kirtman et al., 2014).

6 Conclusions

We have analyzed the variability of the Black Sea ~~ventilation~~ CIL formation over the last 65 years and investigated the existence of regime shifts in this dynamics. To this aim, we have produced a composite time series of the CIL cold content (C), that is considered as a proxy for the intensity of the convective ventilation resulting from the formation of dense oxygenated waters. This composite time series is built from four different data products issued from observations and modelling, so as to optimize its temporal extent in regards to preceding studies (e.g. Oguz et al., 2006). The consistency between those products, and in particular the close correspondence between observational and mechanistic predictive time series supports the reliability of the composite series and its adequacy to describe the evolution of the Black Sea subsurface convective ventilation during the last 65 years.

The composite time series was analyzed to detect different regimes, corresponding to periods characterized by significantly distinct averages. Over the last 65 years, we identified 3 main regimes: 1) a routine regime prevailing during 1955–1984 and 1996–2008 that is consistent with the full-period average C, 2) a cold regime (high C, 1984–1996) which ~~as~~ has been previously documented (see references in Sect. 3) and 3) a warm regime (low C, 2008–2019) which is characterized by the intermittency of the annual partial CIL renewal. ~~Statistical considerations indicates~~ Statistical considerations indicate that the abrupt shift can not adequately be described by a ~~long term combination of~~ combination of long term linear and periodic trends.

~~Clear relationships evidence~~ The synoptic CIL formation rates provided by the 3D hydrodynamical model, and the detailed description of oxygenation conditions provided by BGC-Argo floats, allowed us to detail the role of CIL formation in oxygenating, through convective ventilation, the upper part of the Black Sea intermediate layers (~~ie~~i.e. between σ of 14.4 and 15.4 $\mathrm{kg\,m^{-3}}$). Given that cold winter air temperature is the leading driver of CIL formation (Oguz and Besiktepe, 1999; Ivanov et al., 2000; Capet et al., 2014), given that CIL formation constitutes a predominant ventilation mechanism for the Black Sea intermediate layer, and assuming that oxygen conditions constitutes an environmental structuring factor affecting the ecosystem organization, its vigor and its resilience, this shift in the Black Sea ventilation regime may be associated with global warming and is expected to affect its biogeochemical balance and to threaten marine populations adapted to previously prevailing ventilation regimes.

To understand how global warming impacts on the marine deoxygenation dynamics is a worldwide concern. The relatively fast and clear response that stems from the specific Black Sea geomorphology makes it a natural laboratory to study this dependency and related phenomena. Here, we showed that non-linear dynamics and ~~feed-backs~~ feedbacks in ventilation mechanisms resulted in a significant shift of the average ventilation regime, in response to rising air temperature. Since the temporal extent of low oxygen conditions is critical for ecosystems, we stress the importance to assess the potential for similar ventilation regime shifts in other oxygen deficient basins.

Data availability. The data used are listed in Table 1 Argo data were were collected and made freely available by the Coriolis project (http://www.coriolis.eu.org) and programs that contribute to it. Era-Interim atmospheric conditions were obtained from ECMWF interface (http://apps.ecmwf.int/datasets/). Aggregated weekly outputs of the GHER 3D model, as well as processed annual time series from the different sources are publicly available on a ZENODO repository : https://doi.org/10.5281/zenodo.3691960.

Appendix A: Comparison of the C) time series issued from different data sources

The C time series are noted denoted C_i^m for source m (i is the year index). Each pair of time series (C_i^m, C_i^n) are compared over the years $i \in I^{m,n}$ for which C_i^m and C_i^n are both defined. The following statistics are given for each pair of data sources in Fig. ?? A1 :

- $N^{m,n}$ the number of elements in $I^{m,n}$
- Pearson correlation coefficient :

$$\frac{\sum\limits_{i\in I^{m,n}} (C_i^m - \overline{C^m})(C_i^n - \overline{C^n})}{\sqrt{\sum\limits_{i\in I^{m,n}} (C_i^m - \overline{C^m})^2}\sqrt{\sum\limits_{i\in I^{m,n}} (C_i^n - \overline{C^n})^2}} \tag{A1}$$

- The RMS difference between time series :

$$\frac{\sqrt{\sum\limits_{i\in I^{m,n}} \left(C_i^n - C_i^m\right)^2}}{N^{m,n}} \sqrt{\frac{\sum\limits_{i\in I^{m,n}} \left(C_i^n - C_i^m\right)^2}{N^{m,n}}} \tag{A2}$$

- The average bias :

$$\frac{\sum\limits_{i\in I^{m,n}} (C_i^n - C_i^m)}{N^{m,n}} \tag{A3}$$

- The percentage bias :

$$\frac{\sum\limits_{i\in I^{m,n}} (C_i^n - C_i^m)}{\sum\limits_{i\in I^{m,n}} \frac{(C_i^n + C_i^m)}{2}} .100 \cdot 100 \tag{A4}$$

For a better appreciation of variation scales, the temporal standard deviation is also shown for each data source.

The last value of the atmospheric predictor time series (C_{2013}^{Atmos}, Fig. 1. of the main manuscript; Fig. 1) was not considered in the composite time series, as it was based on the two, rather than three, predictor values available at the time of publication (hence the larger associated uncertainty). It is remarkable, however, that the published prognostic values for 2012 and 2013, match with independent Argo estimates (Capet et al., 2014).

Appendix B: Regime shift analysis

The ~~identification of change points in a time series is the natural first step towards identification of a regime shift (Andersen et al., 2009). The~~ basic change point problem that is considered in this study can be expressed as follows: to identify the change point ~~$i = k$~~ $i = k$ in a sequence ~~x_i~~ x_i of independent random variable with constant variance, such that the expectation of x_i is μ for ~~$i < k$~~ $i < k$ and $\mu + \Delta\mu_k$ otherwise. Obviously, this problem can be generalized to several change points. The procedure for change point detection is stepwise and has been achieved following the methodology described in the documentation of the R package `strucchange` (Zeileis et al., 2003).

First, the existence of at least one significant change point had to be tested. `strucchange` provides seven statistical tests to compute the ~~p-value~~ p-value at which the null hypothesis of no change points can be rejected. The presence of change points can be tested on the basis of F-Statistic tests or generalized fluctuation tests (Zeileis et al., 2003, and references therein). Table A1 provides the significance level at which the null hypothesis can be rejected for each test implemented in the `strucchange` R package. Among the seven tests considered to assess the presence of at least one significant change point in C_i, ~~6 rejects~~ six reject the null hypothesis with a significance level $p < 0.05$.

~~Tests for the presence of significant structural changes in C_i. p-value indicates the probability that the null hypothesis (i.e. there are no significant change points) should be maintained. Approach Test p-valueF-Statistics supF test 1.7e-04aveF test 5.3e-03expF test 1.7e-08Fluctuations OLS-based CUSUM test 1.0e-02Recursive CUSUM test 3.6e-01 OLS-based MOSUM test 1.0e-02Recursive MOSUM test 1.0e-02~~

Second, the locations of the N most likely change points were identified. In this study, we considered from one to five change points. The change point locations can be estimated by finding the index values $k_n = [k_1, .., k_N]$ that maximize a likelihood ratio, defined as the ratio of the residual sum of squares for the alternative hypothesis (i.e. change points at k_n with $\Delta\mu_{k_n} \neq 0$) to that of the null hypothesis (i.e. no change point, $\Delta\mu_{k_n} = 0$).

~~Finally, the choice of a given model (ie. number of change points) had to be considered using the AIC criterion (Sect. 2.2).~~

Appendix C: ~~Statistics~~

Formally, the methodology to identify and date structural change is designed for normal random variables, two conditions which can not be ~~granted~~ guaranteed for environmental time series such as considered here. ~~The following sections detail~~ We detail here why 1) the departure of C distribution from a ~~gaussian~~ Gaussian distribution, 2) the *autocorrelation* in the composite time series C_i, and 3) the biases between source-specific components of ~~the composite time series~~ C_i, do not affect the conclusions drawn above.

B1 Normality

Skewness in the distribution of C values and its departure from normality is visible at low C values (not shown), as expected for physical reasons: C is a vertically integrated property, naturally bounded by zero. However, the Shapiro-Wilk test that measures the correlation between the quantiles of C_i and those of a normal distribution, indicates no significant departure from normality: $W = 0.975$, $p = 0.23$. For completeness, a Box-Cox transformation ($\lambda = 0.7$) of the original data has been tested which slightly enhances the Shapiro-Wilk test ($W = 0.98$, $p = 0.39$), but brings no sensible alteration in the conclusions of the structural change analysis.

B2 Autocorrelation

Similarly, C_i can not be considered as a random variable. In particular, we introduced in Sect. 1 the CIL preconditioning and partial renewal mechanisms, both physical reasons for which autocorrelation may be expected in C_i. Indeed, correlations between the original and k-lagged time series are, at first glance, significant up to $k = 5$ (~~Tab. A2,~~ Table A2; the confidence interval above which autocorrelation can be considered to be significant is given by $1.96/\sqrt{N} = 0.25$). However, it should be considered that the regime shift evidenced in ~~the main manuscript~~ this study may itself induce apparent autocorrelation statistics. To evidence that this is indeed the case encountered here, the correlation between the original and lagged time series of *the residuals* stemming from the 4–segments change point model are indicated in ~~Tab. S~~ Table A2. The fact that no significant autocorrelation persists when change points are considered indicates that the non-randomness of C_i does not jeopardize conclusions drawn from the application of the structural change methodology.

~~Correlations between (first column) time-lagged replicates of the original C_i and (first column) time-lagged replicates of the residuals of the 4–segment model. Lag Original Residuals0 1.0 1.01 0.57 0.132 0.38 -0.223 0.32 -0.114 0.33 0.035 0.26 -0.05~~

B3 Biases between components of the composite time series

Given that biases exist between different data sources, it might be argued that the composite time series is skewed by the uneven temporal ~~cover~~ coverage of the different sources. For instance, if a strongly biased source would solely cover a given period, the composite series would be biased over that period. To ensure that this issue does not affect the presented conclusions, $C_i^{unbiased}$ was constructed as C_i, but removing from each component C_i^m the bias identified ~~identified~~ with the longest C_i^{Ships} time series (which series is used for reference does not impact on structural change conclusions). When $C_i^{unbiased}$ is considered instead of C_i, similar results are obtained in terms of change point models significance and positions of the change points.

Appendix C: ~~Oxygen in situ-data~~

~~Oxygen saturation was computed as the percentage of in situ oxygen concentration (converted to μM using in-situ density), with respect to oxygen concentration at saturation, computed considering atmospheric pressure, in-situ temperature and salinity.~~

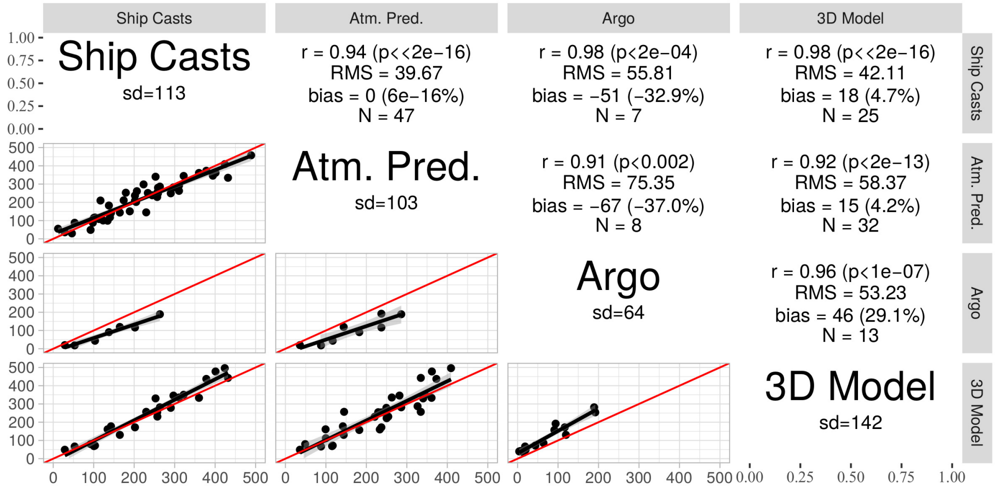

Figure A1. Statistics of comparison between the different data sources.

Table A1. Tests for the presence of significant structural changes in C_i. p-value indicates the probability that the null hypothesis (i.e. there are no significant change points) should be maintained.

Approach	Test	p-value
F-Statistics	supF test	1.7e-04
	aveF test	5.3e-03
	expF test	1.7e-08
Fluctuations	OLS-based CUSUM test	1.0e-02
	Recursive CUSUM test	3.6e-01
	OLS-based MOSUM test	1.0e-02
	Recursive MOSUM test	1.0e-02

Table A2. Correlations between (second column) time-lagged replicates of the original C_i and (third column) time-lagged replicates of the residuals of the 4–segment model.

Lag	Original	Residuals
0	1.0	1.0
1	0.57	0.13
2	0.38	-0.22
3	0.32	-0.11
4	0.33	0.03
5	0.26	-0.05

~~Both oxygen concentration and saturation levels were extracted from each Argo profiles at regular σ intervals (14.4 to 16.0 $\mathrm{kg\,m^{-3}}$, by steps of 0.1). Oxygen concentration $\mu\mathrm{M}$is directly relevant as an environmental factor affecting chemical reactions rates. Oxygen saturation level %is cleared from the direct temperature effect associated with changes in the solubility of oxygen in seawater, and is arguably more relevant as an environmental factor to represent the impact on living organisms (**??**). However, the range of thermo-haline variability in the subsurface and intermediate layers, on which we focus our analysis of oxygen, is relatively narrow compared to surface waters. Oxygen concentration and saturation level are thus highly correlated for the layer of interest (Fig. **??**) and only oxygen concentration is considered in the following .~~

~~Oxygen concentration $\mu\mathrm{M}$and oxygen saturation level %derived from Argo samplings above (left) and below (right) the isopycnal $\sigma = 14.3\mathrm{kg\,m^{-3}}$. The color scale gives the potential desnity anomaly associated with each sampling (σ - $\mathrm{kg\,m^{-3}}$).~~

Author contributions. AC processed the data, set the regime shift methodology, achieved the analyses, issued the visualizations and wrote the initial version of the manuscript. All authors contributed to define the general methodology, to discuss the results and to revise the final mansucript. MG supervised the research.

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Competing interests. No competing interests are identified.

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