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Interactive comment on "Climate change induced a new intermittent regime of convective ventilation that threatens the Black Sea oxygenation status" *by* Arthur Capet et al.

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Review:

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

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 understandng 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.

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 changepoint 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. 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 changepoint analyses, and each need not be only based on the mean level, but could use other metrics (variance, auto-correlation, skewenss) to break up the series. 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.

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 adequate 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 point 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 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).

SPECIFIC:

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.

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?

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

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

Regime shift analysis and number of changepoints. I am unclear on whether the R-package 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).

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).

Figure 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 break-points as optimal, then refer to appendix.

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

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.

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

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.

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).

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.

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