Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-76-RC3, 2020

Biogeosciences Discussions
Open Access
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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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Received and published: 5 June 2020

Review: Biogeosciences BG-2020-76 Authors: Capet, Vandenbulcke and Gregoire Title: Climate change induced a new intermittent regime of convective ventilation that threatens the Black Sea oxygenation status 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)

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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, 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. 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. 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 as 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. 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. 5. L104 Why not describe the data sources in the same order as presented in Table 1? 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. 7. L136 Does that 3D hydrodynamic model include source

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water from the NW shelf? 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. 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. 10. L181 Why 2018 and not 2020? 11. L289 I think Konovalov and Murray (2001) showed this in a figure. 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. 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.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2020-76/bg-2020-76-RC3-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-76, 2020.

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