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Interactive comment on "Looking beyond stratification: a model-based analysis of the biological drivers of oxygen depletion in the North Sea" *by* F. Große et al.

Anonymous Referee #2

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Looking beyond stratification: a model-based analysis of the biological drivers of oxygen depletion in the North Sea

General Comment: In this study an ecosystem model is used to characterise and to explain the main biological and physical factors that drive oxygen dynamics in the North Sea. The authors show that the North Sea can be divided into three different regimes in terms of oxygen dynamics (although zones may be a more appropriate term, as regime may get confused with the term regime shift?). They suggest that this demonstrates the usefulness of ecosystem models in interpreting observations and estimating impacts

of anthropogenic drivers on oxygen conditions in the North Sea.

The manuscript is well written and very clearly explained throughout. The model accurately captures the temporal and spatial development of oxygen conditions in the North Sea (although some positive bias is present). It captures the temporal development and duration particularly well as shown against in situ mooring data. The in-depth analysis of the physical and biological factors responsible for oxygen utilisation in the different areas is compelling and provides an excellent basis for characterising the different areas in the North Sea.

The manuscript provides a valuable and detailed contribution towards understanding oxygen dynamics across the North Sea and the relative contribution of different factors in influencing this dynamic. This understanding in turn should help scientists and environmental managers better understand the relative contribution of different drivers (natural variability, climate change, and anthropogenic nutrient inputs) and put them in a position to make informed decisions about the necessary management measures which might be required to protect this valuable natural resource.

The manuscript is suitable for publication, but I think the material and findings could be further improved by addressing two issues.

First, the paper is very long and some attempt should be made to shorten it. It must be at least 20,000 words in length; does the journal have a maximum word count policy? The authors should consider shortening the paper and more succinctly explaining the main messages of the paper. At its current length the authors are likely to reduce the size of their audience. There's a considerable amount of superfluous material in the methods section which could be removed (see below).

Second, I think the context of the paper needs to be clarified. I am a little bit confused when the term hypoxia is used. I see no evidence of the presence of hypoxia in the North Sea, if you consider that hypoxia is widely (in the scientific literature) defined by oxygen concentrations of less than 2-3 mg L-1. The minimum observed oxygen concentration referred to in this manuscript was 5.9 mg L-1 in 2001, 6.2 mg L-1 in 2005 and 7.2 mg L-1 in 2008, well above the hypoxia threshold. The paper needs to make a clear distinction between very low hypoxic conditions and oxygen deficiency. It would appear that the latter condition is more relevant to the North Sea. And oxygen deficiency at levels well above the normal hypoxic levels of 2-3 mg L-1 can have detrimental effects on biology (see Vaquer-sunyer and Duarte 2008). However, I think a very important question that could be addressed by the authors is: are the levels of oxygen deficiency observed and modelled in the paper indicative of oxygen deficiency or are they representative of background levels in a seasonally stratified water column where you may have a combination of factors at play? It would be very interesting to pose this question for each of the regimes/zones identified in this manuscript?

Having answered that question the authors could comment on the suitability of the OSPAR assessment threshold of 6 mgL-1. Is this an appropriate assessment level for each of the regimes they have identified? Keeping in mind that an appropriate assessment threshold, should be flexible, taking into account natural variability and uncertainty. This is an important question, because meeting this threshold has important implications for the management of human activities in countries whose riverine inputs contribute towards the eutrophication status of the North Sea. I have relatively few specific and technical comments but these are given below.

Specific Comment: In the first line of the introduction it would be useful to define what is meant by low oxygen conditions in terms of oxygen concentration. Because based on this paper I see no evidence of the presence of hypoxia in the North Sea, if you consider that hypoxia is normally defined by oxygen concentrations of less than 2-3 mg L-1. The minimum observed oxygen concentration referred to in this manuscript was 5.9 mg L-1 in 2001, 6.2 mg L-1 in 2005 and 7.2 mg L-1 in 2008, well above the hypoxia threshold. If the focus here is more on oxygen deficiency, that is a deviation from oxygen concentrations present in temperate stratified waters in the absence of anthropogenic pressures, then that should be made clear.

A conceptual map of the three regimes/zones should be included.

I would suggest shortening the methods section which is too long. There are a number of places in the methods section where methods of work already published are repeated. It would be sufficient to refer to the original publication. For example, page 12556, information on calibration/validation of SmartBuoy data is given but this is already given, as indicated in Greenwood et al., 2010). Furthermore, it isn't necessary to give superfluous information on the monitoring progammes (e.g. the MARNET and others).

12564, lines 16-24 and the point that follows. This is an important point particularly for monitoring authorities who are responsible for designing monitoring programmes. It should help with the design of more efficient targeted monitoring programmes.

12568, lines 11-14. Should the hypothesis be built around understanding the reasons why there are differences in bottom oxygen under similar stratification scenarios rather than just focused on why there are low oxygen conditions in the bottom layer?

Table 1. Would it also be informative to include average water column depth and maximum depth of the MLD in the table? Also what about including DO values at the end of the stratification period as well as initial DO values?

Technical corrections: Abstract: line 12, is the bottom layer not always below the thermocline? 12546, line 7, indicates that respiration only occurs below the thermocline, which is obviously not the case. 12547, lines 6-18, reference to oxygen depletion and low oxygen concentration but no information on the corresponding concentration that was observed? Please provide information on concentration if available. 12547, line 22, in relation to figure 1 and use of the <6 mgL-1 is much higher than the normal hypoxia threshold. Even if we acknowledge that biological impacts may occur at levels above the lower hypoxia threshold levels closer to the 6 mg L-1 threshold are likely to have minimal impacts on organisms that are resident in stratified environments. 12556, line 11. Why use the phrase so-called? 12563, line 1, state the values rather than saying less than? 12569, line 7, yellow boxes? 12572, line 16, give the rate value.

Interactive comment on Biogeosciences Discuss., 12, 12543, 2015.