Responses to Anonymous Referee #1
Below the review is reproduced in black font and our responses interspersed in blue.

Comments:
The paper describes a new configuration of a regional coupled ocean-circulation biogeochemical model. The focus is on simulating the variability of oxygen. The main point of the paper is the description of the simulated interplay of biogeochemical processes, oceanic circulation and air-sea gas exchange which results in a variability of pelagic oxygen concentrations.

I see two routes the paper could go from there to be of interest to the wider audience addressed by Biogeosciences:

(1) Highlight one (or more) process which you have identified in your model and which have not been thought about in the past (in the literature). (I guess my main point here is that your "new" science is not easy to recognize. A good example is your abstract where the reader is left wondering which of the processes have been "discovered" by the authors.)

Reply: We believe we can make much clearer in our revision what the new insights and contributions of this study are. We appreciate the Reviewer making this point and forcing us to clarify and emphasize this.

(2) Convince the reader that your model simulation is a realistic copy of reality.

Reply: We intend to expand the model validation by including a model-data comparison for nitrate. In addition, we have presented model-data comparisons for oxygen, chlorophyll, satellite and in-situ temperature and salinity.

In its present form, I got the impression that the manuscript apparently has chosen route (2). The problem I have with this is: the only observations presented that give an impression of the model's fidelity are the oxygen data in Fig. 2. It is not very much data and the fit is not very good either. The overall correlation is 0.76 so your model explains less than 60% of the variance (of which a considerable fraction may be associated to the seasonal cycle meaning that the correlation in each subpanel of Fig. 2 is probably much less.) For comparison: global models are well above 0.8 for nutrients (e.g. Laufkoetter et al. 2015 their Fig. 1) and even for oxygen (e.g. Matear & Lenton 2014 their Fig. 1).

Reply: We are not sure that we agree with the Reviewer's assessment that this manuscript has to choose either route (1) or (2). We believe we are presenting new insights into the oxygen dynamics of the ECS and are doing so with a new model set-up that we believe to be superior to previous models published for the region and that is validated as rigorously as possible given the limited availability of observations. We don't think that a comparison of our model's correlation coefficient with those from global models is meaningful.

In order to give the reader a chance to put your model results into perspective I suggest that you extend you model evaluation section - preferably with in-situ measurements of nutrients,

temperature and salinity (and/or sea surface temperature, chlorophyll, sea surface height estimated from space).

Reply: We are wondering whether the reviewer may have missed the Supplement which includes model-data comparisons of satellite temperature and chlorophyll (S1 and S6) and in-situ temperature and salinity (S2 to S4). Missing was a comparison for nutrients, but fortunately we recently became aware of a suitable nutrient data set. We intend to add a comparison of nutrient distributions to the revised manuscript and will add a subsection dedicated to model validation. We believe that these comparisons give a sufficient level of confidence for us to proceed with an analysis of the simulated oxygen dynamics in the region presented in the manuscript.

References: Laufkoetter et al. 2015 doi: 10.5194/bg-12-6955-2015

Matear & Lenton 2014 doi:10.5194/bg-11-3965-2014