

***Interactive comment on “Modeling the seasonal cycle of the oxygen minimum zone over the continental shelf off Concepción, Chile (36.5° S)” by J. Charpentier et al.***

**Anonymous Referee #1**

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General comments

The manuscript by Charpentier et al. deals with oxygen dynamics on the continental shelf off Chile with a simple 1-D model. The region of study counts with time-series observations as well as glider data, which are used to build up the model. Their main result is that lateral physical processes are largely responsible for the observed seasonal oxygen variability in the area, while biological and vertical processes play a secondary role. The 1-D model and the derived results are interesting and worth of publication. However, I feel the paper would benefit from some re-organization, especially to make it easier for the reader to get to the main point and to distinguish between novel and

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previously published work. Also, the different sensitivity studies should be described, at least briefly, in the main text to prepare the reader to go through the results.

1) Already in the abstract the authors say that they identified two modes, winter and summer. However, at the beginning of section 3 the authors cite several studies that described and analyzed the oxygen seasonal pattern. My understanding is that what this paper developed was the “typical” winter and summer profiles, rather than the identification of the modes. It should be made clearer in the text what is new in this paper and what has been described before. In order to do this, I would recommend having all the information from the literature in sections 1 and 2 (probably the introduction should be trimmed so it does not get too long). Section 3 should mainly describe results/figures from this manuscript. Moreover, I am unsure if these descriptions add new information relative to the literature cited, or if the added information is relevant to the main point of the study (ie, the model and its results). A suggestion would be to remove the whole section 3 from the manuscript, since it distracts from the main results.

2) The paragraph in page 7236, lines 5-13 (section 3) has information that belongs to the introduction.

3) I think it would be useful to describe the sensitivity experiments in the text, probably in section 4.1. At the end of this section, the authors say that 4 parameterizations for the piston velocity were tested; this paragraph would be a good spot to introduce the other tests (mentioning explicitly the letter associated to each experiment). Or move all that information to the first paragraph of section 4.2.

4) Summary and conclusions: This section could/should have more content. So far, it has a summary as a first paragraph and the reminding paragraphs are more “future work” than “conclusions”. As it is, I remain hoping to read about the implications of lateral processes dominating seasonal oxygen dynamics and how these results agree/disagree with other studies in this or other regions (e.g., Connolly et al 2010, Bianucci et al 2011 for the North Pacific OMZ). Moreover, in the second paragraph:

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The first sentence gives me the impression that this model dealt with river runoff and meridional advection, leading to the recommendation of using those in future modeling attempts. It should be re-worded, to make clear that these processes were not included here, but probably should be in future models (also give reasons why they should be included).

#### Specific comments

- 1) If using "DO", the acronym needs to be defined the first time it is used and then DO should be used everywhere in the manuscript. Otherwise, spell out "oxygen" everywhere, but need to be consistent.
- 2) Oxygen units should be consistent through the manuscript and figures. At this point, I've read  $\mu\text{M}$ , mL/L, and  $\mu\text{mol/kg}$ . Choose one (eg, the units from the model output,  $\mu\text{M}$ ) and report all the oxygen numbers in the text and figures in that unit.
- 3) Page 7230, line 13: there should be a better reference for this statement, rather than "it is widely held"
- 4) Page 7231, line 6. I agree there are not tons of models dealing with oxygen, but "Even scarcer" sounds too strong. There are many other modeling studies that include oxygen (e.g., Deustch et al., JGR, 2006; Frolicher et al, GBC, 2009; Bianucci et al., JGR, 2011).
- 5) Pages 7238-7240: The experiments are called first "Biology one" and "Biology two", but then referred to "bio 2" and "bio 2". Again, choose one and be consistent.
- 6) Page 7239, line 15: reference should be to Eq. 4, rather than to Eq. 3?
- 7) Page 7240, lines 16-20: should "bio 1" and "bio 2" be inverted? (bio 2 is the one with the logarithmic decay)
- 8) Figure 6: panels should say which ones are for winter and which ones are for summer.

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- 9) Figure 7: panel d should have the same colors for winter and summer as panels a-c.
- 10) Figure 8: I cannot see lines for experiments D and E in panels c and d.

#### Technical corrections

1) There are many grammar mistakes along the manuscript (e.g., singular verbs after plural nouns and vice versa). For instance, in the abstract "A simple model that accountS for the main physical and biological processes that influencE...". There are also some typos, e.g. in page 7232, line 8 says "where" instead of "were". These are just a few examples. Moreover, the text doesn't read smoothly over some parts of the main text. I'd recommend a careful revision of the language (it would be ideal if an English speaker could read the manuscript).

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