

Interactive comment on “Characterizing deep-water oxygen variability and seafloor community responses using a novel autonomous lander” by Natalya D. Gallo et al.

Anonymous Referee #2

Received and published: 7 April 2020

General comments: This paper addresses the pressing problem of progressing ocean deoxygenation and the effect of variable oxygen availability on fish and epibenthic invertebrate communities along a depth gradient of 100 to 400m off San Diego, CA. For time series measurements of oxygen and other environmental parameters, the authors use a novel lander system, which harbors a SBE CTD, oxygen sensors and a camera/light system. The camera provides video sequences, which were used for the community analysis. The introduction of this novel lander system is a major focus of the MS. This study represents an interesting approach of how benthic community data can be obtained and related to physico-chemical time series measurements, it is based on an extended data set of seven lander deployments and might be very interesting for

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a wider marine ecological community of scientists.

The paper is very well and concisely written. The manuscript is well structured, the methods are appropriate and the data, which are novel are well presented. The authors being well aware that their study does (or better say can only include) a limited number of environmental parameters are careful about their conclusions and present those in an appropriate way.

The MS has a few very minor weaknesses that can be easily solved, which I would like to raise in the following.

I very much like the idea of the small-sized and hand-operated lander and I fully agree with the need for such systems for the performance of more in situ long-term observations. Yet, given the actual size and weight of the lander, the expression ‘Nanolander’ seems a bit exaggerated. This is just a personal opinion and is by no means meant to urge the authors to change the name of their system. In this context, the last section of the MS “A global array of deep-sea landers” goes in the same direction and appears a bit superficial with an emphasis on “selling” the system. The authors might consider to rewrite this last section increasing its profoundness.

As the paper claims to introduce a novel lander-technology, I would have wished to find a brief review of similar already existing systems. The authors mention papers by Jamieson et al. but do not provide details. Please add a few lines highlighting where your system goes beyond existing systems.

Beside oxygen, other parameters were measured (temperature, pH, saturation state of aragonite/calcite) but these were hardly mentioned in the discussion section although e.g. pH in respiration physiology is very important. Please clarify why these parameters were not further included in the interpretation of the data set.

Further comments and edits:

Line 24: please explain “phest”

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Line 67-72 in this context eddy correlation techniques could be mentioned

Line 108: suggest to use only metric units of m or cm instead of ft

Figure 1, suggest to include a more detailed technical drawing of the lander (i.e. better version of Fig. 1A) where the different major components are labeled with numbers which can be referred to in the main text. Figure 1D is not really providing any additional information and could be omitted. Please provide in the final version of the MS the figures in sufficient resolution.

Line 111: “glass filled” sounds a bit odd; do you mean glass-spheres housed by polyamide protective shells

Line 122: “The power supply for the BART board is housed in the upper sphere”, together with the Bart board?

Line 123ff: what would be the maximum deployment time of DOV Beebe with the given battery systems?

Line 131: would be nice if especially details of the camera system could better show up in the improved version of Figure 1A

Line 153: please use metric units

Line 178: I think there is no need to use the word “high-frequency” (it’s rather a matter of the perspective whether 5 min sampling rate is high-frequency or not)

Line 194 please describe spiciness in a bit more detail, it’s likely not common to everybody

Line 309 deconstructed time series - please explain in more detail

Figure 4: the labels for “day” and “night” are difficult to read – please enlarge

Line 448: I am not sure whether the statement “At ~200 m, oxygen, temperature, and pH exhibited high variability (Fig. 2), greater at times than the variability observed

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at 100 m.” is correct for temperature – please check. Although the Figure 2 is quite attractive and informative, especially for the discussion section, when environmental variability is discussed additional Box plots might be helpful to elucidate the differences between the different deployments (i.e. depths).

Line 476 Turbidity can be related to local hydrodynamics caused by the energy dissipation of incipient internal tides at sloping boundaries affecting the suspension, transport and deposition of food particles. If you are interested, please see e.g. Mosch et al. (2012) Factors influencing the distribution of epibenthic megafauna across the Peruvian oxygen minimum zone. *Deep-Sea Research I* 68 (2012) 123–135 and references therein.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-75>, 2020.

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