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

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This paper presents novel results about oxygen variability over hours, days, and weeks and community composition at depths between 100 and 400 m along the nearshore environments in the Southern California Bight (SCB), reporting noble data collected using a new autonomous lander. I was very much interested to read the paper and sure that it will be of interest to many oceanographers; so I hope that this work can be published sooner than later. One of primary value of this paper lies in presenting high-frequency oxygen variability (periods of hours, days, and weeks) in different settings (depth, bathymetry, and season) of the SCB. I am impressed by the scope of the work

presented in this paper and by the envisioned global array of deep-sea landers that I had hoped to see for a decade. Authors presented a strong case convincing me (and readers to my mind) how deep-water oxygen variability can be characterized and that lower oxygen conditions to be associated with a transition from fish-dominated to invertebrate-dominated communities and that this taxonomic shift may be a useful ecological indicator of hypoxia. The hand-deployable Nanolander used in this study will be a powerful capacity-building tool for characterizing environmental variability and examining seafloor community sensitivity to climate-driven changes.

The manuscript is very well written, within the scope of Biogeosciences and informative, contributing to scientific progress substantially. No methodological flaws were detected. My overall recommendation is minor revision. The manuscript raises a few questions/comments some of which need to be addressed before publication to revise it in more attractable form. These are:

1) What is the bottom topography around the Nanolanders? Table 1 well summarizes the seven deployments including information on deployment location and depth. But, 'Scripps Coastal Reserve', 'Del Mar Steeples Reef' with latitudes/longitudes and bottom depth are not enough information for readers (particularly someone who is not familiar to the region) to figure out local bathymetric features, where outer shelf and upper slope are located/ranged/shaped, seafloor area exposed to different oxygen conditions, and so on. It is important to give details of the bathymetry around the deployment sites highlighting the key information as mentioned above. This would also be helpful for better discussing physical drivers of the oxygen variability. Thus, I would like to suggest to add one figure (or incorporated into Figure 1) showing compact and easy to understand map of the local bathymetry along with the deployment locations.

2) There is no summary/concluding remarks/conclusion in the manuscript. Substantial conclusions are reached but they are not presented as a separate section. Thus, I would like to suggest to add Section 5 to conclude or summarize the materials.

3) To give proper credit to related work, I would like to suggest to use '13CW', name of specific water mass linked to the deoxygenated water, instead of its locally defined water types, Pacific Equatorial Water (PEW) although previous works used the terms PEW. Based on recent work (Zachary et al., 2020; "The role of water masses in shaping the distribution of redox active compounds in the Eastern Tropical North Pacific oxygen deficient zone and influencing low oxygen concentrations in the eastern Pacific Ocean" published in Limnology and Oceanography as of 06 February 2020), two water masses – 13CW and deeper North Equatorial Pacific Intermediate Water (NEPIW) act as the two Pacific Equatorial source waters to the California Current System corresponding to upper and lower PEW at isopycnals of 26.2-26.8 kg m-3 when defined locally. Here, the relevant water mass seems to be 13CW (upper PEW), and not NEPIW (lower PEW).

4) The observed oxygen variability over short time scales was compared with multi-decade-long deoxygenation or long-term trends/shifts reported in Bograd et al. (2008) and McClatchie et al. (2010). However, it was not discussed in comparison to inter-annual oxygen variability in the region. Does the period of data collection from August 2017 to March 2018 correspond to normal or more likely abnormal (El Niño/La Niña) year? My suggestion is to provide discussions on the observational results in terms of significant local interannual oxygen variability in association with such large-scale condition presented in Nam et al. (2011; "Amplification of hypoxia and acidic events by La Niña conditions on the continental shelf off California" published in Geophysical Research Letters as of 23 November 2011).

5) What are depths of thermocline/oxycline (any strong vertical temperature/oxygen gradient close to 200 m?) and their sectional structures across the shelf-slope? It would be helpful to check the cross-sectional structures of water temperature and dissolved oxygen across the shelf and slope at a given time, e.g., see Figure 2 of Nam et al. (2011) but focusing on the deeper area (over the slope). Both mean and standard deviation to the mean, thus the CV of the temperature/oxygen can be partly explained from its vertical (and horizontal) gradient. My question is whether relatively high CV is

due to strong vertical (or horizontal) gradient of temperature and oxygen (thermocline and oxycline depths). Also, how the structures are different from spring (D100-DM-Spr) vs. fall (DM100-DM-Fall)? It would also be relevant to high turbidity condition around 300 m as the internal waves/internal tides break and enhance the mixing (to resuspend the sediment) when and where the isopycnals (isotherms) touch the bottom (see the comments #6 below for details).

6) As described in Abstract, the high-frequency oxygen variability was strongly linked to tidal processes. But, I do not understand why it is contrary to expectation. As described in Section 1 (Lines 54-57), Section 3 (Lines 308-313), Section 4 (Lines 449-450 and 479-480), and Supplements, diurnal and semidiurnal oxygen variability is noticeable. This is not something unexpected but consistent with previous works reporting oxygen variability in a shallower zone, e.g., Frieder et al. (2012). Importance of tidal processes may also be confirmed from spring-neap cycles or modulations of semidiurnal/diurnal oxygen fluctuations. I could see such a spring (neap) amplification (reduction), for example, from time series plot of D10 - 98 m or D100-DM-Spr in Supplement 1B. Amplitudes of semidiurnal oxygen fluctuations reach up to larger than 20 μmol kg-1 for Days 0-3 and 10-13 (presumably corresponding to spring tide) while smaller than 10 μmol kg-1 for Days 5-8 and 17-20 (presumably corresponding to neap tide). What are CVs for periods of spring vs neap tides?

I believed and continue to believe that such high-frequency oxygen variability is relevant to internal tides generated and shoaled at a specific phase of the surface tide in a sloping bottom (even up to the zone as shallow as 15 m) as reported in the region by Nam and Send (2011) and others. It is generally known that the isotherms (so iso-oxygen surfaces) move up and down at high-frequency due to propagation and evolution internal tides and associated shorter period nonlinear internal waves (also termed internal solitary waves). When they shoal and break, turbulent mixing is markedly enhanced often forming bottom nepheloid layer that may account for suspended sediments and the high turbidity condition around 300 m. The bottom nepheloid layer has been presented since McPhee-Shaw (2006; “Boundary-interior exchange: Reviewing the idea that internal-wave mixing enhances lateral dispersal near continental margins” published in Deep Sea Research II: Topical studies in Oceanography as of 20 February 2006), e.g., Boegman and Stastna (2019; “Sediment resuspension and transport by internal solitary waves” published in Annual Reviews of Fluid Mechanics as of 15 August 2018).

7) Not being a biologist, I do not know in detail how the seafloor communities respond to short-period (mostly diurnal) changes in environmental conditions, but it is convincing that longer time series data are vital for addressing the science issue. My question is why camera sample should be less frequent for longer-term deployment. Is it limited by battery or memory? There would be several technical ways to overcome battery or memory limit. Why not trying new technologies that allow longer-term deployment keeping the same camera (as well as other sensors) sampling frequency.

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