

Interactive comment on “An Evaluation of the Performance of Sea-Bird Scientific’s Autonomous SeaFET™: Considerations for the Broader Oceanographic Community” by Cale A. Miller et al.

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Major comments: 1) Assess uncertainty if following today’s existing SOP: While this study clearly shows the utility of single- and multi-point calibrations in these three coastal systems, it would be useful to discuss the uncertainty observed when following the existing SeaFET standard operating procedure (SOP). I believe the SOP described by Bresnahan et al. 2014 is to use the factory calibration, but correct the dataset to some independent measure of “true” pH (e.g., discrete bottle samples, pH derived from other biogeochemical sensors combined with locally-constrained carbon system algorithm or TA-S relationship) once the SeaFET has conditioned to the environment in

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which it is deployed. This, along with an explanation of why the Miller et al. results differ from Bresnahan et al., would be a useful analysis for the existing users of SeaFETs.

Response

Specific to Bresnahan et al. (2014), a single-point in situ calibration was performed after the sensor was conditioned. This is different than the factory calibration performed by Sea-Bird. Factory calibration by Sea-Bird is conducted in a highly controlled tank setting void of natural conditions.

We agree with the reviewer that the current SOP described by Bresnahan et al. (2014) is slightly unclear as multiple cross-reference methods are proposed when determining SeaFETTM uncertainty (e.g., discrete bottle samples, pH estimated from O₂ or PCO₂). Thus, uncertainty will vary depending on validating source. We recognize the reviewer's comment regarding the utility of further comparing our uncertainty with that of Bresnahan et al. (2014), and have amended the manuscript to reflect this. See lines 677 - 687 in revised manuscript. Further, we refer the reviewer to lines 618- 636 which detail our assessment as to why some of our findings differ from what is currently in the primary literature.

We believe one utility of this manuscript is adding clarity to some of the ambiguity in terms and methods used in the primary literature regarding SeaFETTM operation and, thus, thank the reviewer for pointing out how we can improve by providing direct comparisons.

2) Characterize environmental variability: While the authors include a thorough explanation of how they minimized the impact of time/space sampling mismatch between the SeaFETs and the various independent validation measurements, it would be useful to develop an estimate of the environmental variability within these time/space constraints. This should be subtracted from (or considered somehow in) the sensor uncertainty estimates. An example of this type of assessment of how environmental variability impacts sensor field evaluations is summarized in the following and its associated

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ACT pCO₂ sensor reports: Tamburri et al., 2011: Alliance for Coastal Technologies: Advancing Moored pCO₂ Instruments in Coastal Waters. Marine Technology Society Journal, 45, 43-51.

Response

We agree with the reviewer that providing these estimates is beneficial. We have amended the manuscript to incorporate how rapid changes in salinity and temperature can affect pH measurements. However, this was done based on our data, and we caution operators that our deployment sites are not representative of different regions.

In addition, we chose to forgo adding an additional figure or amending figure 10 because environmental variability is specific to an operator's location and not specifically an intrinsic uncertainty with the SeaFETTM. For this reason, we feel a detailed description of environmental variability in the text (see lines 638 -654) responding to the potential uncertainties that could arise from the effects of rapid environmental variability are sufficient.

Minor comments/edits: Line 124: Elaborate; what does non-controlled source water conditions mean?

Response

We have elaborated this line to be clearer. Non-controlled source water refers to non-manipulated seawater.

Line 437: State the impact of 0.21 C discrepancy on pH.

Response

This line has been amended to express the uncertainty from this temperature discrepancy.

Lines 472-474: Improved accuracy for the unconditioned vs conditioned calibration based on what? The inter-sensor anomaly seems to be less in Fig 5 (conditioned)

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compared to Fig 4 (unconditioned), which is also shown in Fig 6.

Response

We agree with the reviewer that this may have been a bit unclear. We have amended the manuscript (see lines 481 - 482) to state the accuracy improved relative to the discrete reference samples. The reviewer is correct, the inter-sensor variability is less when calibrated and compared after the sensors were conditioned. When we discuss accuracy, we are specifically referring to sensor pH vs. discrete reference samples. We have provided a table based on reviewer # 2's comments to make clear the terminology used throughout the manuscript is consistent and clear.

Lines 680-683: While spectral analysis is a powerful tool for identifying drivers that are periodic or regular in nature, it will not characterize many phenomenon in the coastal environment such as storms or biological productivity/respiration. These types of events may impact the range over which a multi-point calibration should be made. This caveat should be included when suggesting spectral analysis as a tool for developing a multi-point calibration scheme in an environment with stochastic events.

Response

We agree with the reviewer that spectral analysis is while useful in some systems may not be as insightful in indicating the main drivers of pH. However, if no observable pattern is distinguishable using spectral analysis, this in itself, will help indicate which calibration method is appropriate (e.g., in situ single-point or multi-point). We have amended the manuscript to reflect this comment according to the reviewer's concern. Please see lines 733 – 738 in the revised manuscript.

Line 687: Define M2.

Response

We have removed the M2 term and simply stated "mixed semi-diurnal."

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Figure 3: The temperature difference here could be misleading to the reader. It is important to be transparent by stating in the caption that the SeaFET was not fully submerged in the tank, making it susceptible to air temperature fluctuations unlike the BoL, which was measuring only tank water temperature.

Response

We appreciate this comment, and have amended the figure caption to make clear that the top portion of the sensor may have been exposed to air temperature fluctuations.

Please also note the supplement to this comment:

<https://www.ocean-sci-discuss.net/os-2018-52/os-2018-52-AC3-supplement.pdf>

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2018-52>, 2018.

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