Reviewer #2:

This manuscript provides a very thorough technical description of the data collected by the DOE buoy lidars off the California coast. It will serve as an important reference for anyone using both past and future data sets collected by these instruments. It also provides some basic analysis of the observations, including an evaluation of M-O similarity theory, which is interesting. Other than some minor comments below, the paper is ready for publication.

We thank the reviewer for carefully reading the article and providing constructive and positive feedback. We believe the quality of the article has improved by addressing the reviewer's comments. Below the reviewer comments are in black and the authors responses are in blue.

Line 23. What does "statistically averaged data" mean? Is "statistically" necessary?

We agree that statistically is unnecessary and have removed it in the updated manuscript.

Lines 41-42: The use of "1-year" and "annual" in this sentence seem redundant.

Thanks for catching the redundancy. We have fixed that in the updated manuscript to: "So far, to the best of the authors' knowledge, there have been no wind observations collected over an annual cycle within the air-sea transition zone (ASTZ, encompassing the upper oceanic boundary layer and lower marine atmospheric boundary layer, Clayson et al., 2023) off the coast of California."

Line 48: Do the publicly available NYSERDA data pre-date the DOE buoy lidar data?

The NYSERDA buoy data were collected off the U.S. East Coast from approximately 2019 to 2023 and DOE buoy lidar data shown in this article was collected off the U.S. West Coast. So, yes, purely from a timeline perspective, the NYSERDA buoys pre-date the DOE buoy lidar data along the coast of California. Although, the DOE buoys were deployed back in 2015 – 2017 along the U.S. East coast, for more details please see here: https://doi.org/10.2172/1632348.

Lines 223-226. Given all of the problems with the Windcube IMU described here, I don't see the point of even showing any of the wind data derived using that IMU. Unless perhaps the authors are implying that data from all Windcube lidars would have these same problems. But if this is a one-off bad instrument, it doesn't make sense to me to show the data (e.g. figures 14, 15, 16). At a minimum, the authors should state their reason for showing the data if it is from a broken instrument.

The intent was mainly to show that when advanced users download the raw Windcube data, they need to be aware of this issue and we would prefer if they use the *.b0 post-processed data. We ofcourse don't know if this IMU issue is just with the two Windcube's on our buoy or all Windcube's V2.0 but we do know that Vaisala has stopped integrating the IMU in their upgraded version of Windcube's. But we agree with the reviewer that showing faulty data does not provide any scientific value, so we have updated this section currently and mentioned about the IMU issue in the text of the updated manuscript and changed the figures accordingly. We believe this has improved the focus of the article.

Lines 351-354: “During the Morro Bay deployment, the maximum and average Hmax/Hs were 2.5 and 1.6, respectively, when including questionable data, and were 2.2 and 1.6 when considering good data only. Based on theory, the expected values are 1.7 and 1.6 when including questionable data, and 1.7 and 1.6 when considering only good data. This indicates that the data follows the expected theory.” I’m not sure I follow this. The difference between 1.7 and 2.2 seems substantial (a 30% difference), so how does this indicate that the data follows expected theory. How far off would it need to be to be considered not to follow theory?

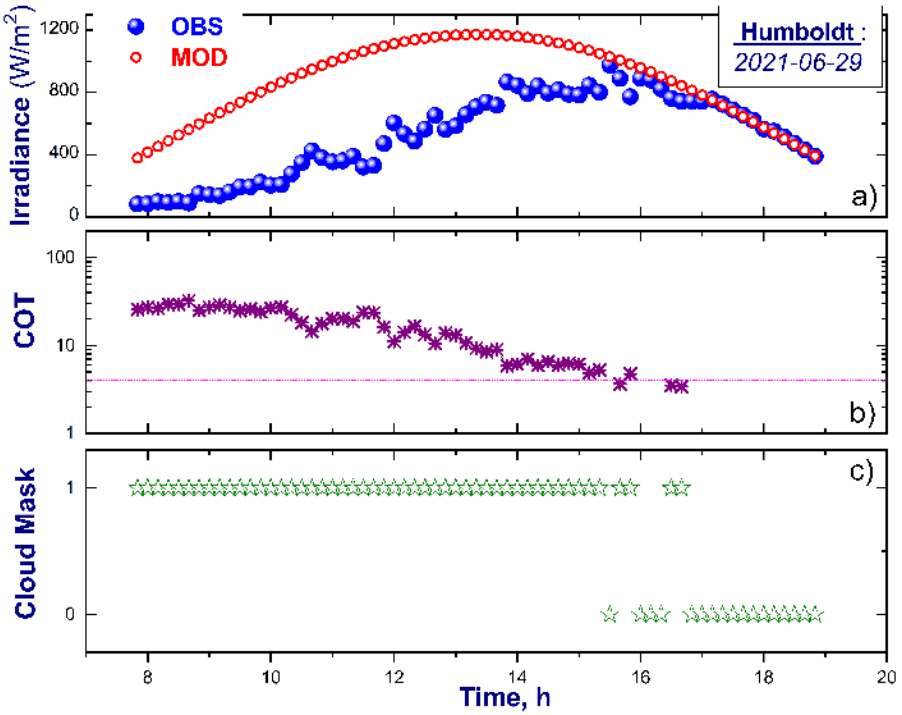
Thank you for bringing our attention to this section. We have further clarified the methodology in the text as some things were not explained in detail. The average $H_{max} \,/\, H_s$ indeed follows the expected Rayleigh distribution which is 1.6. We have clarified in the text that we refer to the average when we refer to the expected theory. We did not make the assessment only looking at the maximum as they can be outliers or represent errors in the measurements. The text manuscript now reads:

This indicates that on average the data follows the expected distribution.

We now turn our attention to wave heights that exceed the expected $H_{max} \,/\, H_s$. There have been verified wave measurements in which the maximum wave exceeds the significant wave height by a factor larger than expected by the Rayleigh distribution. When they do by a factor of 2 (or 2.2 depending on the author) or more they are referred to in the literature as rogue waves. We do not have a way to directly verify the maximum wave height measured in the record. Thus, we decided to use the ratio of 2 (instead of 1.7) to flag waves as suspect. We presented 1.7 as another possible cutoff but is more restrictive than what has been used in other applications.

Figure 9b. The y-axis label says COD, should it be COT?

The typo has been corrected. Please see the updated figure below.



Line 662. “Obukhov length”

The typo has been corrected.