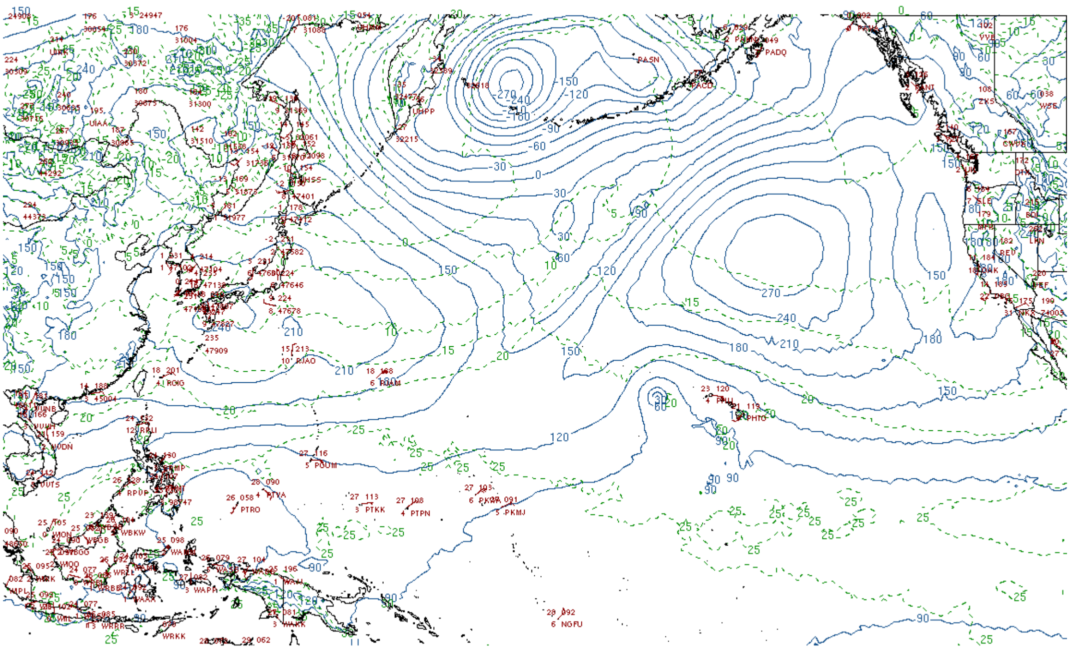
This is a well written and important paper that evaluates the statistics of near-surface wind at an offshore site off Hawaii relative to lidar buoy measurement of the vertical wind profile. Given the sparsity of offshore wind observations, this is an important study, even though it is constrained to just a single observation site. Moreover, the location sits in the unique, persistent tradewind environment, so is remarkably undisturbed by topographic or other continental features. I do however think that there are a number of aspects of the meteorology of the region that have been overlooked, and addressing them would add to the usefulness of this paper. These aspects are described below, followed by a series of minor comments.

1. Interannual variability

Given the position of Hawaii in the central Pacific Ocean and the fact that most of the study is based on a single year of data, it seems remiss to ignore the interannual variability. 2023 was an El Nino year, and was reported to have high rainfall across the pacific (eg https://www.drought.gov/sites/default/files/2023-06/Pacific%20Spring%202023.pdf). Moreover, there is a strong influence of ENSO on the strength and extent of the tradewinds. This means that when comparing the 2023 dataset to the 2000-2019 and 2008-2017 NOW-23 and GWA3 datasets, there will be a particular bias resulting from the number cases of each ENSO phase. I understand that the dataset of 2023 observations and the high-resolution WRF simulation are limited, but I think some analysis of the impact of the interannual variability on this site would be helpful in establishing whether 2023 was a ‘unusual’ year. This could be done using the ERA5 dataset over a longer time period. A further suggestion would be to try restricting the 2000-2019 and 2008-2017 datasets to just the El Nino years, to give a more realistic base for comparing the 2023 data to.

2. Extreme wind influences on the annual cycle

In addition to the systematic interannual variability signal, there are likely some extreme events that contributed to the variability of the 2023 dataset. For example, there were two Kona low events reported in February 2023, which may go a long way to explaining the spike in wind speeds seen in figure 5. Eg see https://www.weather.gov/hfo/KonaLowFeb2023 and the associated MSLP map for 17 Feb 2023 (below).. This is actually quite a fascinating situation, and assessing whether the reanalysis products were able to accurately capture the strong wind associated with these events is an important diagnostic in itself. It doesn’t really make sense to talk about the “steep observed monthly wind increase of 5.66 m/s between January and February 2023” (line 268) as a statistical feature when there was a distinct and unusual weather event causing this difference. Similarly, it doesn’t make sense to compare the 2023 monthly averages with the GWA3 and NOW-23 datasets over different time periods, when what we’re seeing is less a systematic difference and more the influence of a single weather event. Some relevant points that could be address here are: How unusual were the 2023 February Kona wind events, relative to the time periods in the other datasets? How do the datasets compare if you restrict the analysis to days that just had a ‘classic’ tradewind regime?



00Z 17 Feb 2023 1000 hPa University of Wyoming

https://weather.uwyo.edu/cgi-bin/uamap?REGION=npac&OUTPUT=gif&TYPE=obs&TYPE=an&LEVEL=1000&date=2023-02-17&hour=0

3. Local aspects
A great strength of this study is the fact that the wind is almost undisturbed tradewind flow. However, there may be some local influences relating to the effects of the islands. This could be especially relevant to the diurnal cycle in figure 13, and potentially particularly in the rare cases when the wind is blowing from the ESE or SSW. The lidar observations have a larger-amplitude diurnal cycle than either of the reanalysis datasets studied, but is this due to the different prevalence of wind directions in the different datasets? I suggest splitting the diurnal cycle up into different wind direction bins, although I know it will be difficult to get statistical significance for the rarer directions. A relevant reference for interaction of local phenomena with the tradewind environment is Dao et al. (2025):

Dao, T.L., Vincent, C.L., Huang, Y., Peatman, S.C., Soderholm, J.S., Birch, C.E., et al. (2025) Joint modulation of coastal rainfall in Northeast Australia by local and large-scale forcings. *Quarterly Journal of the Royal Meteorological Society*, e70027. Available from: **https://doi.org/10.1002/qj.70027**

4. Treatment of the short dataset
As already mentioned before, the treatment of the short dataset is not quite convincing. The authors should use the ERA5 dataset to establish the links between the other datasets that all cover different periods: Eg. a plot of the monthly wind averages from ERA5 for the periods 1950-present, 2000-2019, 2008-2018 and 2023 will help tie everything together, and clarify the representivity of the different time periods. This may indicate some further filtering as mentioned above, such as restricting the analysis to El Nino years.

Specific Comments
Lines 57-80. Could this information be presented in a more integrated way? Listing them one by one is difficult to read and doesn't leave any overall impression of the issues. Perhaps group by dataset tyoe: "Several studies have evaluated ERA5 at offshore sites, and noted a low wind speed bias (ref ref). etc". I guess the key message is that there has only been a small number of studies so far, the results are inconsistent across the studies, which is what makes this current study important.

Line 92-92 – Define YSU and MYNN acronyms

Line 104 – is **metocean** actually a word?

Line 136 – "off of Oahu" -> "off Oahu"

Figure 2 – Where does the 4 metre observation come from?

Line 177 – 'EMCWF' -> 'ECMWF'

Line 191 – Presumably the 1.5 km simulation didn't use a cumulus parametrisation?

Table 1: The GWA3 dataset should mention the microscale downscaling, not just the mesoscale modelling part

Equations 1-3. Are these standard equations really necessary? Perhaps check with the editors.

Figure 3: It would be better to make the time period for these plots the same (1 Jan 2023 – 15 December 2023).

Figure 4 and accompanying text: Define that you're talking about the median bias, not the mean bias.

Line 284 – 'gently' -> 'smoothly'

Figure 5 – perhaps a bar chart would be more appropriate, since this is not relaly continuous data.

Line 293. Clarify what these correlations are. Are they between the 24 values of the average diurnal cycle for each dataset?

Figure 7 – I think it would be clearer to just show the observed wind rose.

Figure 8 – It might be better to calculate the Richardson number or other stability metric, rather than just the air-sea temperature difference.

Section 4.1. I'm really not sure that it's reasonable to talk about bias here, given the different time periods. See general comments. Use the same time period for ERA5, NOW23 and GWA3, and then also do it for just 2023 for ERA5.

Figure 11 – What do the box plots show? Is it all values in the 2000-2019 period, or the distribution of the 20 annual averages during that time?

Figure 13 - Why were the ERA5 and WRF datasets not included in the diurnal cycle study?