Response to Reviewer #1

By Katherine L. Ackerman, Alison D. Nugent, and Chung Taing

The manuscript "Mechanisms controlling giant sea salt aerosol size distributions along a tropical orographic coastline" presents measurements of sea salt aerosols, including giant and ultragiant particles, from a novel device both on the coast and in the open ocean. Notable differences in the overall SSA concentrations are noted between the two sites, as are differences in the SSA-SD shape parameters and vertical profiles. Some of the mechanisms are illustrated by trajectory tracking in a WRF run of the area.

In my opinion, this is a great study and a very well-written manuscript. It is well-referenced and clear, and a lot of details were attended to properly. It also provides very reliable measurements of a data-scarce topic: SSA and giant particles in particular, but also comparing coastal to open ocean conditions. A lot remains to be understood about how universal the SSA-SD shape parameters are, and this study does the field a service by so clearly demonstrating some of the key differences that can occur. I only have a few very minor points for the authors to consider, but otherwise think that this would make a great contribution to ACP.

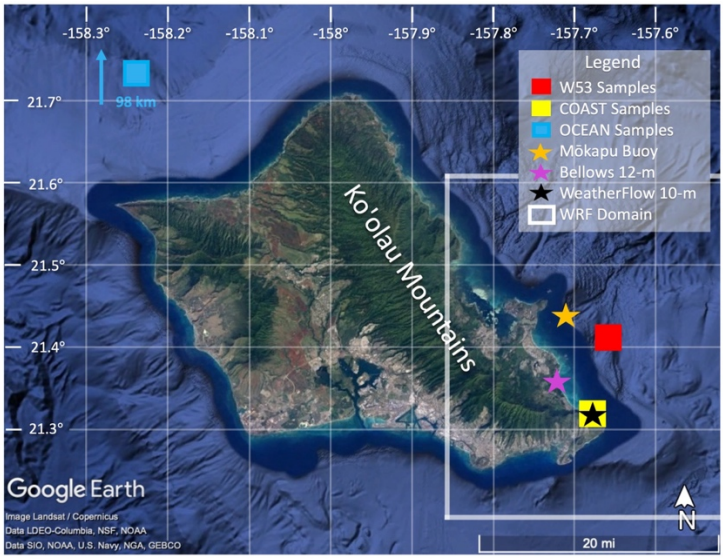
We would like to thank the reviewer very much for their thorough review of our paper! Thier recommendations have helped to clarify key details in the study and strengthen the major points we were hoping to emphasize. We appreciate the time and effort put in and are grateful to have received constructive and encouraging comments. Our responses to the specific comments are in detail below:

1. Line 9: Maybe should be "facilitating"?
   Thank you very much! It has changed from facilitate to facilitating.

2. Sentence starting on line 45: Maybe consider "whitecap fraction" explicitly as one of the things that has been analyzed a lot? It seems to fit with the way the literature has been broken up here – production has been linked with those things listed (U10, SST, SSS) but also whitecap fraction.
   We agree – this sentence was broken up and restructured for clarity to indicate that U10 is typically used to directly and indirectly represent SSA production (by increasing whitecap fraction). The paragraph now reads:

   "To characterize these whitecap interactions, in-situ, remote sensing, and laboratory experiments analyzed how environmental parameters such as 10 meter wind speeds (U10) contribute to the production of SSA, either as a direct mechanism for generating spume-sized SSP or as an indirect mechanism by increasing the whitecap fraction (De Leeuw 1986; Monahan et al. 1986; Andreas 1998; Gong 2003; Lewis and Schwartz 2004; Clarke et al. 2006; Petelski and Piskozub 2006; Andreas et al. 2008; Norris et al. 2008). Other studies have built on these findings by including additional ocean surface characteristics like sea surface temperature (SST) (Mårtensson et al. 2003; Jaeglé et al. 2011; Zinke et al. 2022), and sea surface salinity (Sofiev et al. 2011; Zinke et al. 2022)."

3. Figure 2: It might be nice to have a legend in the figure itself, explaining the symbols (so the reader doesn't have to sift through the caption)
   We agree, we've added a legend to the top right corner.



4. Line 177: I'm not sure what the "Bellows' 12 m" wind speed is here. I searched around the manuscript but didn't see any description of what this meant. So I'm left a little uncertain of why it was regressed to the WeatherFlow, and which one was ultimately used for the reported U10 values
   Bellows was indeed not previously defined, the sentences in these paragraph now read as:

   "A linear regression on almost two years' worth of data was conducted between a 12 m tall anemometer approximately 7 km northwest of Kaupō Bay (Bellows' 12 m) and WeatherFlow's 10 m anemometer for trade wind days (wind directions between 30-90 degrees). The Pearson's r between these two locations was 0.904 and significantly better correlated than our in-situ 2 m Kestrel samples to Bellows' 12 m wind speeds (Pearson's r = 0.681). Therefore, a relationship between the WeatherFlow 10 m wind data and the Bellows' 12 m wind data was derived to approximate historical wind speeds for the SSA sampling dates."

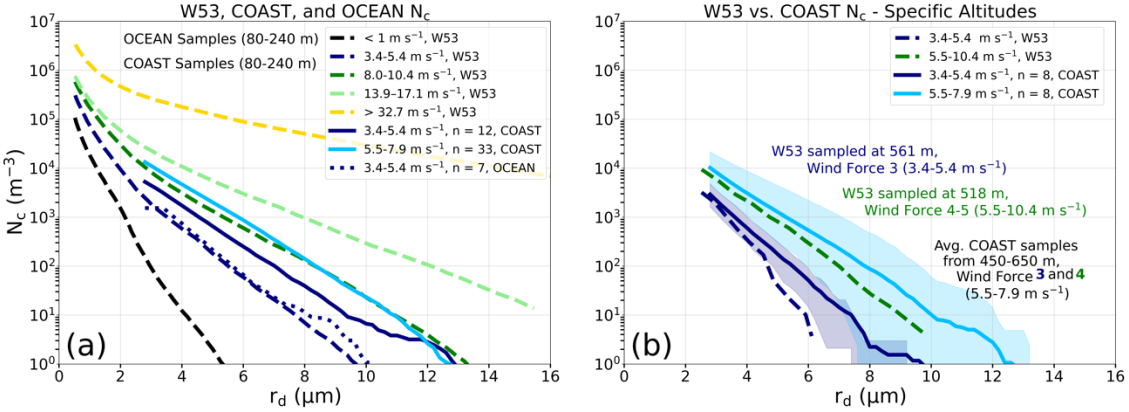
5. Line 206: What is meant by "the Shin and Hong PBL scheme for the grid resolution"? Is the phrase "for the grid resolution" supposed to be there?
   Thank you for catching this! "For the grid resolution" was removed for clarity, and PBL was expanded to say planetary boundary layer.

6. Line 231: Are these using the averaged wind profiles? Or the time-varying profiles from some set frequency of WRF output? It appears to be the average, so it's probably worth stating explicitly here.
   Thank you for asking this clarification question – yes, this is the 3-hour averaged wind

profile around 2 pm (the average sampling time across our studies) for both the low and high wind days. While we could have had time-varying profiles for the trajectories, we wanted these trajectories to represent an average circumstance for particle paths in high wind vs. low wind circumstances. We've updated this section so that our intentions are clearer to the reader:

"Lastly, trajectories for GSSPs sized $r_{d2.8}$, $r_{d7.4}$, and $r_{d12.2}$ were simulated using the 3-hourly averaged WRF wind profiles around the average time for our in-situ samples (2 pm local time) as well as the calculated fall velocities for both wind days."

7. Line 245: This isn't necessarily a suggestion for this study, but in the future it might be more realistic to use the PBL scheme parameters to approximate this variability.
   This is an excellent suggestion for the future! We'd love to explore how these trajectories change under more realistic conditions once we gather a larger collection of samples.
8. Line 267-269: If I'm reading figure 6 correctly, this sentence is really only referring to figure 6b (at specific heights), right? The COAST samples in 6a don't always exceed W53 at the wind speeds mentioned in this sentence. However I might be misunderstanding because the next paragraph explicitly introduces fig 6b.



We have updated this figure to be more colorblind friendly, as well as with additional annotations to better highlight some of the subtle differences. At first it might appear that only the smaller wind range COAST Nc (the solid navy line) exceeds the W53 sample (dashed navy line). We have highlighted the larger differences between the COAST and W53 Ncs for the next wind range in figure 6a. For this next range, the COAST Ncs are from winds between 5.5-7.9 m s^{-1}, while the W53 Ncs are from wind speeds from the range 8.0-10.4 m s^{-1}. So while they appear to be extremely close, the W53 samples are from a higher wind speed range, and we therefore assume that if our wind speed range had been within this range our COAST Ncs would exceed the W53 Ncs. We realize, though, that the green dashed line is used twice between Figure 6a and 6b for W53 samples even though the wind speed range differs between these two figures.

9. Line 314: "chances" should be "changes".
   Thank you for catching this error, this has been corrected.

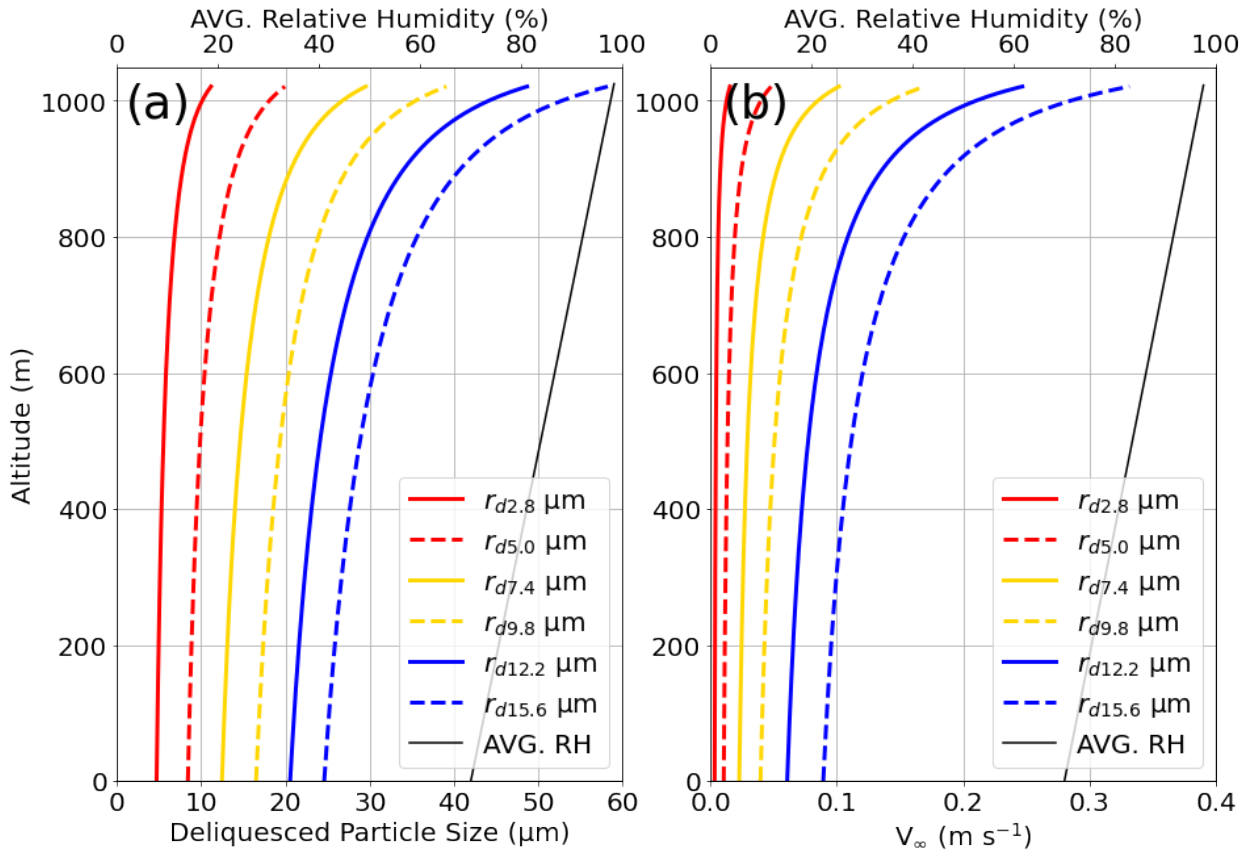
10. Line 315: “strong” should be “stronger”.

Thank you for clarifying this sentence - “remains strong” was changed to “becomes stronger”.

11. Line 351: I’m not sure the units are needed after “concentrations”

These units were removed as they were explained in previous sections of the paper.

12: Figure 4: It would be helpful if this figure had a panel “c” which showed the average RH profile that these are based on.



Thank you for this suggestion! To save space, we added the average RH profile for the August sampling date to both a and b plots. Now, readers can compare how relative humidity affects the deliquesced particle size of the different dry radius particles, and consequently the fall velocities.

13: Line 410 (or thereabouts): Somewhere at the end of this section I think it’s important to point out the relatively simple way of doing the trajectory simulations. I think it’s totally appropriate for what the authors are trying to do, but the treatment of turbulence in particular is only very crudely represented. I think the authors do a good job of only drawing conclusions which are supported by this method – I just think that a brief 1-sentence reminder at the end of the processes (especially turbulence) which were left out and could lead to significant differences in some of the details.

Thank you very much for this suggestion! We definitely want to emphasize that these results do not definitively represent all coastal processes, but merely help to demonstrate how production distance could play a role in changing the concentrations of different sized sea salt particles at different altitudes. To make this clearer, the following two sentences have been amended to say:

"These simulations offer a simplified representation of coastal orographic processes and further investigation into the roles of turbulence and coastal controls on dynamics should be completed, but overall, they demonstrate the potential impact that distance plays in controlling coastal SSA-SD shape parameters. Future studies will likely require a more robust set of observations and modeling capabilities to improve our understanding of coastal dynamics on SSA-SDs."

14: Lines 435-436: Elsewhere it's spelled "Hawai'i"

Thank you for spotting this! This has been corrected.