Reviewer 2 Response

We thank the reviewer for their time and their responses to our manuscript. Our replies are inserted below into the reviewer's comments

The paper demonstrates the use of random forest regression (RSR) to estimate wind speed profiles based on satellite 10m winds and ERA5 temperature data (SST and 2m). The work is novel and the comparison is quite extensive comparing with several independent sources. My main concern is that the authors compare their results to a neutral log profile and a power law profile with a fixed exponent. The neutral log law is used as the authors state that there is insufficient information to make the stability correction using a diabatic profile. This seems somewhat surprising. They are using ERA5 as an input to their RSR model which already uses delta T. This could equally be used along with wind speed to infer a bulk Richardson number which could be related to Obukhov length (L) and a formulation of the psi(z/L) function such as that by Businger and Dyer used to make the stability correction to the neutral log law. Indeed other parameters from ERA5 (e.g. sensible heat flux) could be used to get a more accurate estimate of surface stability. This would seem to be a much fairer comparison than using the neutral log law.

Thank you for the comments. In response to the concern about our use of the neutral log law, we would have preferred to use stability corrections. However, we are only comparing our RFR with the log law at the locations of the lidar buoys. At these stations, we do not make any use of ERA5 for SST since the buoys already provide in situ measurements of SST and air temperature (and thereby ΔT). Furthermore, ERA5's 0.25° grid is not always co-located with the buoy locations and ERA5 has lower temporal resolution (1 hour) than the lidar buoys (10 minute resolution). Our use of ERA5 for ΔT only comes in as an input into the RFR model when it is applied to extrapolate the Blended Seawinds 10 m winds speed to full profiles. The Blended Seawinds product is on the same 0.25° grid as ERA5.

Furthermore, it would be interesting to see a more detailed assessment of the accuracy of the RSR/log law/power law profile as a function of direction to see if the coastal transition plays a role in model accuracy.

We agree that this would be an interesting analysis. In this initial analysis we just focused on speed, however we could look into direction as well in future work.

Finally, the results of the performance metrics for the floating lidar sites would be more readable as a table as was done for the ASOW sites.

We agree that our initial discussion of the performance metrics for the float lidar sites was a bit dense. As part of a response to another reviewer, we revised sections 5.1 and 5.2 to not include as many values of the metrics. The values for these metrics are still shown on the figures in those sections. We believe this should resolve the concerns noted here.

Minor comments:

- The value of z0 for the log law extrapolation does not seem to have been mentioned (unless I miss it somewhere). Or was a Charnock relationship used?

The value of z0 used is 0.0001 (as in Optis et al., 2021). We also considered 0.0002 which has been assumed in offshore environments, but found 0.0001 to be more appropriate. This value has been added to the paper (Line 180) as the reviewer is correct that it was missing.

- Units are missing in Table 4.

  Thank you for finding this. Units have been added to the caption of table 4.

- On page 8, the word 'importances' is used several times. This sounds odd and I suggest that 'importance' is used as a collective noun.

  We agree with the reviewer that “importance” sounds better than “importances” and have made the changes necessary.

- Line 522: change 'shown decrease' to 'show a decrease'

  The phrase has been corrected.