Responses to RC2:

1. Original Submission

1.1. Recommendation

Major revision

2. Comments to Author:

Overall opinion: Most comments I raised in the first iteration were addressed. However, the language (especially) in the abstract must be improved. The current narration suffers from unclarity, vagueness and is sometimes terminologically puzzling not because the reported phenomena are complex, but because the language is confusing. Besides that, I am concerned that the authors did not understand my remark on how important to understand the behavior of surface-contained bin while using Aeolus data and simply stated it is out of their scope by explaining what previous studies on that topic did from their point of view. I explain below why this might be a major pitfall especially given the fact that the authors registered enhanced backscatter in the lowest layer, e.g. marine boundary layer with the highest chance to hit the surface return. Except that, the results are convincing enough and confidently pave the way toward acceptance.

AR: Thanks for your comments. We have tried to improve the language and grammar throughout the manuscript according to your suggestion. In the aspect of the behavior of surface-contained bin while using Aeolus data, we supplemented the relevant explanation that "during the data processing, it was discovered that all data (Level 2A particle optical properties, Level 2C wind vectors) below 0.25 km, which could be contaminated by reflections from the land or ocean surface, were all screened out using Aeolus quality control flags, then the lowest data bins became at around 0.25 km. This may indicate that the actual altitude range of marine aerosol optical properties in $Layer_1$ is around 0.25 km to 1 km.

Although the data near the sea-air interface are missing, all available data avoids the contamination of the ground return signals and eliminates the risk of being affected by ocean surface dynamical conditions." in the revised manuscript. The point-to-point response are shown as below.

2.1. Major comments:

1. Title. Overall, the title is good, but it is beneficial for you to speak not about correlation but about physical link or association between marine aerosol optical properties and wind fields. This link is revealed then by correlation as a tool, nothing else. But it's up to you of course.

AR: Thanks for the advice. To better present the physical causal relationship between wind speed and marine aerosol optical properties, we intend to modify the title as "Effect of wind speed on marine aerosol optical properties over remote oceans with use of spaceborne lidar observations."

2. Abstract: From my point of view, it must be improved. It's very "wordy" ('Furthermore, thank to…" introductory structures are obviously unnecessary; the last sentence is just incomprehensible). It contains unclear terminology like "aerosol related atmospheric background states". Sentences like the one below also convey very simple ideas are structurally barely unreadable ("The marine aerosol extinction/backscatter coefficients and the background wind speeds show positive relationships and they were fitted by power law functions, of which the corresponding 2 R are all higher than 0.9"). Make abstract concise, clear, quantitative and free of vague terms that even atmospheric scientists will not comprehend. The current setbacks are not acceptable for the abstract of high-impact factor study.

AR: Thanks for the advice. We have reviewed the abstract. Removed, revised or rephrased the sentences that you pointed out. And we also made necessary changes to improve its clarity and coherence. The revised abstract is shown as below:

"In this paper, using Aeolus data, the relationships between the marine aerosol optical properties at 355 nm and the corresponding instantaneous co-located wind speeds of three remote ocean areas are investigated and analysed at two separate vertical atmospheric layers (0-1 km and 1-2 km, corresponding to the heights within and above marine atmospheric boundary layer (MABL)), revealing the effect of wind speed on marine aerosol over the remote ocean. Marine aerosol extinction/backscatter coefficients and background wind speeds show positive relationships. Their correlations are modeled using power-law functions, with corresponding R^2 values all greater than 0.9. Both the MABL and the layer above it receive the marine aerosol produced and transported by the wind from the sea-air interface. The marine aerosol load in the lower layer (MABL) is stronger than in the higher layer. The intensity of marine aerosol extinction/backscatter coefficients enhancements caused by the background wind is greater in the MABL. The slope variation points occur during the marine aerosol extinction/backscatter coefficients increasing with wind speed. Above these points, the growth rate decreases. This may indicate that the wind-driven enhancement of marine aerosol involves two phases: a rapid growth phase with high wind dependence, followed by a slower growth phase after the slope variation points. The correlation between the marine aerosol optical depth at 355 nm and the corresponding wind speeds is established, and verified by comparing it with CALIPSO-derived results from previous research. The variation of the marine aerosol lidar ratio at 355 nm with wind speed is also examined, suggesting a possible increasing-decreasing-increasing trend of marine aerosol particle size as wind speed increases."

3. Language: Besides the remark on the abstract, the language suffers from unclarity and vagueness in some other parts of the manuscript. For instance, it is stated that "marine aerosol is mainly produced by wind" without mentioning sea/ocean surface. Grammatically, there is nothing wrong with this formulation, but technically, it's a misleading remark. There are more cases like that in the text I highlight in minor comments. Please thoroughly check the style and logic of your text even if grammar is ok. Ask English speaking colleague or language check service to help if needed; you will save your time by this.

AR: Thank you for your careful review. The sentence "Marine aerosol is mainly produced by wind" has been deleted in the revised manuscript. And we have modified all the cases you highlighted in minor comments. Besides, we also reviewed and checked the whole manuscript for the grammar and the logic.

4. Methodology:

• "it should be emphasized that the lowest altitude bins of Aeolus Level 2A and Level 2B products could be contaminated by reflections from the land or ocean surface, and are thus not representative for the atmospheric wind speed and the aerosol optical properties (Wu et al., 2022)." This made me thinking about methodological pitfalls of your study. Can you give me please the definition of atmospheric bin that does not contain surface from your methodological point of view? Do you have any risk to include this unaccounted bin into your MABL? Because the lowest lidar bin of Aeolus does not necessarily contain surface, this setting is changing, your lowest lidar bin can in some cases be way above 0 meters for instance.

AR: As you mentioned, the altitude of the Aeolus vertical bins could change along the track. Therefore, the altitudes of the lowest bins could be above or below the surface.

The altitude information of each bin is all provided in the Aeolus L2A/L2C products. In our methodological point of view, we used the altitude information of the bins to select data rather than

their order in the profiles. Specifically, for $Layer_L$, we selected data bins with an altitude range of 0 km to 1 km, while for $Layer_H$, we selected bins with an altitude range of 1 km to 2 km. Furthermore, after quality control with Aeolus quality control flags, there is no data available below 0.25 km. Therefore, it is considered that there is no risk in including surface bin into MABL.

In addition, the sentence "It is important to note that the lowest altitude bins of Aeolus observation products may contain the reflections from the land or ocean surface, thus they are contaminated and not representative of the atmospheric wind speed and the aerosol optical properties (Wu et al., 2022)." has been moved to Line 242 in the revised manuscript.

• My concern is confirmed by your answer on the comment by the way; you mention 0 km here ("Therefore, it is considered that the statistical results of the 0-1 km layers and the 1-2 km layers are capable to generally represent the atmospheric conditions within the MABL and above the MABL").

AR: Yes. As we described in the previous answer, we selected data for the two layers (0 km-1 km, 1 km-2 km) based on the altitude of each bin. This ensures that the statistical results of these data could represent the state within above the MABL.

• This further statement goes in conflict ("in L Layer , the lowest Aeolus Level 2A products (particle optical properties) data bins with the altitude of lower than about 0.25 km, are absent to avoid the ground return signals' contamination.") with the statement above on the MABL range starting from 0 km. So you consider 0-1 km layers or 0.25 – 1.00 km layers, be precise with your methodological remarks.

AR: Thanks for your kind reminder. The actual meaning of this sentence was that in the data processing procedure it was found that after quality control all data below 0.25 km were screened out, then the lowest data bins were at around 0.25 km. To make it clear, this sentence has been modified as "during the data processing, it was discovered that all data (Level 2A particle optical properties, Level 2C wind vectors) below 0.25 km, which could be contaminated by reflections from the land or ocean surface, were all screened out using Aeolus quality control flags, then the lowest data bins became at around 0.25 km. This may indicate that the actual altitude range of marine aerosol optical properties in $Layer_L$ is around 0.25 km to 1 km. Although the data near the sea-air interface are missing, all available data avoids the contamination of the ground return signals and eliminates the risk of being affected by ocean surface dynamical conditions."

5. Conclusions: I'll go through conclusions here.

• Line 553 It is logical to say by using particle optical properties (Level 2A product -> in brackets), not the other way around because L2A is the Aeolus-specific term here. I already raised a concern that such terms are unknown for general readers.

AR: Thanks. The sentence has been revised as "By utilizing particle optical properties data (Level 2A products) and wind vector data (Level 2C products) provided by ALADIN, and L2 Vertical Feature Mask (VFM) products provided by CALIOP, the optical properties at 355 nm of pure marine aerosol are derived."

• Line 555 "Then" is not needed here, there is no temporal order or sequence here.

AR: Thanks, removed.

• Lines 553 – 559. Boldly emphasize what is your research aim, it's very vague now and blurred among all these activities that comprise your study.

AR: Thanks for the advice. To make it clearer, this paragraph has been revised as "By utilizing particle optical properties data (Level 2A products) and wind vector data (Level 2C products) provided by

ALADIN, and L2 vertical feature mask (VFM) products provided by CALIOP, the optical properties at 355 nm of pure marine aerosol are derived. The correlation between marine aerosol optical properties at 355 nm and the instantaneous co-located wind speed over remote ocean areas is investigated and discussed at two separate vertical atmospheric layers ($\mathrm{Layer_L}$ with the height of 0-1 km and $\mathrm{Layer_H}$ with the height of 1-2 km, corresponding to the heights within and above marine atmospheric boundary layer (MABL)), revealing the effect of wind speed on marine aerosol within and above the MABL over the remote oceans."

• Line 569 Which statistical results, just name parameters or metrics directly

AR: Thanks. The sentence has been revised as "The correlation between the marine aerosol optical properties (extinction coefficient (α_{mar}) and backscatter coefficient (β_{mar})) at 355 nm and the wind speed (ws) are analysed at $\mathrm{Layer_L}$ and $\mathrm{Layer_H}$, for three study areas respectively." in the revised manuscript.

• Line 572 The fact that MABL can receive marine aerosol is trivial finding. Perhaps, Aeolus can detect it is a thing to report?

AR: Thanks for the advice. This sentence has been revised as "It is found that the Aeolus observations can provide evidence of the fact that the MABL receives the marine aerosol produced and transported from the air-sea interface. Furthermore, the observations suggest that even the layer above the MABL may also receive the marine aerosol input." in the revised manuscript.

• Line 573 Once again, enhancement in the lowest MABL layer is suspected to be linked to inclusion of surface bin. Let me know why I am wrong here please.

AR: After using the quality control flag provided by Aeolus, it was found that there is no available data left below 0.25 km. Therefore, it is considered that no data bin could be linked to the inclusion of surface bin.

To illustrate this issue, the description has been added in Line 250 of the "Methodology" section, as "during the data processing, it was discovered that all data (Level 2A particle optical properties, Level 2C wind vectors) below 0.25 km, which could be contaminated by reflections from the land or ocean surface, were all screened out using Aeolus quality control flags, then the lowest data bins became at around 0.25 km. This may indicate that the actual altitude range of marine aerosol optical properties in $\mathrm{Layer_L}$ is around 0.25 km to 1 km. Although the data near the sea-air interface are missing, all available data avoids the contamination of the ground return signals and eliminates the risk of being affected by ocean surface dynamical conditions."

The phenomenon that "The marine aerosol enhancements caused by the background wind are more intensive at the MABL." may result from that the MABL is closer to the sea-air interface so it will receive more effects. The explanation for this phenomenon has been added after this sentence in the revised manuscript, as "This may be due to the MABL's proximity to the sea-air interface, making it more susceptible to such effects."

• Line 575. Unsupported surmise about "might illustrate", give facts or rationale behind please.

AR: We think the phenomenon that "the slope variation points (15 $\mathrm{m \cdot s^{-1}}$ for α_{mar} and 10 $\mathrm{m \cdot s^{-1}}$ for β_{mar}) were found during α_{mar} and β_{mar} increasing with wind speed, above which the growth

rates become lower" illustrates the statement that "the enhancement of marine aerosol driven by wind includes two phases, among them one is a rapid growth phase with high dependency of wind, and another is a slower growth phase with higher fluctuations after the slope variation points".

We calculated the averaged slopes and the corresponding standard deviations of α_{mar} and β_{mar} of the two growth phases (below and above the slope variation points) and have supplemented these results in Table 2 of the revised manuscript, as the argument of this statement. Table 2 and the relevant descriptions in the revised manuscript are shown as below:

"We named these two wind speed (15 $\mathrm{m \cdot s^{-1}}$ for α_{mar}, 10 $\mathrm{m \cdot s^{-1}}$ for β_{mar}) "slope variation point" in this paper. Table 2 presents the averaged slopes (Mean) and the corresponding standard deviations (SD) of α_{mar} and β_{mar} below and above the slope variation point, for the two layers of the SP and SI areas. All of the averaged slopes below the slope variation points are larger than those above the slope variation points, except for the α_{mar} in the SI area. The reason for the inverse results of α_{mar} in the SI area may be due to its rapid increase above 24 $\mathrm{m \cdot s^{-1}}$. All of the SDs of β_{mar} above the slope variation points are greater than those below, indicating a more fluctuating growth phase above the slope variation points. These results could provide the evidence for the statement that the wind-driven enhancement of marine aerosol includes two phases: one is a rapid growth phase with high dependency of wind, and another is a slower growth phase with higher fluctuations.

Table 2: Mean ± SD of the slopes below and above the slope variation point, grouped by areas and layers.

Optical property	Area	Layer	Mean ± SD of the slopes [$\mathrm{Mm^{-1} \cdot (m \cdot s^{-1})^{-1}}$ for α_{mar}, $\mathrm{Mm^{-1} \cdot sr^{-1} \cdot (m \cdot s^{-1})^{-1}}$ for β_{mar}]	
			Below slope variation point	Above slope variation point
α_{mar}	SP	H	2.48 ± 1.81	1.79 ± 5.71
		L	3.11 ± 4.62	1.26 ± 16.11
	SI	H	1.96 ± 3.10	2.81 ± 12.59
		L	2.16 ± 4.28	3.28 ± 8.79
β_{mar}	SP	H	0.20 ± 0.17	0.07 ± 0.17
		L	0.28 ± 0.11	0.12 ± 0.29
	SI	H	0.21 ± 0.16	0.09 ± 0.20
		L	0.22 ± 0.16	0.12 ± 0.13

"

• Line 576… Why LRmar results are omitted from abstract? If they are not relevant, then there is no

sense to put them into conclusions as well. Please harmonize conclusions and abstract by content.

AR: Thanks. We have revised a more detailed description of marine aerosol lidar ratio in the abstract, as "The variation of the marine aerosol lidar ratio at 355 nm with wind speed is discussed, suggesting a possible increasing-decreasing-increasing trend of marine aerosol particle size as wind speed increases."

• Line 589 What do you consider as "total consistency" from statistical point of view. I'm hinting here – give numbers and rely on them while making such statements.

AR: Actually, what we meant here is that the regression curves are different because the regression equations of three study areas are not the same. To reduce the misleading, the phrase "not totally consistent" was revised as "inconsistent".

• Line 590 Conditions cannot participate anywhere, please rephrase and check the language throughout the article.

AR: The sentence has been revised as "while the meteorological and environmental parameters, apart from wind, differ across various regions." in the revised manuscript.

• Line 594 – 595 Implication sentence is not giving any new insights. If more data are available, more information can be derived. This has been known and is clear without this study and should be therefore removed.

AR: Thanks, removed.

• I feel that the sentence which can explain what your study added to this research field is missing. Let me elaborate on what your study added to this research field from my point of view. See below quick speculation based on your results that you may or may not use as your implications if you'd like to.

i. Perhaps, you deepened our understanding of relationship between marine aerosol optical properties and wind in remote ocean areas using unique Aeolus observations given its truly unique setup and ability to deliver winds?

ii. What I also think is nice that you demonstrated that the relationship between aerosol optical properties and wind speed are MORE COMPLEX than we might have expected if you need so many more parameters to scrutinize it in different regions?

iii. Moreover, this is an important lesson for missions similar to Aeolus like Aeolus-2? Where a synergy of aerosol and wind observations gives unique and proven ability to quantify aerosol-wind speed interactions in poorly observed regions of ocean? You showed basically very useful application of Aeolus synergistic analysis

AR: Thanks for your suggestions. We found all these three comments to be very helpful so, we incorporated them into the final paragraph of the revised manuscript. The revised final paragraph is shown as below:

"This study demonstrates Aeolus' ability to quantify interactions between aerosols and wind speeds in poorly observed ocean regions through a synergy of aerosol and wind observations. The α_{mar} - ws, β_{mar} - ws models within and above MABL at remote ocean areas were established with Aeolus provided data. These models deepens our understanding of the correlation between marine aerosol optical properties and wind in remote ocean areas across two vertical layers based on the unique setup and the ability to deliver winds of Aeolus observations. Nevertheless, the regression curves of α_{mar} - ws and β_{mar} - ws above three study areas (the NP area, located in the Pacific Ocean, the low

latitudes of the Northern Hemisphere; the SP area, located in the Pacific Ocean, the middle latitudes of the Southern Hemisphere; the SI area, located in the Indian Ocean, the middle latitudes of the Southern Hemisphere) are inconsistent, while the meteorological and environmental parameters, apart from wind, differ across various regions. It implies that the relationships between marine aerosol optical properties and wind speed are more complex than a linear or exponential relation. In order to obtain more precise α_{mar} and β_{mar} models, besides wind speed, other meteorological and environmental factors, e.g., atmospheric stability, sea and air temperature, RH, etc. should participate in the establishment of the models, because the production, entrainment, transport and removal of the marine aerosol above the ocean are not only dominated by the wind, but also be impacted by these factors (Lewis and Schwartz, 2004)."

6. Previous comments from my side:

• Third, you discuss marine aerosol optical properties – near wind relationship from lidar perspective, but omitted a large corpus of works dedicated to this issue. I think you misunderstood my point on this initial comment here. You used a backscatter as a source of information about atmosphere using Aeolus over ocean, but the problem is that previous lidar studies have demonstrated that attenuated particle backscatter can be driven by the change of surface conditions of the ocean. And then, Aeolus has very coarse vertical resolution which means that the lowest bin contain surface. On top of that, you normally do not know where this surface bin is located because range gate of Aeolus setup was spatio-temporally changing. Most studies I mentioned are centered around one physical principle – atmospheric backscatter derived from lidar over ocean surface can be not affected, but is normally literally driven by the ocean surface conditions, not only atmospheric condition. You can learn a lot from this experience and be therefore more aware about these pitfalls. From statistical viewpoint, you may have been encountering adverse physical confounder in your analysis that you could not refute methodologically. You assumed in your answer that it's not the scope of your interest, but this assumption does not refute the fact that lidar returns from Aeolus could be affected by this ocean surface-related phenomena, thus inflicting unknown bias in your results about atmosphere. Your abstract immediately makes this assumption about possibility to study wind-aerosol relationship over ocean without considering surface contamination and range gate variations (lines 22-23 about "different vertical layers"). The wind-ocean surface return aspect of this issue does not seem to be critical in the current iteration though because in the mean-time, Aeolus-related work about surface returns showed that there is no wind-backscattering sensitivity at the lowest bin of Aeolus for oceans (highly non nadir incidence, weakening the returns, 355 nm – transparent ocean surface for beam heading to nadir). For experts with similar concerns to mine, this works makes it easier you can just instead add some sentence like that for clarity in your methodology. "On one hand, it is known that for ocean applications, lidar attenuated backscatter can be affected by the processes at the surface of ocean, namely, stronger winds, weaker backscattering [Josset et al. 2008; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2008GL033442]. On the other hand, the recent study on Aeolus surface returns [Labzovskii et al., 2023; https://www.nature.com/articles/s41598-023-44525-5] indicated that Aeolus returns are unlikely sensitive to ocean surface dynamics (related to wind), which makes the analysis of AOD/marine boundary layer conditions/*any atmospheric phenomenon* free from adverse effects, stemming from ocean surface." Rephrase it the way you want, though if you got my idea here.

AR: Thanks for the careful and patient explanation. We thought we got your idea about the relationship

between the attenuated backscatter coefficients of the bins containing ocean surface and the ocean surface dynamical conditions. We have incorporated your comments into the methodology section of the revised manuscript. The supplemented descriptions are shown as below:

"It is important to note that the lowest altitude bins of Aeolus observation products may contain the reflections from the surface or even be subsurface, thus they are contaminated and not representative of the atmospheric wind speed and the aerosol optical properties (Wu et al., 2022). Regarding the ocean applications of spaceborne lidars observations, it is known that the lidar attenuated backscatter coefficients of the bin containing the ocean surface can be affected by the processes at the surface of ocean, namely, stronger winds resulting in weaker backscattering (Josset et al., 2008). Labzovskii et al. (2023) indicated that Aeolus return signals are unlikely sensitive to ocean surface dynamical conditions (related to wind), which makes the analysis of marine aerosol optical properties in the MABL free from adverse effects stemming from ocean surface. Nevertheless, during the data processing, it was discovered that all data (Level 2A particle optical properties, Level 2C wind vectors) below 0.25 km, which could be contaminated by reflections from the land or ocean surface, were all screened out using Aeolus quality control flags, then the lowest data bins became at around 0.25 km. This may indicate that the actual altitude range of marine aerosol optical properties in $\mathrm{Layer}_{\mathrm{L}}$ is around 0.25 km to 1 km. Although the data near the sea-air interface are missing, all available data avoids the contamination of the ground return signals and eliminates the risk of being affected by ocean surface dynamical conditions."

• Comment about Hoaglin et al. 1986. Your said that this approach of eliminating outliers… that "is a widely used approach in data analysis, especially in the statistical analysis". Most statistical approaches are tools to be used and they do not fit for every situation where we are having "big data" problem. You are working with physical observations. While exploratory data analysis can be a helpful approach in identifying outliers in certain types of data, it may not be suitable for physical observations where factors such as distribution, range, and scientific context need to be taken into consideration. Please give references of applications of this method to geophysical/atmospheric data or justify the choice.

• Same comment: You gave a very simple explanation like "Before the elimination, the outliers of extinction coefficients and

backscatter coefficients can catch up to 1000 1 Mm − and 30 1 1 Mm sr − − while generally the particulate extinction coefficients and backscatter coefficients are within 300 1 Mm− and 101 1 Mm sr − − ". But if this explanation is true the simplest confidence interval approach/z-score, any more simple statistical way to find outliers, would work for this purpose, no?

AR: Sorry for the misleading that the name of the outlier labeling and elimination method we used is "boxplot analysis", not Tukey's test. Below are some references that use the same method to label and eliminate outliers in the field of atmospheric research.

- In the "Chapter 3 - Empirical Distributions and Exploratory Data Analysis" (Page 23-70) of the volume "Statistical Methods in the Atmospheric Sciences" (Volume 100) of the book series "International Geophysics", Wilks (2011) introduced the boxplot analysis for outliers labeling and elimination in atmospheric scientific analysis.
- van Zoest et al. (2018) used the boxplot analysis to identify and label the outliers in a full year's urban air quality sensor observations divided into 16 spatio-temporal classes.
- Guevara et al. (2021) used boxplot analysis to help establish the quality assurance flags of the NO2 observational dataset in the research of "Time-resolved emission reductions for atmospheric

chemistry modelling in Europe during the COVID-19 lockdowns".

- Santhana Lakshmi and Vijaya (2023) also applied the boxplot analysis to identify the outlier present in the dateset, which are air pollution parameters including PM2.5, PM10, CO, SO2, ozone, NOX, and NH3.

So we consider that this method is applicable to atmospheric data, including particle optical properties provided by Aeolus.

References:

Guevara, M., Jorba, O., Soret, A., Petetin, H., Bowdalo, D., Serradell, K., Tena, C., Denier van der Gon, H., Kuenen, J., Peuch, V.-H., and Pérez García-Pando, C.: Time-resolved emission reductions for atmospheric chemistry modelling in Europe during the COVID-19 lockdowns, Atmos. Chem. Phys., 21, 773–797, https://doi.org/10.5194/acp-21-773-2021, 2021.

Santhana Lakshmi, V., Vijaya, M.S. (2023). An Exploratory Data Analysis on Air Quality Data of Trivandrum. In: Joshi, A., Mahmud, M., Ragel, R.G. (eds) Information and Communication Technology for Competitive Strategies (ICTCS 2022). ICTCS 2022. Lecture Notes in Networks and Systems, vol 623. Springer, Singapore. https://doi.org/10.1007/978-981-19-9638-2_68.

van Zoest, V.M., Stein, A. & Hoek, G. Outlier Detection in Urban Air Quality Sensor Networks. Water Air Soil Pollut 229, 111 (2018). https://doi.org/10.1007/s11270-018-3756-7.

Wilks, D. S., Chapter 3 - Empirical Distributions and Exploratory Data Analysis, International Geophysics, Academic Press, 100, 23-70, https://doi.org/10.1016/B978-0-12-385022-5.00003-8, 2011.

2.2. Minor comments (p-ll, where P – page, LL – line).

• Line 1 Marine aerosol is produced by wind, but what about role of ocean in this production? I once again refer to this language-related vagueness in your descriptions here.

AR: Thanks. This sentence has been removed in the revised manuscript.

• Line 20 "Aeolus, the worldwide first ever wind detection lidar satellite", it has sense to use more conventional terms for explaining what Aeolus is. See abstract here for good example (https://amt.copernicus.org/articles/14/6305/2021/)

AR: Thanks. This sentence has been removed in the revised manuscript.

• Line 25 Do you really provide a discussion or also analysis? Readers may think that you speculate on existing findings if you put it this way, but you did way more beyond discussion.

AR: Thanks. We have replaced the word "discussed" with "analysed" in the revised manuscript.

• Line 30 – 31 "The marine aerosol load at the lower layer (MABL) is stronger than at the higher layer." Here I refer to the comment on the methodological choices. If by some chance you included surface bin into your assumed MABL bins by not tracking the actual surface bin/bins of Aeolus, your backscattering in this bin would be higher than in the bin above, but due to other reason – surface return backscatter (nothing to do with aerosol backscatter).

AR: As we stated in the previous replies, we used the altitude information of the bins provided in the Aeolus products to select data. Specifically, for Layer_L, we selected data bins with an altitude range of 0 km to 1 km, while for Layer_H, we selected bins with an altitude range of 1 km to 2 km. Furthermore, after quality control with Aeolus quality control flags, there is no data available below 0.25 km. Therefore, it is considered that there is no risk in including surface bin into MABL.

• Line 32 "The gradient change points of marine aerosol extinction/backscatter coefficients appear during the growth of them with wind speed, above which the growth rate becomes lower." I try to understand it, but I don't. Gradient change points? I already raised the concern about this term (Line 440 of original submission). This is very counterintuitive term. Think about that. Gradient change can

happen temporally and spatially (both vertical and horizontal direction). And then "growth of them with wind" -> Very bad wording for English. Rephrase please, it's completely unclear.

AR: Thanks for the comments. We have replaced "gradient change point" with "slope variation point" throughout the manuscript.This sentence has been revised as "The slope variation points occur during the marine aerosol extinction/backscatter coefficients increasing with wind speed."

• Lines 33 – 34. It might illustrate that it is also a statistical coincidence without further statistical arguments. How to prove your claim here or at least support it?

AR: We calculated the averaged slopes and the corresponding standard deviations of α_{mar} and β_{mar} of the two growth phases (below and above the slope variation points) and have supplemented these results in Table 2 of the revised manuscript, as the argument of this statement. Table 2 and the relevant descriptions in the revised manuscript are shown as below:

"We named these two wind speed (15 $m \cdot s^{-1}$ for α_{mar}, 10 $m \cdot s^{-1}$ for β_{mar}) "slope variation point" in this paper. Table 2 presents the averaged slopes (Mean) and the corresponding standard deviations (SD) of α_{mar} and β_{mar} below and above the slope variation point, for the two layers of the SP and SI areas. All of the averaged slopes below the slope variation points are larger than those above the slope variation points, except for the α_{mar} in the SI area. The reason for the inverse results of α_{mar} in the SI area may be due to its rapid increase above 24 $m \cdot s^{-1}$. All of the SDs of β_{mar} above the slope variation points are greater than those below, indicating a more fluctuating growth phase above the slope variation points. These results could provide the evidence for the statement that the wind-driven enhancement of marine aerosol includes two phases: one is a rapid growth phase with high dependency of wind, and another is a slower growth phase with higher fluctuations.

Table 2: Mean ± SD of the slopes below and above the slope variation point, grouped by areas and layers.

Optical property	Area	Layer	Mean ± SD of the slopes [$Mm^{-1} \cdot (m \cdot s^{-1})^{-1}$ for α_{mar}, $Mm^{-1} \cdot sr^{-1} \cdot (m \cdot s^{-1})^{-1}$ for β_{mar}]	
			Below slope variation point	Above slope variation point
α_{mar}	SP	H	2.48 ± 1.81	1.79 ± 5.71
		L	3.11 ± 4.62	1.26 ± 16.11
	SI	H	1.96 ± 3.10	2.81 ± 12.59
		L	2.16 ± 4.28	3.28 ± 8.79
β_{mar}	SP	H	0.20 ± 0.17	0.07 ± 0.17
		L	0.28 ± 0.11	0.12 ± 0.29

SI	H	0.21 ± 0.16	0.09 ± 0.20
	L	0.22 ± 0.16	0.12 ± 0.13

”

• Line 35 “As derived data from Aeolus…”, this sentence has no grammar and stylistic sense in the present form. Please rephrase the entire conclusive remark as well.

AR: Thanks. We rephrased the final sentence in the original manuscript as “The correlation between the marine aerosol optical depth at 355 nm and the corresponding wind speeds is established, and verified by comparing it with CALIPSO-derived results from previous research. The variation of the marine aerosol lidar ratio at 355 nm with wind speed is also examined, suggesting a possible increasing-decreasing-increasing trend of marine aerosol particle size as wind speed increases.” in the revised manuscript.

• Lines 107 – 109. As mentioned above, Labzovskii et al., 2023; [https://www.nature.com/articles/s41598-023-44525-5] demonstrated that surface-containing Aeolus bin (where range bin has intersection with digital elevation model) manifest surface reflectivity over land with very high agreement with Lambertian equivalent reflectance from passive instrument. Note that you cannot speculate on the lowest bin because lowest bin can be subsurface in case of water if you do not track what is the lowest bin of Aeolus using either DEM data or Aeolus information on surface location (vertically).

AR: Thanks for the reminder. We have rephrased this sentence as “It is important to note that the lowest altitude bins of Aeolus observation products may contain the reflections from the surface or even be subsurface, thus they are contaminated and not representative of the atmospheric wind speed and the aerosol optical properties (Wu et al., 2022).” and moved it to line 242 of the methodology section. We used the altitude information of the bins provided in the Aeolus products to select data. After quality control using Aeolus quality control flags, no data is available below 0.25 km. Therefore, it is considered that all data bins are above the ocean surface, avoiding the impact of surface reflectivity.

• Line 113 “As mentioned above, Aeolus can provide global high spatial and temporal resolution aerosol optical properties profiles and wind speed profiles despite the lack of the lowest bins close to the ground”. What is the lack of the lowest bins close to the ground? How do you judge abundance and lack of the bins quantitatively?

AR: We decided to remove “despite the lack of the lowest bins close to the ground” in the revised manuscript.

• Line 121 How did you quantify where is ocean surface? You make references to MABL, but it is unclear. The same applies to the orange block of Figure 2.

AR: We can quantify the 0 km altitude of Aeolus data bins using its altitude information. Therefore, we replaced “ocean surface” with “0 km” in the revised manuscript.

• Line 201 Not only contaminated, but also totally attenuated if your clouds are above studied aerosol layer, right?

AR: Yes. If the clouds above the studied aerosol layers are thick enough to attenuate all the light signals, then there will be no available data for the aerosol layers below. We revised this sentence as “The Aeolus products do not differentiate between aerosol and cloud layers, which means that the particle optical properties of a single data bin may contain a mixture of both types of information.”

Responses to RC4:

The manuscript is greatly improved after the revision. The authors well-addressed the reviewer's comments and suggestions. One additional comment is:

The authors claim that 'the atmosphere of the two vertical layers bill both receive the marine aerosol input produced and transported by the wind and the turbulence'. However, the decrease of lidar ratio with wind speed indicates that 'marine aerosol get larger'. Given that aerosol optical properties are impacted by both the concentration and particle size, it is unclear whether the increase of optical properties with wind speed is dominated by more aerosols or by the growth of aerosols. So I believe the authors should tune down a bit as the manuscript strongly indicates that the increase of optical properties is caused by more aerosol inputs without fully considering the impact of aerosol particle size.

AR: Thanks for your reminder. We added the comment that "As aerosol optical properties are affected by both particle concentration and size, this reminds us that the increase in α_{mar} and β_{mar} with wind speed may not only be due to the enhancement of particulate quantity produced from the sea-air interface, but may also be impacted by the size variation." in line 604 of the summary and conclusion section in the revised manuscript.