"Ship-based lidar measurements for validating ASCAT-derived and ERA5 offshore wind profiles"
Rev v1
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Authors response to reviewer comments

We would like to thank the referees for their time and effort in reviewing our work. We appreciate their feedback and comments, and we have carefully considered their recommendations and requests to improve and clarify our work.

Below, we addressed all the referees' comments and reply to them point by point. First, the referee's comment is included in italics and bold font, followed by our answer, and when applicable, the new excerpt of the revised version of the manuscript (highlighted in blue).

Anonymous Referee, Referee #1

Referee #1 general comments

1) **The mix of ASCAT and ERA5 and afterwards validating them against each other does not appear as a scientifically sound approach. ASCAT is assimilated into both NWP-models and ERA5. At the same time, sensible heat flux, 2m temperature and friction velocity from ERA5 is used in extrapolating ASCAT to 250m height and correcting for stability. Nowhere in the paper is this justified or discussed.**

The comparison between ASCAT extrapolated and ERA5 profiles is not intended as a validation against each other but rather as a means to explore similarities and differences in their retrieved values. This comparison pretends to provide some insights regarding how factors such as the temporal resolution and discretization of ASCAT, and the amount of data available may influence the final extrapolated values obtained using the statistical adaptation of MOST (Section 3.1). Additionally, this comparison aims to show the differences between these datasets along the region of study (due to factors such as coastal contamination) and before and after the application of the extrapolation methodology, i.e., at 10 m and 100 m height (Section 3.2). Finally, Section 3.3 presents the validation of extrapolated ASCAT and ERA5 values by comparing them against the reference lidar measurements. These clarifications have also been included in the manuscript for clarity (see Section 3).

It is worth noting that while ASCAT is assimilated in NWP models (and ERA5), there remains value in comparing these datasets due to their different characteristics (e.g. vertical, horizontal and temporal resolution) and the fact that the ASCAT retrievals are (direct) measurements, and not model data as ERA5, which results in different benefits and drawbacks, and therefore, different suitability for different applications. This might have been the motivation for the numerous scientific reports and peer-reviewed publications dedicated to this type of comparisons. E.g.: (Belmonte Rivas and Stoffelen 2019; Hauser et al. 2023; Duncan et al. 2019; Badger et al. 2016; Hasager et al. 2020; Hatfield et al. 2022; Takeyama et al. 2019; Takeyama et al. 2020). Some of them including the extrapolation approach also used in our study.

Finally, the utilization of the ERA5 parameters used for implementing the mean stability correction approach are listed in Section 2.4, together with the equations in which these parameters are employed for the calculation of the stability correction factor: "…However, since we wish to develop an extrapolation method independent from in situ measurements, the mean temperature and heat fluxes in Eq. (1) are replaced by the ERA5 parameters air temperature at 2 m and surface sensible heat flux, respectively. Additionally, friction velocity values from ERA5 are also utilized…".

2) ***The terms such as "ASCAT wind profiles" and "wind speed from ASCAT" when talking about wind in 100m are used in several places. Since satellites provide sea surface measurements, such terms should be defined early on (e.g. around line 60) with a short introduction to what it is.***

A clarification regarding ASCAT wind profiles has been added in Line 72: "…comparison of vertically extrapolated ASCAT wind profiles (hereafter referred to as ASCAT wind profiles) …".

Additionally, the employment of "ASCAT winds", "wind speed from ASCAT", and similar terminology have been reviewed throughout the manuscript, making necessary clarifications when referring to extrapolated or 10m measured ASCAT values.

3) ***How many collocations between ship lidar and ASCAT in the open sea are there actually?***

As detailed in section 2.5, it is important to note that there is not a one-to-one collocation of ASCAT and lidar measurements, since the extrapolation methodology used for ASCAT only provides mean wind profiles within a considered time period (in this study, the duration of the measurement campaign). Therefore, there is not a specific number of collocations.

However, we have addressed this concern by incorporating a new figure to the manuscript (Figure 6 in the new version of the paper), which provides an overview of the ASCAT grid boxes used in the comparisons, as well as the amount of lidar data utilized at each of these grid boxes.

4) **The discussion of MOST in section 205-215 is confusing. A clearer overview of validity and why this can not be used, while Kelly and Gryning is valid, is needed. Later (line 371) the overestimated wind profiles based on this method get this comment: "This is due to the fact that these heights are well beyond the range of applicability of the extrapolation methodology employed (Kelly and Gryning, 2010)."**

The section has been revised with the aim of enhancing clarity and readability, while also properly highlighting the differences between the instantaneous MOST methodology for extrapolation and statistical adaptation used in this study, along with the potential advantages offered by the former approach.

5) **After seeing the poorer results near land on both sides, it is strange that these data are included further in the analysis (page 14 and figure 11). The issue is not investigated to any degree, it is only written that it is due to a number of different effects such as land contamination in ASCAT (line 335, 348, 376), coarse resolution of**

ERA5 (line 340, 348, 378), and even wave breaking and surface slicks in the coastal zone (line 456)! But if there is land contamination in ASCAT (and it looks like it from figure 5), then the other effects are irrelevant because the measurements do not represent wind speed.

As seen in Figure 14 of the new version of the manuscript (and Figures 13 in the old version), while nearshore grid points present higher deviations respect to lidar measurements, this is not uniformly applicable to all nearshore locations. Both ERA5 and extrapolated ASCAT show a significant agreement (even higher than those points farther away from shore) at some points very near to the harbors. Therefore, we believe including these results is pertinent, as excluding points from the comparison solely based on distance to shore may introduce a bias in the analysis. However, and in order to clarify and properly discuss these effects more comprehensively, several amendments have been made to the text:

- Page 14 of previous version of the manuscript highlights 3 main factors for the differences between the collocated and full campaign approach at near shore locations. In the new version of the manuscript, this section has been revised and we have included a detailed description of how the coarse resolution of ERA5 can lead to a different mean stability at coastal sites, resulting in different stability correction factors for the collocated and full campaign and therefore different wind speeds at 100 m (See lines 330 - 353).
- In Section 3.2, a more detailed discussion regarding the differences between ASCAT and ERA5 at 10 m has been added, including further details between the agreement of the two datasets depending on the distance to shore and therefore, isolating the higher deviation of ERA5 and ASCAT in nearshore conditions (See lines 374 – 382).
- Fig. 11 has been modified to also separate the effect of nearshore grid points in the error distribution presented.
- The discussion has been also modified to cover this topic.

Furthermore, we want to clarify that “wave breaking and surface slicks” are referred as a potential cause of coastal contamination, as evidenced in previous literature, rather than as a reasoning to explain deviations between the datasets: “…satellite measurements proximate to shorelines are susceptible to coastal contamination, occasioned by different factors such as waves breaking and surface slicks.”

6) **Please revise the conclusions. At present the chapter does not summarize the main results and include statements which are not justified: “The long-term stability correction employed in this study demonstrated a strong performance for extrapolating ASCAT winds, yielding to a good agreement compared to the in situ measurements from the ship-based lidar measurements, despite the relatively constrained temporal window of the study.” And “ASCAT derived wind profiles are a valuable asset for portraying offshore wind conditions at turbine operation heights, manifesting a level of accuracy similar to numerical model outputs.”**

The conclusions of the manuscript have been revised.

Referee #1 specific comments

7) ***Line 37: Why is shallow and deep water mentioned here? What is the definition of shallow and how is it used later on?***

We refer to near-shore and far-off offshore regions. The terminology has been reviewed and adjusted throughout the whole document.

8) ***Line 42: "To overcome the limitations of in situ measurements and numerical models, satellite remote sensing devices have emerged as a potential alternative for characterizing ocean winds and climate over large areas, capturing the wind variability with a temporal coverage of over 15 years." It is strange to talk about a potential alternative when satellite remote sensing has been around for so long. What is meant by wind variability?***
The sentence may be misleading, and it has been revised to emphasize that satellite measurements are not a novel alternative, but they represent an additional option that can be considered when looking for diverse datasets applicable to offshore wind and climate studies.
"wind variability" refers to horizontal wind variability. This has been clarified in the manuscript.

9) ***Figure 2: what height are these wind speeds recorded in?***
The wind speed at 100 m height is presented. This has been indicated in the text referring to this figure as well as in its caption.

10) ***Lines 131-133: This statement sounds like a conclusion and does not belong in the data chapter.***
This sentence has been removed from the manuscript.

11) ***Lines 145: please revise this sentence as it is unclear: " … being zero when having completely smooth surfaces and simultaneously increasing with the roughness".***
Sentence adjusted for clarity.

12) ***Line 168: "By applying the IQR outlier detection, the impact of coastal contamination on the wind speed data is minimized, leading to more accurate and reliable results in nearshore areas." This statement sounds like a fact. Please modify and provide a reference.***
Sentence has been reformulated and a reference added as suggested.

13) ***Line 186: the title of section 2.4 should include "vertical extrapolation".***
Section 2.4 title adjusted to "Satellite vertical extrapolation".

14) ***Line 200: The objective is to validate this method, so naturally this method is used. However, it is not clear if it is because the authors expect that it provides better results than other methods. Why can single collocations of ERA5 and ASCAT not be used to extrapolate the values upwards? Or even the ERA5 wind profile?***

The decision to use the proposed methodology has considered few factors, rather than being solely based on the expectation of superior results compared to alternative methods. Firstly, the scarcity of available data due to the limited time extension of the measurement campaign, particularly given the coarse temporal resolution of ASCAT (about 2 measurements per day), which is not enough for the application of data-driven methodologies such as triple collocation or machine learning algorithms.

Additionally, the interest of the long-term correction approach comes from its potential better performance compared to the instantaneous correction approach. Previous studies showed that while numerical models can accurately capture average meteorological conditions over extended periods (Peña and Hahmann 2012), the accuracy of instantaneous stability information from these datasets is questionable, introducing additional uncertainty to extrapolated profiles using this instantaneous data (Badger et al. 2012). Another advantage of the long-term stability correction over the instantaneous correction is that we avoid calculating wind speed for conditions and heights outside the valid range of the MOST model. MOST is tailored to characterize turbulent fluxes within the surface boundary layer (Lange et al. 2004; Högström et al. 2006), but it has limitations when dealing with instantaneous data analysis, especially under stable conditions. The long-term adaptation of MOST, however, remains effective up to turbine operating heights, as it falls within the range where MOST is applicable.

In summary, this methodology was selected based on an evaluation of available data and the limitations of alternative approaches. Being our decision primarily driven by the pragmatic constraints of data availability and with the aim of evaluating a potentially better performing methodology for ASCAT extrapolation.

To clarify this in the manuscript, the corresponding section of the paper (Section 2.4 of the latest version) has been modified and extended for further clarification in this regard.

15) ***Line 241: how were the values for C+ and C- chosen?***
The selection of values for the constants C- and C+ was based on an empirical validation, by comparing the theoretical distribution calculated from Eq. (2) (and dependent on the selected C- and C+ values) against the normalized probability density (NPD) function of the inverse Obukhov length derived from ERA5. Through this process, values were chosen to ensure that the theoretical distribution closely matched the ERA5 NPD across all the ASCAT grid boxes along the entire ship route.

At the moment, and to the authors' knowledge, no other methodology apart from this empirical validation (e.g. (Kelly and Gryning 2010)) has been presented in previous literature focusing on the application of this mean stability correction.

The corresponding section of the manuscript (Lines 345-351 of the new version) has been modified to clarify this methodology for the definition of C- and C+ values.

16) ***Line 246: Remove long-term the second time in "Finally, the long-term stability correction of the mean long-term wind profile at a specific height z is calculated as:".***
Done.

17) ***Line 255: The "theoretical" distribution from eq 2 is also using ERA5 data, so is there much point in comparing for the "full campaign"? As expected they are quite similar in figure 4.***
As mentioned in the response to comment 15, the theoretical distribution derived from Eq. (2) depends on the values of C± utilized. Therefore, comparing this theoretical

distribution with the NPD from ERA5 provides insight into the suitability of the selected C± for accurately representing the atmospheric stability.

Below, we have included a comparison between the theoretical and ERA5 distributions for the same location used in Figure 4 of the preprint manuscript, also considering the data from the full campaign. However, the theoretical distribution presented below has been calculated using C± values of 12 and 5, respectively, as utilized in a previous study (Optis et al. 2021). As can be observed, even with the inclusion of ERA5 data from the full campaign, the theoretical distribution struggles to closely resemble the NPD of the 1/L parameter calculated by ERA5.

In summary, we included this comparison because it shows that the data used for the derivation of the theoretical distribution, as well as the model configuration (i.e. C+- values selected), correctly represents the atmospheric stability calculated from ERA5.

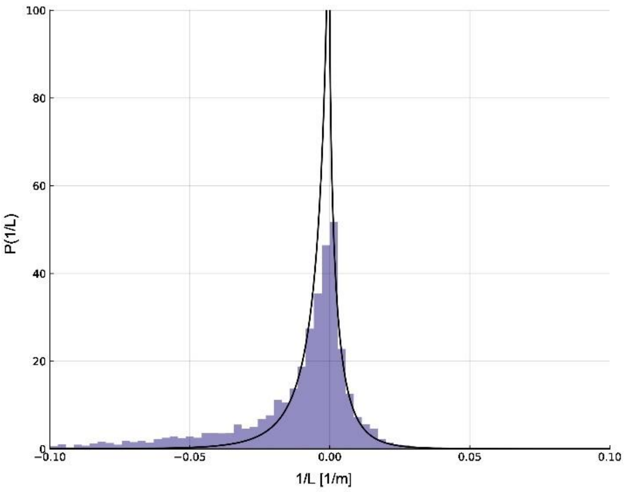

Figure 1: Normalized probability density functions of inverse Obukhov length 1/L from ERA5 and theoretical distributions calculated from Eq. (2) using 12 and 5 for C- and C+. The same offshore location as in Figure 4d and full campaign data was considered for this plot.

18) ***Figure 4: add definition of "full campaign" and "collocated" to the figure caption.***
This clarification has been added to the figure caption.

19) ***Line 286: Please comment on the fact that there is land in two of the grid boxes and argue why they can be included in the analysis.***
A comment regarding this fact has been added in lines 312-315 of the new version of the manuscript.

20) ***Figure 6: the labels A-F are not used anywhere else.***
They are now used when commenting results from Figure 8 and Figure 11, where the differentiation of these locations is used.

21) ***Line 300: "...two different approaches…" insert collocation: "...two different collocation approaches…".***

Done.

22) ***Figure 10b: Please include the pdf for values over the open sea only, so it is better documented that "... wind speed differences above this threshold correspond to those to near-shore grid points." (line 356).***
Figure has been modified.

23) ***Line 390: "...ERA5 appears to outperform ASCAT …": Why "appears"?***
Adjusted to "…ERA5 outperforms ASCAT…"

24) ***Figure 13: Please add a line to show the number of collocations along the track.***
A new Figure (Figure 6 in the new version of the manuscript) has been included with the amount of collocated lidar data along the ship route.

25) ***Page 20: Notice that there are some typos: highes lidat . And the use of "resemble" in line 404.***
Text on this page has been reviewed and typos corrected.

26) ***Lines 450-456: "The comparison between ASCAT and ERA5 winds revealed a good agreement between the two datasets.": Please be more specific. Was the agreement really good? If so, how about the mix of the two datasets as mentioned above? This long paragraph consists of hypotheses that are not investigated. Was it expected that ASCAT and ERA5 should perform well near the coast?***
This entire paragraph has been rewritten for better clarity and including a more specific discussion of the obtained results.

27) ***Line 469: "...ASCAT exhibited a closer similarity to the lidar wind profile than ERA5.": where? It seems to be a coincidence that the values are close for the mean wind profile in figure 11a since it is a mix of nearshore and offshore values.***
As pointed out by the reviewer, the better agreement of ASCAT overall profile is indeed a result of averaging profiles from both onshore and offshore locations. This aspect has been clarified in the manuscript.

28) ***Line 475: "... the notorious overestimation suffered by ASCAT is evident…": this doesn't fit with figure 13 where there are values below -2?***
This paragraph has been revised, and this particular assertion has been removed.

29) **Line 480: What is the value of employing ASCAT and the long-term height extrapolation using ERA5, versus just using ERA5?**
ASCAT are observations, and ERA5 is not, and only the stability information of ERA5 is assumed accurate to obtain ASCAT values at higher heights.

<u>Ine Wijnant, Referee #2</u>

Referee #2 general comments

1) ***My main problem with this paper is that your aim is to provide a means for offshore wind resource assessment which is an alternative for ERA5, but***
    a. ***You still need ERA5 to modify ASCAT***

    Yes indeed – however, it is important to clarify that ASCAT provides wind field **measurements**, and **only the stability information for ERA5** is required for this methodology.
    In addition, the non-stationarity of ship-based lidar measurements leads to a comparison not focused on a single location, but within an extensive region. Consequently, using stability information from stationary measurements is unfeasible, due to the lack of an extensive enough network of measuring devices. Therefore, using ERA5 (or other potentially applicable numerical models) is essential, and a requirement imposed by the nature of ship-based lidar measurements.

    b. ***You do not take into account wake and blockage effects. This is probably okay for now (there are no wind farms yet in the Baltic according to https://map.4coffshore.com/offshorewind/), but it will become a problem during the lifespan of the wind farm when more wind farms are built (the effect is already significant on the North Sea). ERA5 does not take these effects into account and ASCAT is too course to measure them (SAR can: WINS50 - Winds of the North Sea in 2050 | Publications I Wijnant, A Stepek (2023): Fit(ch) for shipping Wind farm wake effects at 10 m height, KNMI WINS50 Report.). The only way to predict how much power a wind farm will produce during its lifespan is to understand and properly model wind farm effects and then to use this knowledge/ these models for future wind farm scenarios. The old method for wind resource assessments (measure-correlate-predict) no longer works.***

    We acknowledge the referee's comment regarding wake effects. But as our study focuses on comparing ASCAT and ERA5 wind data in the Baltic Sea region, the issue of wake effects falls outside the scope of our investigation. While wake effects are indeed important considerations for wind farm development, they do not directly impact our findings, given the absence of nearby wind farms in our study area.
    Our paper does not aim to predict wind farm production, but to evaluate the performance of ASCAT and ERA5 data against reference lidar measurements, providing insights regarding their potential and limitations in characterizing offshore winds. We have tried to make clear statements about the goal of our paper in several parts of the manuscript:

    From the title: Ship-based lidar measurements for validating ASCAT-derived and ERA5 offshore wind profiles

From the abstract: …For this reason, this study presents a comprehensive comparison between wind profiles derived from the Advanced Scatterometer (ASCAT) satellite observations and the ERA5 reanalysis dataset against ship-based lidar measurements in the Northern Baltic Sea.…

From the introduction: The objective of this paper is to assess the accuracy of ASCAT-derived wind speed profiles in the nearshore and offshore locations of the Northern Baltic Sea by conducting a comprehensive comparison against ship-based lidar measurements. … To the authors' knowledge, this study represents the first comprehensive comparison of vertically extrapolated ASCAT wind profiles (hereafter referred to as ASCAT wind profiles) to wind turbine operational heights against non-stationary in situ measurements, covering a wide horizontal extent that extends from nearshore to offshore locations…

From the discussion: The objective of this study has been to evaluate the accuracy of ASCAT-derived wind speed profiles for the characterization of offshore winds at turbine operating heights in the Northern Baltic Sea… Subsequently, the analysis incorporated a comparison of both gridded datasets against in situ observations obtained from a novel ship-based lidar campaign.

From the conclusion: Satellite-borne scatterometers and numerical models are two potential alternatives for characterizing offshore winds. This study undertakes an intercomparison of these datasets and validates them against reference ship-based lidar measurements.

Finally, SAR measurements were contemplated as an alternative to ASCAT due to their higher resolution and potential better performance in near-shore areas. However, given that SAR's lower temporal resolution (one overpass every couple of days) and the relatively short period of the campaign, we opted for ASCAT in order to maximize the amount of collocated data and ensure the consistency of the statistical metrics evaluated. A clarification regarding this has been added to the discussion section of the manuscript:

… However, for this study, SAR measurements were not contemplated due to their lower temporal resolution compared to ASCAT and the relatively short duration of the campaign. This decision aimed to maximize the amount of collocated data and ensure the consistency of the statistical metrics evaluated.

c. ***Mean values of the wind are not relevant if you want to predict power: you need to look at correlation on a 10 min (or hourly) basis, especially for wind speeds between cut-in and rated (power curve).***

As mentioned above, our paper does not aim to assess or predict wind farm energy production, but to compare ASCAT (and ASCAT extrapolation) and ERA5, and then evaluate both of them against reference lidar measurements, providing insights regarding their potential and limitations in characterizing offshore winds. Additionally, we must note that mean values are not the only statistical measure used in this study. Please see the results section, where additional metrics (error distributions, bias, nRMSE, confidence intervals, or box plots, to mentions some) have been applied and analyzed.

2) ***The concept of doing lidar measurements on a ferry is interesting and probably even more interesting on the North Sea where you no doubt can measure wind farm effects this way. How to compare these measurements to ERA5 is as you describe not straightforward (ship-motion compensation algorithm), comparing them to ASCAT I presume even harder. You can only do that when ASCAT is available which is around 09 and 19 UTC when the ferry is in the harbour or reasonably close to the coast and you say that there the ASCAT-signal is disturbed? Are there other measurements available near the harbours (or in the Baltic Sea) that you could use to compare the lidar measurements to?***

   We appreciate the referee's insight into the challenges associated with comparing ship-based lidar measurements to ERA5 and ASCAT data. For this reason, Section 2.5 includes a detailed description of the collocation methodology used in this study, which aims to address factors such as the different temporal and spatial resolution of the different datasets and the non-stationary nature of the ship, thus providing a robust comparison methodology.

   In addition, to provide further information regarding the amount of collocated lidar-ASCAT data used in this comparison, we have included a new figure (Figure 6) in Section 2.5. This figure illustrates the number of lidar retrievals collocated at each ASCAT grid box along the ship route.

3) **I think that comparing ERA5 to 'ASCAT extrapolated to hub height with ERA5' is not very useful (and scientifically sound). What would be interesting is to do triple collocation with (1) ASCAT-winds extrapolated to hub height with your method (or different methods) based on ERA5, (2) ship based lidar measurements and (3) a mesoscale model in hindcast mode, for now without Wind Farm Parametrization (WRF? Unless you can get hold of COSMO-CLM or HARMONIE?). The aim of your paper then would be the best possible extrapolation of ASCAT to hub height. Why that is useful is something you will have to explain (not for wind resource assessments).**

   The application of triple collocation may require a high amount of data for a successful performance. However, due the relatively short measurements campaign and the rather low temporal resolution of ASCAT, the amount of data available in this study is not sufficient for the application of triple collocation. For this reason, and although the investigation of the potential application of triple collocation is an interesting point for future work, it is not in line with the scope of this paper. This has been indicated in the manuscript:
   Furthermore, and although previous literature highlighted the good performance of data-based extrapolation methods, the limited time extension of the measurement campaign results in an insufficient amount of data to implement these approaches in this study….

Referee #2 specific comments

4) ***Bias ERA5 at hub height 0.5 m/s is also what is found on the North Sea in Characterisation of offshore winds for energy applications — Research@WUR and at Cabauw in Energies | Free Full-Text | Dutch Offshore Wind Atlas Validation against Cabauw Meteomast Wind Measurements (mdpi.com). NEWA comparable to ERA5 (at least on the North Sea). Undisturbed winds in DOWA (2008-2018) and WINS50 (2019-2021) are much better than ERA5 (including correlation) and the domain covers most of the Baltic Sea, but hourly data unfortunately not available for 2022 and 2023 when you have the lidar measurements (Home | Dutch Offshore Wind Atlas; WINS50 - Winds of the North Sea in 2050).***
   The two additional references recommended by the referee have been added (Section 3.3). Regarding the additional measurement datasets suggested by the referee, as the referee rightly pointed out, they are not within the time frame of the ship-based lidar campaign and thus not applicable for this study.

5) ***Line 52: Unclear sentence: Lastly, the trustworthiness of satellite retrievals remains a knowledge gap, due to the lack of available in situ datasets for validation especially in deep water regions.***
   This sentence has been removed from the manuscript.

6) ***We know that the Dutch part of the North Sea (DEEZ) does not experience a trend in offshore wind speed, only an Interannual Variability (IAV) of 5 and 4% for sites in the northern part of the DEEZ and between 4 and 4.5% in the southern part of the DEEZ (Inter-annual wind speed variability on the North Sea | Report | KNMI Projects). Is any information like this available for the Baltic Sea? How representative is 28-6-22 until 21-2-23 for the wind climate in the Baltic Sea? This you can check e.g. with ERA5 data (compare ERA5 28-6-22 - 21-2-23 to ERA January1940-now).***
   Interannual variability has not been considered in this study since it does not have any influence on the study. The three datasets used comprise the same time frame of about 6 months, therefore no interannual changes are captured.
   To avoid confusions, some clarifications have been added:
   - In Section 2.2: The implemented ASCAT data processing for this study focused on satellite measurements retrieved during the period of the ship-based lidar measurement campaign, and includes …
   - In Section 2.3: It must me noted that only ERA5 data within the time frame of the measurement campaign have been used in this study.
   - Along the entire manuscript, we have adapted the terminology “long-term stability correction” to “mean stability correction”, to avoid potential confusions regarding the time period covered by the data used in this study.

7) ***Line 143: (fig 3) maybe I missed it, but what ASCAT data did you use?***
   Section 2.2 has been rearranged for better clarity on this. Now it is clear that the ASCAT data used was the corresponding to the product id WIND_GLO_WIND_L3_NRT_OBSERVATIONS_012_002, downloaded from the Copernicus Marine Data Service (CMS) and corresponding to the period of the measurement campaign.

8) ***Line 189: (typo): Several methodologies to vertical satellite extrapolation… not to, but for.***
   Corrected.

9) ***Line 201: This method involves a long-term correction of atmospheric stability effects, obtained from the numerical model dataset ERA5, along with an adaptation of the MOST to vertically extrapolate the satellite wind measurements. What is long-term about it? Why the name 'long-term extrapolation method'?***
   The term "long-term" was adopted from the previous literature employing the same methodology (e.g. (Optis et al. 2021; Badger et al. 2016; Hasager et al. 2020)). However, we recognize that this may be confusing for readers, since our study only applies this methodology for the time duration of the ship-lidar measurement campaign.
   To address this, we have revised the terminology "long-term stability correction" with a more appropriate term such as "mean stability correction".

10) ***Lines 203-205 not clear: do you mean that a wind profile can be stable up to a certain height and above unstable and that this 'long-term extrapolation method' can handle this?***
   No, we mean that the stability correction factor calculated in Eq. (4) can switch from positive to negative values with varying heights, because it combines both stable and unstable terms.
   Section 2.4 has been revised for further clarity regarding the extrapolation methodology employed.

11) ***Line 206-217 not clear: what is the difference between the 'instantaneous stability correction' and the 'long-term stability correction'?***
   The answer to comment 14 from Referee #1 also clarifies this, highlighting some differences between these two methods, as well as some benefits of the average stability correction approach against the instantaneous. Furthermore, this part of the manuscript has been revised for further clarity.

12) ***Line 221-268: so basically the stability correction has only 2 values for C per height which are the same for the whole Baltic Sea, one for stable and one for unstable. It does not matter how (un)stable the atmosphere is or whether the grid box is near the coast or further offshore: correct?***
   The mean stability correction method uses a single set of C- and C+ values for each ASCAT grid point in which the extrapolation is made, with no variation based on height.
   As in previous literature, the definition of C+ and C- has been done empirically, by comparing the theoretical stability distribution calculated from Eq. (2) against the normalized probability density (NPD) function of 1/L derived from ERA5. Through this method, we selected values of C- and C+ which allow a representative theoretical stability distribution at ASCAT grid points along the ship route. This is illustrated with two example sites, one near the shore and a second far from it.
   Additionally, as mentioned in the manuscript, the same C- and C+ values were applied to all grid points.

This clarification has been added to the paper:
…the selection of these values for this study was based on a empirical validation, by comparing the theoretical distribution calculated from Eq. (2) against the normalized probability density (NPD) function of 1/L derived from ERA5. Through this process, values were chosen to ensure that the theoretical distribution closely represented the ERA5 NPD of 1/L across all the ASCAT grid boxes along the entire ship route. Furthermore, identical values of C± were applied to all ASCAT grid points.
Also, in section 3.1:
… Finally, as mentioned in Section 2.4, the same values of the semi-empirical constant C± are assumed for the entire region, instead of using a site-specific definition of these constants.

13) ***Line 302: (fig 7). You compare the collocated approach (only ERA5 stability information at moments when ASCAT overpasses is considered) to the full campaign approach (all ERA5 stability information from the whole duration of the campaign is used). Both approaches do not include spring which is often the most stable period (cold sea water and warmer air above). Also, mean wind speed is not really relevant for wind resource assessments. So I do not really understand the sentence: 'This highlights the robustness of the employed methodology and indicates that the dataset size allows for an accurate characterization of atmospheric stability conditions during the campaign and along the entire ship track'.***
We acknowledge the concern regarding the exclusion of stability information from spring in our comparison. However, it is important to note that the stability information utilized aligns precisely with the timeframe of the lidar measurement campaign (June 2022 to February 2023). Therefore, by incorporating stability information from this period (and no further months not "seen" by the measurements), we ensure consistency in our comparison, as the extrapolation of ASCAT reflects the stability conditions during the measurement campaign. As well, as mentioned in our answers to comment 1, this paper does not aim to assess or predict wind farms energy production, but to evaluate the performance of ASCAT and ERA5 data against reference lidar measurements, providing insights regarding their potential and limitations in characterizing offshore winds.

Finally, the sentence highlighted by the referee directs to the fact that both collocation approaches yield highly similar speeds at 100 m, indicating a significant level of robustness. Therefore, the choice between the full campaign and collocation approach has a minimal impact (except near shore) on the final extrapolated ASCAT profiles. Additionally, the good agreement between the theoretical and empirical stability distributions, as illustrated in Fig. (4), further supports the consistency of the methodology. We have revised this paper excerpt to enhance the clarity of this:

Both strategies for calculating the stability correction factor and the corresponding wind profiles demonstrate a high level of agreement, except for some nearshore locations. This, together with the revealed representativeness of the theoretically derived stability distributions observed in Fig. (4) highlights the robustness of the mean stability correction approach in characterizing the atmospheric stability conditions during the period covered by the measurement campaign and along the entire ship track.

14) ***Line 307-320: 'pronounced instability in the morning?' Why would ERA5 produce stronger unstable conditions (lower 1/L) in the morning at Nynashamn? What do we know about the water temperature near Nynashamn and how it is modelled by ERA5 (shallower/warmer water between Bedaron and the mainland maybe?)? ERA5 has grid boxes of 31 km2 so model values are probably very land-contaminated in that area: can you show the ERA5 grid boxes near the harbours? What is the prevailing wind direction? Basically ERA5 and ASCAT are not very good in coastal area: maybe you should take them out of your analyses?***

    The ERA5 grid box corresponding to Nynäshamn harbour has a land mask of 56%. Therefore, the stability daily cycle at this location presents higher variations regarding to other sites. This, together with the temporal discretization of ASCAT overpasses leads to a more unstable mean distribution of the stability conditions, resulting in a lower wind speed compared to the full campaign approach, as can be derived from Eq. (4).

    Section 3.1 has been revised to provide a detailed explanation about this.

15) ***Line 341 (fig 9 10m validation): compare to Validation of DOWA ('undisturbed wind' = HARMONIE without WFP) with ASCAT (too coarse to measure wind farm effects) at 10 m height: TNO report - DOWA validation against ASCAT satellite winds | Report | Dutch Offshore Wind Atlas.***

    The results have been compared with the suggested reference (Section 3.2). "Similar results were reported in (Duncan et al. 2019) in their comparison of ASCAT and ERA5 wind speeds at 10 m over the North Sea and the Dutch coast. Specifically, (Duncan et al. 2019) found a nearly zero bias in far-offshore locations and approximately 0.6 m s^{-1} in coastal regions."

16) ***Line 341 (fig 9 100m validation): so we can conclude that ERA5 is internally fairly consistent (profile depends on ERA5 stability parameters)?***

    We appreciate the referee insight although we believe further analysis would be needed to derive final conclusion about the internal consistency of ERA5.

17) ***Line 354: '… highlighting the consistent overestimation of wind speed from ASCAT at this height'. At 100m this is not ASCAT, but ASCAT extrapolated with ERA5. And we all know that ERA5 is not unbiased at 100m (0.5 m/s underestimation) so you cannot draw this conclusion. See also Line 364/365.***

    The term "overestimation" in this context refers to a comparison between ERA5 and extrapolated ASCAT data, indicating that extrapolated ASCAT values exceed those of ERA5 at 100 m height. We have tried to clarify this in the manuscript, together with a specific mention to extrapolated ASCAT values: "overestimation of wind speed from ASCAT" has been replaced by "…overestimation of extrapolated ASCAT wind speeds at this height compared to ERA5.".

    A 0.5 m/s bias is expected between ERA5 and in situ measurements, but not necessarily in comparison between ERA5 extrapolated ASCAT values. An overestimation of extrapolated ASCAT winds relative to ERA5 at 100 m is not only plausible but a conclusion directly drawn from the results shown in Fig. 10 and 11 (from the new manuscript version).

18) ***Line 364/365: Characterisation of offshore winds for energy applications — Research@WUR and Energies | Free Full-Text | Dutch Offshore Wind Atlas Validation against Cabauw Meteomast Wind Measurements (mdpi.com)***
References have been included in the new version of the manuscript.

19) ***Line (section 3.3): you need to address the uncertainty in the lidar measurements. are the differences that you find with ERA5 and/or modified ASCAT significant? Page 14: TNO report - DOWA validation against offshore mast and LiDAR measurements | Report | Dutch Offshore Wind Atlas***
The discrepancies observed when comparing lidar observations against both ERA5 and extrapolated ASCAT are significantly larger than the uncertainty attributable to floating lidar measurements (below 2% according to (Wolken-Möhlmann et al. 2022)).

A clarification on this regard has been added in the discussion of the paper, including a reference to the paper suggested by the referee.

Other relevant literature provided by the referee:

- ***Comparing available Wind Farm Parametrisations for mesoscale models (Fitch and EWP best): Review of Mesoscale Wind-Farm Parametrizations and Their Applications | Boundary-Layer Meteorology (springer.com)***
We thank the referee for the reference suggestions; however, we think this reference is not pertinent to our study since wake effects and wind farm parameterization lie outside the scope of our research.

- ***Wind farm effects modelled with COSMO-CLM and Fitch WFP: https://wes.copernicus.org/articles/9/697/2024/***
We thank the referee for the reference suggestions; however, we think this reference is not pertinent to our study since wake effects and wind farm parameterization lie outside the scope of our research.

- ***Quadruple collocation: KNMI Technical report - Uncertainty analysis of climatological parameters of the Dutch Offshore Wind Atlas (DOWA) | Report | Dutch Offshore Wind Atlas.***
We thank the referee for the reference suggestions; however, we think this reference is not pertinent to our study since quadruple collocation is not discussed or covered in our paper. As indicated in our answer to comment 14 of referee #2 and the manuscript itself, these type of approaches are not suitable for this study.

- **Validation of HARMONIE+Fitch WFP with e.g. lidar measurements: A One-Year-Long Evaluation of a Wind-Farm Parameterization in HARMONIE-AROME - Stratum - 2022 - Journal of Advances in Modeling Earth Systems - Wiley Online Library**

We thank the referee for the reference suggestions; however, we think this reference is not pertinent to our study since this paper covers neither wake effects nor wind farm parameterization issues.

- ***Wake effects:*** ***https://www.researchgate.net/publication/340838550_Long-range_modifications_of_the_wind_field_by_offshore_wind_parks_-_results_of_the_project_WIPAFF***
  We thank the referee for the reference suggestions; however, we think this reference is not pertinent to our study since wake effects lie outside the scope of our research.

- ***Internal boundary layer caused by change in surface roughness (coast): An effective parametrization of gust profiles during severe wind conditions – IOPscience***
  We thank the referee for the reference suggestions; however, we think this reference is not pertinent to our study since this paper.

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