Dear reviewers and editor:

We would like to thank you for taking the time to review our manuscript and suggest such valuable information in order to improve its quality. Even the most trivial suggestion helped in the progress of the text's value.

A set of minor changes and typos has been corrected throughout the document. Following this introduction, you may find a detailed answer to Reviewer#2's comments. All of the changes can be seen in the new version of the manuscript, marked in red.

Reviewer#2

General comments

1) *However, in my opinion, the paper is not suited for ACP as the only objective is on validation only. It should thus be shifted to AMT – as also partly recommended by the first reviewer. As the current SI is an inter-journal SI, I hope this can be done without starting a new review process, which would be overdone…*

The authors believe that this work is suited for ACP. In fact, there have been several studies covering issues related toAeolus validation that have been published in ACP, both as scientific articles and as technical notes in this Special Issue (e.g. Chen et al., 2021; Guo et al., 2021; Abril-Gago et al., 2021).

2) *I sincerely recommend a proper language editing from which the manuscript would for sure benefit. There are some sentences and phrases I cloud not understand, some others for which I had to guess what is meant.*

We appreciate this comment, as language editing issues may not be detected by the authors of a single mother tongue. The text has been largely rephrased and we expect that the language editing is now of enough quality to be published and understood by other readers.

3) *The validation itself is done in a solid way, but taking into account the low amount of radiosonde launches (7 in total), I consider a separation between ascending and descending orbit not useful. The authors come to same conclusion, but in my opinion, it is not worth to present. In general, the whole „radiosonde section" can be shortened, as it is mostly a repetition of sentences from the comparison with the Doppler lidar (just exchanged numbers), with not much new information. The validation with the radiosondes and the Doppler instrument is completely separated without linking the results which should be from an atmospheric point of view similar. Thus, consider a reshaping and a linking of the results as partly already done.*

The authors agree with Reviewer#2. However, we believe that the separation between the orbit modes raised the conclusion that a larger database is needed for that, which is a result itself. This has been specified in line 509.

On the other hand, the "radiosonde section" has been shortened accordingly. However, the authors believe that all of the information included is relevant for the study.

Ultimately, the authors agree that not every result of each instrument has been linked, mainly because the databases, the periods (and Aeolus baselines) and especially the vertical extension of the results are different. However, the results have been linked as much as possible in lines 458 to 460, 464 to 468 and 585 to 593.

4) *Furthermore, some important information are missing, e.g., the season of the radiosonde launches etc.*

We appreciate this suggestion, as we agree that this information was missing from the manuscript. The information is now included in line 433.

Specific comments

P1#1 makes references to the comment number 1 on page 1.

P1#1) when? please specify date

It is now specified in line 14 that the date was July 2019.

P1#2) I do not understand this sentence.

Following this comment, the sentence in line 19 was rephrased as:

> "Multiple analyses were performed varying the criteria of maximum distance and the average period for the ground-based lidar measurements, in order to confirm the reliability of the criteria considered."

P1#3) from surface up to

Following this comment, the sentence in line 18 was rephrased as:

> "However, the ground-based lidar measurements were limited to the lower 3.5 km of the atmosphere, …"

P1#4) what is a bin distance? not clear...

It is true that the expression bin distance was not clear. However, following the Reviewer#1's comments, the word "bin" was replaced with "observation" throughout the manuscript. We hope that "mean observation distance" is now clear. Thus, the sentence in line 22 was rephrased as:

> "… (mean observation distance of ~75 km, to the station), …"

P1#5) is this a significant result to be stated here in this general way?

The authors think that indeed this result is a significant result of the study and this is why it must be included in the abstract, given the mean difference, Δ, obtained between the radiosondes and the Mie cloudy products.

P1#6) what was the outcome?

We understand that this information was missing from the abstract. However, we tried to present everything that was done in the study, but not every result could be included in the abstract. Nevertheless, the main outcome is now included in line 27 as:

> "Radiosondes not ideally collocated were proven to be still useful for the comparison with the Rayleigh clear observations but not with the Mie cloudy observations."

P1#7) I do not understand what is meant

The authors agree that the sentence about aerosols and clouds was not understandable. For this reason, and for a Reviewer#1 suggestion, lines 28, 29 and part of line 30 (according to previous line numbers) were removed from the manuscript.

P1#8) relevant for the paper?

Similarly to comment P1#7), lines 28, 29 and part of line 30 (according to previous line numbers) were removed from the manuscript. The removed information about aerosols and clouds was not relevant for our study and may distract the readers from the main topic.

P2#1) accommodates

Done.

P2#1)

Removed.

P2#1) deployed?

"Developed" was replaced by "carried out" in line 62, which we hope makes the sentence clear.

P3#1)

We understand that the sentence did not introduce any relevant information. Thus, line 84 (previous line numbers) was removed accordingly.

P3#2) HSR - High Spectral Resolution

The information in line 87 (previous line numbers) was rephrased and moved to line 47, in order to avoid duplications in the manuscript. As the sentence was rephrased, no addressing to HSR is now needed in line 47.

P3#3)

The authors are unable to understand what Reviewer#2 suggested in this comment. However, in order to solve possible misunderstandings, line 95 was rephrased as:

> "Additionally, an atmospheric scene classification is available for the wind products (Rennie et al., 2020)."

P3#4)

Removed.

P3#5) ever defined?

Yes, SNR is spelled out in line 86.

P4#1) I do not understand this sentence.

The sentence in line 109 was rephrased as:

> "Since the start of the Aeolus mission, multiple baseline versions were released."

P4#2)

The highlighted information was removed accordingly from line 107 (previous line numbers).

P4#3) what does this mean?

ANX was spelled out and explained, and corresponds to a code denoting overpass features. Then, line 122 was rephrased as:

> "During the second half of June 2021 Aeolus orbit setting changed from ANX4.5 (ascending node crossing 4.5, where 4.5 is the longitude of the reference ascending node orbit used to set the satellite orbit) to ANX2.0. The orbit shift took place in the framework…"

P4#4) Is this relevant for the wind validation?

We understand the issue raised by Reviewer#2. The information was included just to provide an idea about the location of the instruments and the type of aerosols that can be transported by wind. However, this information has been removed from the improved manuscript

P4#5)

Accordingly, this sentence has been removed.

P6#1) configuration is not the correct word to be used in this context

The authors think that configuration is an appropriate word in this case, as previously used in the manuscript. However, "configuration" was replaced by "products" in lines 172 and 174.

P6#2) ?

That sentence meant to say that within the, generally, 100 km spatial collocation criteria several observations at the same bin height could be considered for a single overpass. This was observed to be more frequent for the Mie channel. However, this could also happen for the Rayleigh channel, so the authors have decided to omit this information from line 177 (previous line numbers).

P6#3)

The sentence in line 182 was rephrased as:

"Aeolus observations with valid quality flags were used."

P7#1) I don't understand this. Are the percentages a conclusion from Wiitschas in general? Or did you apply the same methodology to obtain the numbers?

The authors meant to say that when the thresholds presented by Witschas et al. (2020) were used in the present work, those percentages of available observations were obtained. Thus, line 186 was rephrased as:

"Around 94.5 % of Rayleigh and 97 % of Mie observations were available for the comparison in the present work when the criteria presented by Witschas et al. (2020) was used."

P7#2) A campaign between Aeolus and radiosondes????

The authors agree with Reviewer#2 that the information was not clear. Thus, the sentence in line 214 was rephrased as:

"A special validation campaign of Aeolus using radiosondes was planned …"

P7#3) what does this mean?

The authors understand that the concepts of ascending and descending overpasses may not be clear for readers unfamiliar with satellite data. These terms are commonly used in satellite communities. Thus, line 224 was rephrased as:

"… a distinction between ascending (the satellite moves northwards) and descending (southwards) overpasses was considered …"

P9#1) Did you here average all orbits? because the HLOS should be in opposite direction between ascending and descending, thus a value close to 0 is expected, but I do not see the value to report this.

Indeed, all orbits were averaged in line 274. Although a mean value of 0 was not obtained, the obtained mean ($\pm$ SD) was 1 ± 17 (2 ± 17) m/s for the Rayleigh clear (Mie cloudy) configuration. Given the standard deviation, both mean averages are close enough to 0. This statistic is provided

as part of a general analysis of Aeolus performance, and it was not intended to be a statistical analysis about the wind fields.

P9#2) what data base? the 144 overpasses around your station? please specify clearly.

To avoid ambiguity, "whole database" was replaced with "144 overpasses" in line 279.

P11#1) if referred here, the table need to appear earlier.

We agree with this comment. The table was moved to a location before it was referred to. The same was done for Table 2. However, further changes can be expected in the typesetting stage.

P11#2)

The authors think this information is important for the manuscript, as it provides an indication of how significant each dataset is. In this case, half of the overpasses were of each mode type. If this was not the case, and for example only 20 % of the overpasses were ascending, then the results would be less significant for ascending overpasses, and the fact that only 20 % were available should be investigated. This was not the case, and then it was an interesting comment to be made.

P11#3)

This comment was taken into account accordingly.

P11#4) two sentences before you write that they are equally distributed....what is true?

Both statements were true. In line 309 it is said that from all of the available overpasses, half of them correspond to each orbit mode, so they were equally distributed. In line 315 it is said that from the available Rayleigh clear (Mie cloudy) observations *coincident with ground-based lidar measurements*, the 40 % (48 %) corresponded to descending overpasses. It is important to note that in the second case, we are talking about Aeolus observations coincident with ground-based lidar measurements. Thus, line 315 was rephrased as:

> "From the Rayleigh clear (Mie cloudy) observations coincident with ground-based measurements, the 40 % (48 %) corresponded to descending overpasses."

P11#5)

Done.

P11#6) why? what measure you are referring to?

The authors understand the misunderstanding and the mistake was solved. Thus, the sentence in line 325 was rephrased as:

> "However, the correlation coefficient was significantly higher for the…"

A similar change was implemented in line 525.

P11#7) please split into two sentences.

The authors agree that the sentence was too long. Thus, lines 328 to 331 were rephrased and reduces as:

> "The obtained slopes are significantly larger than those reported by Iwai et al. (2021) and Wu et al. (2022) of 0.98 (1.02) and 0.96 (0.92), respectively, for B10 and B11 Rayleigh clear (Mie cloudy) observations and ground-based Doppler lidars separated less than 100 km. Meanwhile, the obtained intercepts are slightly higher, i.e., -0.88 (0.22) m/s and -1.2 (-0.33) m/s, respectively."

P11#8) is there any online abstract or presentation available?

Unfortunately, there seems to be no available presentation or abstract online. However, this reference was finally omitted from the manuscript.

P12#1) Do you have evidence for the statistical significance?

The authors understand the issue raised by Reviewer#2, as in suggestion P11#6. The sentence in line 345 has been rephrased as:

> "… frequent for larger error estimates, but no linear or non-linear relation could be fit for this configuration or the Mie cloudy configuration …"

P12#2) Do you have evidence for this statement?

Yes. What was seen was that large differences between Aeolus observations and ground-based Doppler lidar measurements were associated with equally large error estimates of the Aeolus observations, and consequently to larger SNR.

P12#3) phrasing, please rephrase sentence.

The comment was taken into account and line 348 was rephrased as:

> "The error limit threshold for the Mie cloudy observations could be increased, e.g. to 5 m/s. However, similar statistical results were obtained."

P14#1) Where is this shown?

The authors understand the issue raised by Reviewer#2. In this case, line 363 was rephrased as:

> "… where the RMSE was lower (~5 m/s), contrary to the statistical error."

P14#2) please change axes limits, e.g. -5 to +5

The axis limits were set to -15 and 15 m/s in order to homogenize Figures 3 and 5 (former Figures 4 and 6). However, it was set to -8 and 8 m/s, in order to zoom in the axis but not losing the homogeneity for comparison purposes. Thus, Figures 3a, 3c, 5a, and 5c were reedited.

P15#1) what is this?

As introduced in line 257, R coefficient is the Pearson correlation coefficient. However, "R" was replaced with "Pearson correlation" in lines 384 and 513.

P15#2) How and where is this seen?

This was observed in the fact that the slope of the linear fitting and the mean Δ increased significantly when the maximum distance criteria was reduced, presented in Table 1, rows f to i. The following comment was include in line 398:

> "However, the comparison for the Rayleigh clear configuration worsened, as the slope moved further from the ideally 1:1 relation and the differences increased."

P15#3) I do not understand this concluson. Could you please interpret you numbers more and explain which parameter is valid for what? I.e. what does a changing intercept mean?

This conclusion comes from the fact that, out of all of the maximum distance criteria used, the 100 km was proven to be the best, agreeing with ESA's requirements and suggestions. Mainly, the slope, intercept and mean Δ were used to make that statement. Thus, the discussion from line 389 to 404 was improved in order to make the reasoning clearer:

> "An analogous analysis was performed taking one-hour (Table 1, row d) and two-hour (Table 1, row e) averages of the ground-based Doppler lidar measurements. The agreement between the

instruments slightly worsened when the average interval increased, especially the slope of the linear fitting. The main analysis was also repeated just decreasing the maximum distance between the considered observations and the station (fixed 30-min average) to 90, 80, 60 and 50 km (Table 1, rows *f*, *g*, *h* and *i*). In this case, the slope and intercept significantly increased (slightly decreased) when the maximum collocation distance decreased, for the Rayleigh clear (Mie cloudy) configuration. Additionally, for both configurations the number of available observations was significantly reduced, affecting the statistical error, while the Δ for the Rayleigh clear configuration significantly increased, indicating larger discrepancies between the observations. Thus, a more restrictive spatial colocation of Mie cloudy observations slightly improved the comparison results, as cloud spatial inhomogeneities were avoided. However, the comparison for the Rayleigh clear configuration worsened, as the slope moved further from the ideally 1:1 relation and the differences increased. A similar examination was performed increasing the maximum distance between the considered observations and the station to 110 and 120 km (Table 1, rows *j* and *k*). It was observed that when the maximum distance was increased the slope and intercept for the Rayleigh clear slightly reduced but also did the Pearson correlation coefficient, while the results for the Mie cloudy remained almost unaffected (only the intercept was slightly reduced). Then, based on the available dataset and the geographical features of the station region, it is recommended to work with observations within 100 km to the station, following ESA's requirements."

P15#4) row 1 or L ?

The highlighted character is a letter "L". However, the rows identification is done with lower case letters. In order to solve this issue raised by Reviewer#2, this letter is written in italics

P15#5) I guess you mean R, correct? Any reason for that?

Indeed, the authors meant to make reference to the R coefficient. The sentence in line 414 has been rephrased accordingly as:

> "The slope of the linear fitting reduced significantly after the orbit shift, while intercept increased, but most importantly the Pearson correlation coefficient decreased notoriously, due to the larger distances between the observations."

P15#6) Does this mean, that within your two-hour average wind, there is a significant variation? I.e., a steady increase, decrease, or wind direction change?

The authors understand the misunderstanding. We meant to say that when the 30-min average was extended to a two-hour average of the ground-based Doppler lidar measurements, then the results of the comparison significantly worsened. In order to avoid this misunderstanding, the sentence in line 421 was slightly changed as:

> "When this analysis was repeated taking a two-hour average of the ground-based Doppler lidar measurements the results of the comparison (Table 1, row *m*) worsened considerably, as a consequence of the wind variability."

P17#1) Again, if you refer here already to Tab.2 it needs to appear earlier. i recommend to leave out the summary sentence in the beginning here.

Similarly, to comment *P11#1)*, Table 2 was moved before being referenced.

P17#2) as this is a very limited number, it would be good to know in which season and under which conditions they have been launched.

The authors agree with this suggestion, and the information was included in line 449 as:

> "1 radiosonde was launched during a clear-sky day of autumn, 1 in a densely cloudy day of winter, 3 in partly cloudy days of spring and 2 in clear-sky days in summer."

P17#3)

The comment was taken into account accordingly.

P17#4) phrasing. Available for what?

The authors understand that some information was missing. The sentence in line 433 was rephrased as:

> "A total of 191 (43) Rayleigh clear (Mie cloudy) observations were available for the comparison."

P17#5) I don't understand this, what do you mean?

The authors meant to say that the distribution inhomogeneity of the descending observations might be associated with the fewer number of descending overpasses (just 2 out of 7) and also to the few number of cases available (7 overpasses). Thus, the sentence in line 436 has been rephrased as:

> "This inhomogeneity might be associated with the few descending overpasses available (29% of overpasses), and also to the reduced dataset of overpasses."

P17#6) Is it the minimum mean distance or the mean minimum distance?

It is the mean of all the minimum distances between Aeolus ground tracks and the station, where the minimum distance is the closest the ground track is to the station. This is a parameter widely used in the Cal/Val activities of Aeolus.

P17#7) I do not understand this sentence.

The authors agree that the sentence is not understandable, especially the second part, where some information seems to be missing. Thus, the sentence in line 439 was rephrased as:

> "Aeolus observations within 100 km were taken into the comparison."

P17#8) I know what you mean, but this is not written. Please rephrase the sentence.

Information in line 442 was rephrased as:

> "Thus, in the case of the radiosondes, most of the available Aeolus observations were coincident with radiosounding measurements. This is due to the radiosonde's wide vertical coverage, providing wind information up to the lower stratosphere, unlike the ground-based Doppler lidar system mainly restricted to the boundary layer."

P17#9)

Done.

P17#10) where is this shown? Any numbers?

The authors understand the misunderstanding and believe that this specific discussion should be removed. Thus, line 403 (previous line numbers) was omitted from the manuscript.

P17#11) ???

The authors understand the mistake and the information in line 449 was rephrased as:

> "…, while the Mie cloudy slope was slightly further."

P17#12) why on the other hand?

We agree that the expression used is not appropriate in this context. Then, it has been replaced with "Moreover", in line 449.

P17#13) but in the lowest 3 km you also have more variability in time and space. So it is for sure a combination of both.

The authors agree that this information was missing from the discussion, so it has been added to line 452 as:

> "… altitudes due to the lower SNR and where the spatiotemporal variability of the wind may affect the comparison."

P17#14) any interpretation of these findings?

The authors understand that the interpretation was missing in the text. We believe that this difference could be mainly attributed to the differences in the locations. Thus, this comment was included in line 459.

> "However, it should be noticed that the studies were performed in different locations with particular geographical features."

P18#1) Please consider that your Mie cloudy distribution is not of Gaussian shape, thus the use of mean and SD is not appropriate.

The authors agree that this specific distribution is not Gaussian shape. However, the original distribution was a Gaussian one (Figure 1a, former Figure 2a), and so were the subsequent distributions (Figure 2c Mie cloudy and Figure 4c Rayleigh clear, former Figures 3c and 5c). We understand that this specific distribution is not Gaussian shape because of the lower number of observations available, but if a larger dataset was available we expect that the distribution would be Gaussian shape. In addition, we consider that the same statistics should be used in order to homogenize the analysis.

P18#2) I do not understand this sentence.

The sentence in line 469 was rephrased as:

> "… which resulted in the average discrepancy between Aeolus and the radiosondes."

P18#3) In which case?

The authors agree this reference was not clear, so line 469 was rephrased as:

> "The mean |Δ| for the Mie cloudy configuration was significantly large, probably because of the larger distance between Aeolus observations and the radiosondes' measurements."

P18#4) But does this affect the horizontal wind?

The authors realized that the highlighted discussion was not directly related to the horizontal wind itself. We wanted to express that if each instrument measures a different cloud, then the wind field could have been different. However, we should have focused on horizontal wind, so line 471 was rephrased as:

> "The wind conditions measured within the cloud by Aeolus might be different to the ones measured by the radiosondes, so the horizontal wind captured by each instrument could be different."

P18#5) I know what you mean, but please rephrase sentence.

The authors agree that the sentence was hard to follow. Thus, line 474 was rephrased as:

> "In the case of the Rayleigh clear configuration, significant absolute differences related to large error estimates were observed. However, no significant relationship was identified between the error estimates and the absolute differences, either for Rayleigh clear or the Mie cloudy configurations."

P18#6) same sentence as above. Please conclude. it is still the same site for validation.

It is not clear what the Reviewer#2 meant to say with "same sentence as above". The authors understand that Reviewer#2 suggested to rephrase the sentence, as in previous comment P18#5. Thus, line 477 has been rephrased as:

> "A larger error estimate threshold could be set, e.g. 13 (6) m/s for the Rayleigh clear (Mie cloudy) configuration. However, the statistical results would not change significantly."

Additionally, the second part of the comment is also not clear for the authors. We understood that Reviewer#2 is asking if the site of the validation is still the same, which indeed it is. All of the validation activities took place in Granada.

P18#7) evidence?

The only evidence is that these filtered out observations present larger error estimates, which are directly related to SNR. However, it was finally removed from the text.

P20#1) ??? What does this mean?

We meant to say that some specific behavior could be observed. For example, for this case, it was observed that between 2 and 12 km asl, the biases were lower than above 12 km asl. However, we understand that this sentence does not provide any relevant information, which is provided in the next sentence. Then, the authors decided to omit this sentence from line 442 (previous line numbers).

P20#2) But even though, Mie cloudy is at 10 km mean value. So why do you have a bias in on direction only? Is it a orgraphical wave cloud effect?

The authors do not clearly understand what Reviewer#2 meant to ask.

P21#1)

Done.

P21#2) Same conclusion as for Doppler wind. Is it worth to repeat all this?

The authors agree that some duplication can be seen in this discussion. However, the authors believe that it is interesting to include it here as well, as the vertical coverage of the radiosondes is larger and so is the representativity of the results.

P21#3) Again it would be good to know for which season of the year this conclusion can be drawn...

This information is now included in line 433.

P21#4) Please. Table on one page!

This issue is now solved as Table 2 was moved.

P22#1) what does it mean?

SNR is now spelled out in line 537. On the other hand, "biases introduced by SNR issues" means those differences between the instruments that are mainly due to the fact that the Aeolus signal is significantly weaker in the lowermost part of the atmosphere.

P22#2) The radiosonde is not uplooking, isn't it?

The authors think that the radiosonde can be considered to be uplooking. However, in order to avoid any misunderstanding, "uplooking" was replaced with "upwards probing" throughout the manuscript.

P22#3) phrasing

The sentences in line 547 has been rephrased as:

> "… when the atmospheric boundary layer (ABL) height is lower and the region with enough backscattered signal is thinner (lower SNR)."

P22#4) But the Aeolus cannot penetrate the cloud, correct?

Aeolus can penetrate clouds if these are not highly dense or thick. Although it was not presented in the study, we could observe cases in which Aeolus managed to penetrate clouds and many other cases in which Aeolus was unable to penetrate them.

P23#1) ,

Done.

P23#2) if so, why haven't you done it?

The authors believed that this combination is in fact what has been done in the study. Aeolus products were studied with both Doppler lidar and radiosonde measurements, each of them providing a different type of dataset, with different temporal coverage, availability and vertical coverage.

If Reviewer#2 means that no combined study was made for the exact same overpasses, this could not be done due to the financial limitations (i.e. radiosondes could not be launched twice each week over more than one year) and instrumental limitation (i.e. for the radiosonde launches available, the Doppler lidar system provided very few measurements).

P23#3) measurements

Done.

P23#4) I do not understand this. Are you referring to the radiosonde or to the general validation?

In this final part of the Conclusions, we are presenting a discussion of the general validation and a discussion of Aeolus products itself. The highlighted comments do not refer to the radiosondes nor the ground-based Doppler lidar system.

P23#5) And what does this mean?

This entails that the products present generally less error estimates. This information was added to line 572 as:

> "However, by default the Mie channel presents higher SNR, and consequently lower error estimates, than the Rayleigh channel."

P23#6) you haven't analysed the systematic error, have you?

In order to avoid misunderstandings, the sentence have been rephrased as:

> "Additionally, for both channels the statistical differences reported exceeded the systematic errors limit set by the mission requirements (0.7 m/s)."

P24#1) This is actually not any more state of the art. Please consider providing your data. There are many options, like e.g. zenodo.

The datasets have been added to a Zenodo repository. The data availability statement has been updated as:

"Aeolus data files are available from the ESA Aeolus Online Dissemination System (https://aeolus-ds.eo.esa.int, ESA, 2022). The accessibility of these files is limited based on the ESA criteria. The Doppler lidar and radiosonde measurements used in this study are available in the following Zenodo repository: https://doi.org/10.5281/zenodo.7626611 (Abril-Gago et al., 2023). However, readers may contact Jesús Abril-Gago (jabrilgago@ugr.es) and Juan Luis Guerrero-Rascado (rascado@ugr.es) before using the measurements."

and the reference was added to the list as:

"Abril-Gago, J. Ortiz-Amezcua, P., Guerrero-Rascado, J. L., Alados-Arboledas, L.: Ground-based Doppler lidar and radiosonde measurements for Aeolus wind products validation, Zenodo [data set], https://doi.org/10.5281/zenodo.7626611, 2023."

P25#1)

The citation was included by mistake and has been removed.

P25#2)

The citation was included by mistake and has been removed.

P32#1) any downloadable information?

Unfortunately, there seems to be no downloadable information online. However, this reference was finally omitted from the manuscript.

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