Response to RC1: 'Comment on egusphere-2023-1465', Anonymous Referee #1, 08 Aug 2023.

We appreciate the valuable comments given by the reviewer. We repeat the reviewer's concerns and provide our respective responses in italics.

In this work, the authors analyze radar wind datasets from two sites at different latitudes, infer summer wind maxima in wind components and derive trends. They find a robust strengthening of the mesospheric westward wind at mid latitudes. The influence of daily varying geomagnetic activity on the winds is studied and excluded as source for significant trends. Gravity waves are suggested as cause of the trends.

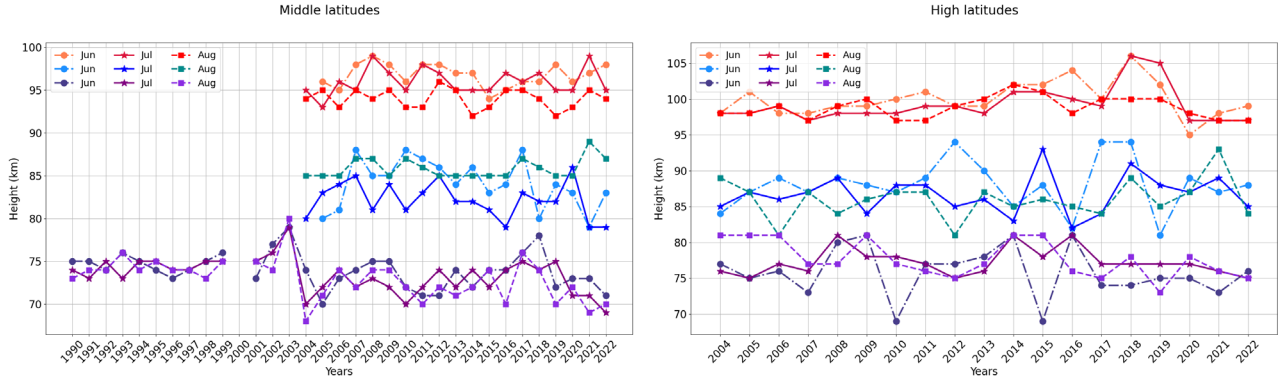
The paper is well structured and the figures are clear. In few places there are descriptions that can be confusing, mainly when the authors speak of "maximum velocity amplitudes" when actually absolute wind values are meant, which is likely a language problem. I notice that title is a bit unspecific, being almost identical to the special issue title.

We understand the confusion with the nomenclature. When we observe a monthly median vertical profile of the horizontal velocities, the wind jets look like a wave with the maximum as the highest amplitude of the velocity. From there we started to call it amplitude. We will remove the 'amplitude' word in the manuscript.

The title has been changed to "Long-term studies of the summer wind in the mesosphere and lower thermosphere at middle and high latitudes over Europe".

The authors list a number of similar observational studies, sometimes based on the same datasets, that sometimes come to contradicting results regarding the trends. This is attributed to different years or altitude ranges. If trends depend so sensitively on the selection of years or altitude ranges, this should be investigated in more detail to be of scientific value. For example, if altitude of wind maxima change with time, this could have been documented.

Indeed there are studies indicating that the MLT heights have decreased in the past decades (Peters et al. 2017, Dawkins et al. 2023). Having this knowledge in mind, we used the wind maxima and not the altitude as the object of study. We did explore the altitudes of the wind maxima and found no trend in the 19-year time series. For the first years of the longer time series (33 years), the accuracy of measured altitudes of this initial radar system is arguable. So in the case of testing for a trend, we only consider the years after 2004. We haven't included the plots since there are no significant trends, but indeed they show variability over the years. Below are the Figures, in red colors are the eastward jets, in purple are the westward jets



and in blue are the southward wind maxima. The error bars are not included, but the distribution depends on the instruments as follows: Middle latitudes eastward and southward +- 1km, westward +- 5km. High latitudes eastward and southward +- 2km westward 2km.

Another possible source of variability, longitude, is not mentioned at all. The authors generally refer to "middle and high latitudes" and "zonal mean winds" and therefore make the impression that their results are valid for all longitudes. This however cannot be inferred from the datasets the authors used, and some discussion of this topic should be included. Whether the obtained results are only valid locally or in the zonal mean is relevant.

Thank you for highlighting this. Indeed our results are local since the radars are in a fixed location. We did mention this in section 2.1 lines 106-107. However it is known that the zonal climatologies are a good representation of global behavior, but the trends are not. This is one of the potential reasons why we have found strong trends while other authors and instruments at different locations don't. Furthermore, we suspect that the observed variability is actually a consequence of GWs or PWs, including their local/regional properties. We are including this comment in the discussion.

No references to model studies were made, and models were mentioned only briefly in one sentence at the end of the manuscript. The work is mainly a report of measurements without detailed exploration of the findings regarding the underlying mechanisms. For example, periodograms with periods of 2-4 and 11 years are presented but discussion with references to QBO and ENSO remain very vague. The same applies to gravity waves being the suggested cause of the trends, but no proposals or attempts were made at how this could be tested or verified. If this is out of scope, but similar studies are available, that sometimes agree, sometimes contradict, the value of presenting measurements only is limited.

We are adding citations of works using models in the discussion briefly. Sadly there are not many studies with models that represent the MLT summer well enough in order to compare it directly. We will add more citations. In the case of QBO and ENSO we will expand the discussion supported by references. We are expanding the GW discussion as well, supporting it with references since it is a study that the authors intend to study next. We will share this part of the discussion in the following reply.

I noticed a mismatch in the presented data between Fig. 1d that shows meridional wind data above 70 km only (where the wind is southward) and Fig. 4c and 4d where, I think, this same data was partitioned regarding Ap and that shows meridional wind data also below 70 km altitude, and the wind is suddenly northward.

It is correct that there is a difference in the data shown, mainly because for the first part (wind climatologies) we only used SMR for the meridional component as it is well covered by SMRs, but for the geomagnetic activity study, we wanted to extend the altitude range and also implemented the data from the PRRs. Part of this difference is because of the objective of the study and the difference between instruments. While the zonal wind is strong and shows strong agreement, the meridional wind is weaker and the comparison is not that good. Wilhelm et al. (2017) studied these differences. We will add more information in the data section explaining this in more detail.

I also wonder why a larger Ap threshold of 20 is used for mid latitudes than for high latitudes. The authors refer to different geomagnetic latitudes of the radar sites, but I don't see why this is an argument. I would appreciate more substantial explanations and discussions, e.g. of the mechanisms of how wind maxima

are a "proxy for MLT dynamics", or how geomagnetic activity affects wind maxima. In Fig. 4b, is the enhancement in 2020 related to any major solar event? Do other studies exist as it seems to be a major effect?

The different thresholds are for the following reasons. The idea was to follow Jacobi et al. (2021) who used Ap 20 for middle latitudes and we extend the analysis to high latitudes. For middle latitudes, this value reliably discriminates strong geomagnetic distortions from quiet periods. For high latitudes, it was found by Renkwitz et al. (2017), that the majority of the particle precipitation events already occur at kp=3 (~ Ap=15). Larger distortions will affect more southern locations. Given the fairly low solar activity in the last cycles and to get a sufficient amount of data for robust statistics we tend to use Ap=15 instead of Ap=20, which is used for the longer time series at middle latitudes (Fig. 6,7). We'll modify lines 132-135 to make this more clear.

Regarding the year 2020, it looks enhanced due to the low amount of days with geomagnetic activity above the used limit (see Fig. 7). We could remove the data from the figure, but Figure 6 intent is to have a visual behavior of the winds under low and high geomagnetic activity. We will add this comment to the text.

Regarding the spectral analysis I wonder why a generalized Lomb-Scargle analysis is preferred over a Fourier transform. Isn't the data evenly spaced? The authors do not mention measurement uncertainties. I guess they are smaller than the variability. In Fig. 3, significance levels could have been added to the plots.

Indeed the data is evenly spaced, but for the time series from the PRR Juliusruh, we have a missing year (2000). To avoid interpolation over the missing value and inconsistency, we made use of Lomb-Scargle analysis. We also compared to Fourier transform and checked the values were in agreement with the Fourier transform for all the time series. Mossad et al. (2023) compared both methods and found that LS is slightly more accurate for estimating the amplitude of a single frequency in the presence of minor gaps. The only disadvantage of LS compared to FFT in our case is computation time which is not really an issue.

The monthly variability is used as uncertainty since it is bigger than the instrument's uncertainties, as the reviewer correctly assumed. We mention this on lines 120-125, and due to the FAP being only related to the main peak, we did not want to add it to the figure to avoid common confusion with the rest of the peaks. We will add the comment of the uncertainties being smaller than the monthly variability.

I come back to the author's nomenclature of "the maximum velocity amplitude" as their proxy for MLT dynamics which confused me. I expected an "amplitude" to relate to an oscillation, for example a tide or a gravity wave. The meaning of "maximum" was unclear, it could have related to a period of time, or altitude, or a peak-to-peak amplitude.. ? In l. 79 the authors write "the maximum velocity amplitude of the horizontal winds, independent of altitude, variability, and trends..", and I wondered how this value could be independent of altitude, variability and trends. Then, in l. 111, "the maximum amplitude of the velocity per month" seemed to indicate some deviation from a monthly mean. And indeed in l. 115 a reference to "monthly median values" was made, but Fig. 2 shows maximum values of wind components, and not amplitudes (that is differences of wind values) of any kind. I suggest to improve the language in these descriptions or add more details, and not use the term "amplitude".

Thank you for this comment. Indeed we will change it to make it clearer. In the case of the velocity maxima being independent of altitude, we refer to the fact that we did not use fixed altitudes to obtain the wind value. Regarding the "amplitude", we already answered and explained above.

A similar language problem might apply to "zonal mean wind" (l. 196 and others), which implies a global zonal mean, when in fact the authors probably meant "mean zonal wind". Also, the terms "a mid latitude" or "a high latitude" might be more honest than generally speaking of "mid latitudes" or "high latitudes", as only data from sites at one specific longitude was used.

It will be corrected.

In general, grammar could be improved. Examples are "Below the mesosphere is located the stratosphere..", "linear functions were adjusted to…" instead of "fitted to", "periodograms were extracted…" instead of "calculated", "The zonal component is built with the combination of two datasets ". Some sentences lack subjects, e.g. in l. 91, l. 138, l. 308 or in Fig. 6 caption. Specific comments and questions are listed by line number:

line 4: "mainly focuses on the summer season" what part of the study does not focus on the summer season?

Thanks for highlighting this. The climatologies are covering the full year, but we understand the point and we will change it.

line 6: is there no northward wind? Northward wind is mentioned in l. 16

Indeed there is a northward wind, e.g. during spring and autumn, but for the first part of the study focusing on the summer wind maxima, the northern component does not have a distinct maximum. Nonetheless, we still investigated the meridional wind northern component for a potential geomagnetic influence.

line 23: 1980s

Corrected, thanks.

line 28: regarding the broad subject of greenhouse gas monitoring, the authors only cite the works of two authors from the author's institute

Thanks for pointing this out, we will add more references.

line 91: FMCW is not defined. In l. 103, abbreviations are defined that are not used a second time

FMCW stands for Frequency Modulated Continues Wave, a technique that was used for the former Juliusruh PPR. System descriptions and further references can be found in Singer et al. (1992). Since the modernization in 2001 a pulsed PRR is used.

line 104: what is the horizontal dimension of the observational volume?

Monostatic SMRs typically cover about 250km radius, while for the combination to radar networks it depends on the number of systems and their separation reaching 500km and above.

line 111: do the "different ranges in the … data used for the climatologies" refer to the range of the color bar in the plot?

It refers to the altitude range, using different instruments to capture the wind maxima where they have the best capabilities. We will make it more clear. Thanks.

line 130: what is meant by the "complete" 19-year time series?

It just means the full time series. We will remove the word. Thanks.

line 134: what was the result of this study? How did the MLT respond to the change in the index?

We answer this above, when we explained the different threshold of the Ap taken for different latitudes.

line 140: what is meant by "the complete time series from the selected Ap index"? Isn't it rather data from all days with Ap index values above or below the respective threshold?

Indeed, thanks, we will make it clear.

line 154: "at the mesopause"

Thanks.

line 230: the study contradicting the author's results, was it done at a different longitude?

No. One of the radars used for this study is the same that we are combining to obtain our high latitudes time series of the eastward and southward wind maxima. Even that the trend does not agree with ours, in Figure 2d between 2004 and 2012 a trend is visible that agrees with their study. This is explained in lines 231-234.

line 262: what is a "missing solar cycle"? Do you mean conditions of solar minimum?

The missing solar cycle effect comes from Hervig et al. (2019). We will rephrase to be more clear. Thanks.

line 295: please add year range

We will add it, thanks.

line 300: "three studied months" this could me misinterpreted in two ways. First, the dataset is larger than three months, and second, the decline does not occur over the course of three months.

We understand, and we will rephrase. Thanks.

Citations:

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Mossad, M., Strelnikova, I., Wing, R., and Baumgarten, G.: Assessing Atmospheric Gravity Wave Spectra in the Presence of Observational Gaps, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2023-1598, 2023.