Authors Response

1 Review Comments and Detailed Authors: First technical review comments

Review: Major Comments

The manuscript addresses a relevant topic and presents a technically plausible extension of the VVP method to a spherical formulation. However, it does not demonstrate that this extension constitutes a substantive methodological advance. The lack of rigorous validation, uncertainty analysis, and scale-aware interpretation prevents the main conclusions from being convincingly supported. I therefore recommend a major revision, as the current version does not meet the level of scientific rigor required for publication.

General Reply: *We thank the reviewer for his assessment of the submitted manuscript and appreciate the feedback. We revise and clarify aspects related to implementing the SVVP (spherical volume velocity processing) algorithm. In particular, we expanded the technical details section and an appendix to outline all steps involved in verifying our implementation and its physical applicability. However, the concern about rigorous validation cannot be addressed, as there is no other technique to provide ground-truth information about spatial variability or vertical winds. Our justification of the vertical winds is based on SABER CO2 trace gas measurements, which are at least a good proxy for the vertical motion and its seasonal behaviour. We provide a more detailed and comprehensive answer below in our detailed replies. Furthermore, we consider the comments as a review rather than a technical correction, and thus, we use the opportunity to expand the manuscript as requested by the review.*

1. *Added Appendix on error propagation*
2. *Added Appendix on latitudinal Spherical coordinates*
3. *Expanded paragraph on error propagation*
4. *Revised equations*
5. *Fixed a plotting mistake in Figures 7 and 8 (displays misplaced)*

Comments:

Reviewer Comment Unclear definition of improvement in vertical winds. The authors claim that the spherical formulation improves the recovery of vertical winds; however, this statement is not quantitatively supported. It is unclear what metric of improvement is being considered (e.g., bias, variance, spectral fidelity), and over which spatial and temporal scales this applies. A scale-dependent analysis is necessary to determine whether the method recovers physically meaningful vertical structures or simply modifies the smoothness of the solution.

Response: *A quantification of the improvement of the vertical wind is not possible. There is no ground truth for the mesopause region. Models tend to underestimate the vertical transport, while observations often do not reach a sufficient sensitivity to detect the 'small' mean values. The lower long-term benchmark is given by the trend of the meteor peak altitude (Stober et al., 2014; Dawkins et al., 2023, 2024). These measurements indicate that the decade mean value is much smaller than 1 mm/s. A summer mean vertical wind of 1 m/s over two months results in an upwelling of 172.8 km. This appears physically unrealistic. Models predict values of 2 cm/s up to 6 cm/s for the summer mesopause upwelling and about 1 cm/s for the winter downwelling. A comprehensive discussion of these numbers is provided in a companion paper. Furthermore, vertical wind measurements and the resulting magnitude also depend on the temporal resolution and the resolved small-scale GW dynamics. Currently, we can resolve waves with a spatial scale corresponding to twice the diameter of the observation volume for the SVVP (about 250 km diameter). The temporal resolution is limited to about 15 min. However, the 30 min temporal product appears to be more reliable due to the higher number of detections entering the statistical inversion to obtain the winds. We investigated the scale dependency by presenting results for different diameters and different temporal resolutions.*

Reviewer Comment: Confounding between method and regularization. The comparison between methods is not well controlled. Statements regarding smoother wind fields are not meaningful without explicitly accounting for regularization strength. Comparable levels of smoothness can be achieved in either method by tuning regularization parameters. Without a consistent and documented regularization framework, the comparison does not isolate the effect of the spherical formulation.

Response: *In our analysis, the Tikhonov regularization parameter (alpha) was tuned for the 3DVAR+DIV method, and the resulting wind fields were compared to those from the SVVP method. The regularization strength for the 3DVAR+DIV method is indicated by the alpha values used. We acknowledge that by tuning the regularization parameters of the 3DVAR+DIV method, comparable smoothness similar to the SVVP wind field is achieved. We intended to demonstrate that the 3DVAR+DIV method can produce wind fields similar to SVVP with appropriately tuned regularization parameters, rather than to attribute differences solely to the spherical formulation. We also have to note that the strength of the Tikhonov regularization parameter depends on the implemented non-linear error propagation. The strength of the Tikhonov regularization is optimized to ensure a -5/3 spectral slope of the horizontal wavelength spectra (Kolomogorov Spectra). A lower regularization parameter leads to spurious solutions and physically unrealistic wind fields related to the variability in the space and time sampling. Stronger*

Tikhonov regularizations result in smoother wind fields, but likely suppress small-scale GWs. There is no explicit framework to derive a perfect regularization strength given the available data and differences between the meteor radar network. Furthermore, the diurnal cycle of the meteor flux and seasonal effects along Earth's orbit revolving the sun introduce additional effects in the number of detections and hemispheric differences. Currently, it is considered the best strategy to use a fixed lower limit that is tuned with the lowest count rates during the day and season.

Reviewer Comment: Lack of ground truth and ambiguous validation. The manuscript lacks a well-defined reference for validation. The comparison with DIV-VAR is problematic, particularly since the authors acknowledge that DIV-VAR outputs may contain both physical signal and noise. If the structures are physical, the discrepancies between methods suggest differences in resolved scales. If they are noise, the comparison is not meaningful. In both cases, the current validation strategy does not support the claimed performance improvements.

Response: *There is no ground truth for winds and higher-order kinematic terms in the mesosphere. GCMs often fail to reproduce the seasonal climatology of winds at these altitudes (see https://acp.copernicus.org/articles/21/13855/2021/acp-21-13855-2021.html). Other measurement techniques often only provide episodes of data, but lack continuity. Space-borne platforms often don't reach the required temporal resolution. The 3DVAR+DIV algorithm and the monostatic retrievals have been validated against the gravity waves caused by the Hunga-Tonga eruption and the mechanistic circulation model (https://angeo.copernicus.org/articles/41/197/2023/angeo-41-197-2023.html and https://acp.copernicus.org/articles/24/4851/2024/). We intentionally compare only the seasonal behaviour of the mean winds to avoid issues caused by the small-scale variability.*

Reviewer Comment: Absence of uncertainty quantification. There is no rigorous treatment of uncertainties. The manuscript does not address measurement error propagation, sensitivity to sampling density, or robustness to data gaps. Given the ill-posed nature of the inversion, uncertainty quantification is essential to assess the reliability of the reconstructed winds.

Response: *The algorithm includes a full non-linear error propagation that was introduced over the past decades and described in detail in other publications and references therein. (see for a summary: https://amt.copernicus.org/articles/15/5769/2022/amt-15-5769-2022.html). We added an appendix providing more details of the error budget.*

Reviewer Comment: Lack of scale-aware analysis. A central limitation is the absence of scale-dependent diagnostics. The manuscript does not establish which spatial scales can be reliably reconstructed by each method. Without this, differences between methods may reflect implicit filtering rather than improved physical reconstruction.

Response: *The 3DVAR+DIv algorithm is designed to resolve small-scale features in the wind field. It is based on classical retrieval techniques to cure the ill-posed mathematical problem. There is no other technique that can provide spatially resolved winds at such high temporal resolution in the mesosphere. Airglow is limited by cloud conditions and often contaminated by Aurora activity above Fennoscandia. Other radars only provide data for a rather small geographic location and short episodes. LIDARs often suffer from tropospheric cloud coverage. The SVVP algorithm was developed to permit a simplified comparison for large-scale winds and the seasonal climatology, as well as the inter-year differences. The benefit is obvious. The SVVP is computationally much more efficient and independent of a priori information. Therefore, a meaningful comparison between the 3DVAR+DIV and the SVVP is only feasible at climatological scales or for inter-year variability. Higher-resolution case studies have been performed for the Hunga-Tonga gravity wave.*

Reviewer Comment: Unsupported claims of bias reduction. The authors claim a reduction in bias, yet this is not supported by comparison to an independent or known reference. In the absence of ground truth, such claims remain speculative. A more rigorous validation framework is required, for example, using synthetic data in an Observing System Simulation Experiment (OSSE) configuration, as commonly implemented in the remote sensing community (e.g., https://doi.org/10.1175/JTECH-D-15-0038.1).

Response: *Currently, a community paper presenting results of data assimilation of meteor radar winds leveraging the OSSE framework is submitted to JGR. This framework is only implemented for the horizontal winds. At mesospheric altitudes, there is no ground truth, which has implications for the validation process. The bias reduction due to the spherical geometry was demonstrated previously (https://amt.copernicus.org/articles/11/4891/2018/). The winds are calculated using average domains of 200-250 kilometer radius or 500 km in diameter. Below, we summarize the importance of a WGS84 geometry compared to planar geometry (see reply below).*

Reviewer Comment: Insufficient error budget and interpretation of geometric effects The manuscript suggests that the spherical formulation improves vertical wind reconstruction, but does not demonstrate that geometric representation is a dominant source of error. In practice, limitations are often driven by sampling geometry, data sparsity, angle uncertainty, and inversion assumptions, among others. If curvature effects are significant, their contribution should be quantified relative to these other error sources.

Response: *The VVP and SVVP are implemented with the same non-linear error propagation. The differences presented in Figures 4-9 show the impact of the spherical coordinate implementation compared to planar geometry. All retrievals are performed within the same script using the same reading routine, the same inversion algorithms (svd-solver), and the same treatment of data gaps and angular uncertainties to eliminate these effects, ensuring that only the curvature effects due to the spherical geometry remain.*

distance (km)	altitude (km)	zenith (°)	azimuth (°)
100	0.9319	89.1040	43.2982
200	3.2769	88.2086	41.4890
300	7.1832	87.3141	39.5674

Table 1. Comparison of a plane geometry projection at 0 m altitude with nominal azimuth of 45°, at 90° off-zenith angle for distances of 100, 200, and 300 km.

Table 1 shows the deviation between a pure plane geometry assumption for a nominal azimuth and zenith angle. It is obvious that for our large domain, it is mandatory to account for the spherical geometry, which clearly presents a big advantage. Please note that the VVP, as implemented already, includes a WGS84 correction for the normal line-of-sight projection, but not yet for the volume quantities or higher kinematic terms, which were estimated along a constant latitude and along the meridian.

2 Review Comments and Detailed Authors: Reviewers 2 - 6

Review: Review Report

This manuscript presents a significant methodological advancement for multistatic meteor radar wind retrievals by formulating the Volume Velocity Processing (VVP) in a rigorous spherical coordinate system (SVVP). The authors demonstrate the impact of this new implementation using four years of observations from the Nordic Meteor Radar Cluster and provide a thorough intercomparison with both the traditional plane-geometry VVP and the more sophisticated 3DVAR+DIV tomographic retrieval. The topic is timely, the analysis is comprehensive, and the results convincingly show that accounting for Earth's curvature is essential—particularly for vertical wind estimates and higher-order kinematic quantities. The manuscript is generally well structured, and the figures effectively support the narrative.

Nevertheless, several aspects require improvement before the paper can be recommended for publication in Atmospheric Measurement Techniques. These concern the presentation and discussion of the validation/uncertainty aspects, the quality of the language, and the completeness of the figure captions and citations. Below, I list specific comments and suggestions, separated into major and minor issues.

General Reply: *We thank the reviewer for the thorough assessment of the submitted manuscript and appreciate the constructive feedback. In response, we have revised and clarified several aspects, including the discussion, language quality, figure captions, and citations. In particular, we have addressed most of the specific concerns in a companion paper and have further expanded the section on the derivation of Volume Velocity Processing on a Sphere, now including details on vertical wind bias correction. For the analysis of radius performance, we have added Figure 14, which shows optimal coverage for the selected reference point. Detailed responses to each comment are provided in the attached PDF. The revised manuscript will be prepared with LaTeX diff tracked changes.*

1. *Expanded the method section to address bias correction*
2. *Added an Appendix on Wind Seasonal Climatology*
3. *Moved Figures 5 and 8 to the new appendix*
4. *Added Figure 14 to illustrate the influence of the circle radius*

Comments:

Review: Major Comments

Reviewer Comment: Treatment of vertical wind bias and uncertainty. The manuscript mentions that a bias correction of 1-4 cms^{-1} is subtracted from the vertical winds (lines 225–227, 417–423) based on the assumption that the long-term mean vertical wind should be zero.

This correction is critical because the absolute values of the vertical wind are central to the conclusions. The authors should provide more details: how exactly is the bias estimated? Is it a single constant per station, per altitude, or per domain size?

The statistical significance of the debiased vertical wind patterns (e.g., upwelling of $10\ cms^{-1}$ in summer) should be evaluated, perhaps with confidence intervals or by showing that the bias-subtracted values indeed vary seasonally and not merely as residual noise.

Moreover, the 3DVAR+DIV vertical winds are an order of magnitude larger (lines 328–330). The authors attribute this to lower meteor counts per grid cell and the Tikhonov regularization. A more quantitative explanation (e.g., typical measurement response, averaging kernels) would strengthen the discussion.

Response: *The bias is estimated from the calculated climatology and the five-year-long time series consisting of all measurements. The climatological bias is almost identical to the resolved long-term record. The bias is estimated for each altitude separately. The most precise measurement of the vertical motion is provided by the meteor ablation altitude, which has been determined for almost 2 decades and shows a long-term trend and solar cycle effects Stober et al. (2014); Dawkins et al. (2023). Transferring these long-term trends into a vertical velocity corresponds to* $2.7 \cdot 10^{-6}$*m/s, which is essentially negligible. Therefore, the Doppler- and divergence-derived vertical winds should result in a seasonal mean vertical velocity that is zero for all altitudes. Considering that 1 mm/s vertical velocity would result in an upwelling of 31.536 km. This is not observed from trace gases or other indirect measurements on seasonal time scales (Yue and Wang, 2025a). A more detailed discussion is provided in a companion paper. Figure 1 shows a comparison of the derived bias correction and the corresponding measurement statistics.*

We also performed a comparison to existing meteorological analyses and the free-running SE-WACCM-X model computing. The grey shaded areas indicate the 1-σ *interval of our statistical uncertainties, which also includes geophysical variability. Figure 2 shows the resulting vertical winds integrated as a seasonal mean profile average between 84 and 94 km. The model data were prepared with a similar data pipeline. The observations are convolved with the model grid and averaged. The 3DVAR+DIV winds are much more sensitive to projection errors, causing a substantial increase in the vertical winds. Typically, only 1-3 meteors enter the solution of a grid cell, which appears to be sufficient for horizontal winds, but results in large uncertainties for the vertical winds. Typical uncertainties are in the order of 1-3 m/s, and thus, the seasonal pattern can not be reproduced and is dominated by individual measurements and the much larger uncertainties. The comparison here is only included to demonstrate that for the seasonal vertical winds, the 3DVAR+DIV algorithm needs to be further improved. The measurement response shown in Figure 15 underscores our conclusion that a 200-250 km radius is the optimal sampling*

volume. The averaging kernels are not stored due to the enormous size of these data.

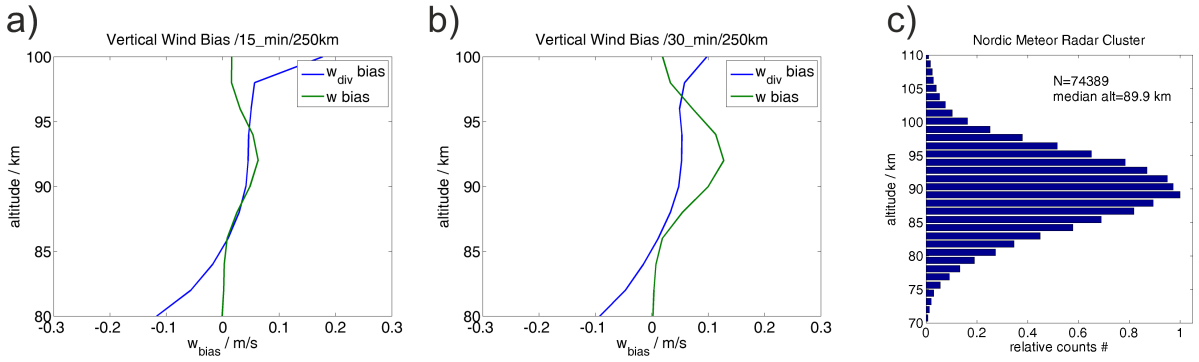


Figure 1. Vertical wind seasonal bias for 15-minute and 30-minute temporal resolution is shown in panels a) and b), respectively. A normalized altitude distribution is visualized in panel c) as a proxy for the measurement statistics.

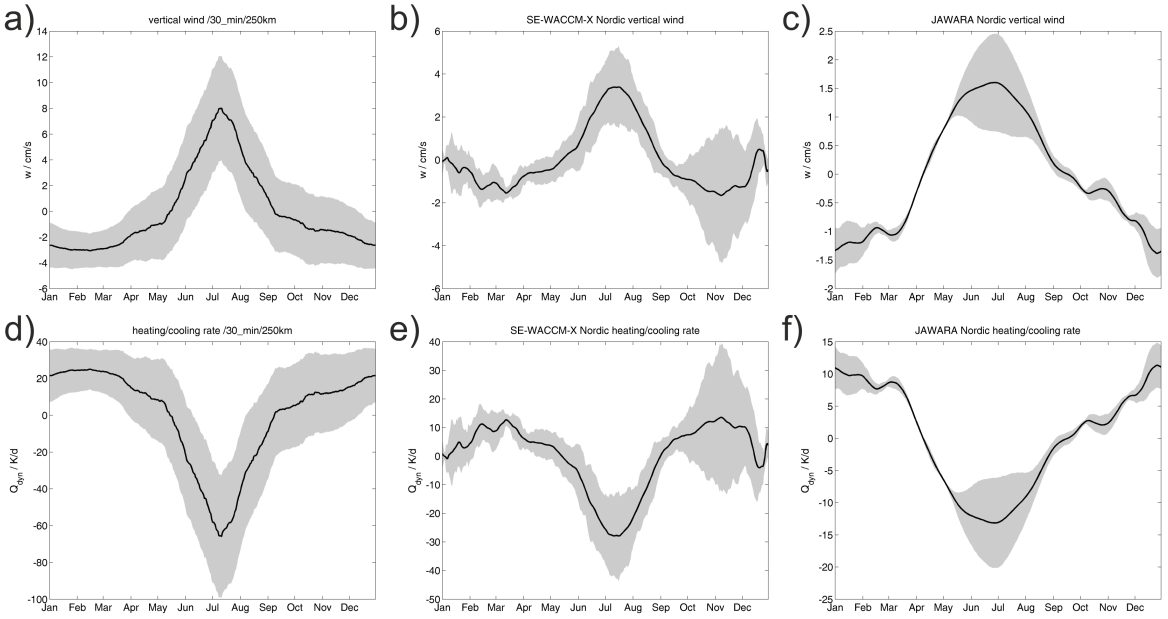


Figure 2. Vertical winds (top row) and heating/cooling rate(bottom row) for Nordic Meteor Radar Cluster with a 250 km diameter domain (left column), the free-running SE-WACCM-X (middle column), and the JAWARA reanalysis (right column).

Reviewer Comment: Intercomparison with 3DVAR+DIV and sensitivity to Tikhonov parameter. Section 4.3 presents a single snapshot (1 January 2021) to illustrate the effect of the regularization parameter.

The representativeness of this single case should be commented on. Are the differences shown robust across many days/seasons?

The text states that "the 3DVAR+DIV performs best with regularization strengths between $\alpha = 0.4\ 5$. How was this range determined? A brief description of the metric used (e.g., cross-validation, spectral analysis) would be helpful.

The comparison between VVP and 3DVAR+DIV vertical winds (Fig. 14) shows a regression slope of only 0.105 (spherical) and 0.019 (plane). The discussion merely notes that this "validates and [is] consistent with the climatology results". Given the huge difference in magnitude, a deeper analysis of why the two methods disagree so strongly is warranted. The role of the divergence-derived vertical wind in 3DVAR+DIV versus the Doppler-derived vertical wind in SVVP should be explicitly addressed.

Response: *The Tikhonov parameter was optimized using several criteria: the mean wind should not have a bias compared to the monotonic mean wind analysis, the spectral analysis shows planetary waves, tides, and gravity waves (coherent wave structure) (Qiao et al., 2025) and a spectral slope of -5/3 (horizontal wavelength spectra). Furthermore, we used the Hunga Tonga eruption to test whether the gravity wave was captured (Stober et al., 2023, 2024). We expand this discussion in the revision to clarify our choice, but also to make clear that the numbers are based on rather soft criteria. However, the shown examples (we added one more) are representative of typical wave fields at 90 km altitude.*

The difference in the magnitude of the vertical winds obtained from the 3DVAR+DIV and the SVVP has several reasons. The 3DVAR+DIV implicitly enforces the continuity equation, which depends on the integration boundary. During the retrieval, we enforce a zero vertical mean when integrated above the entire meteor layer for each grid cell. This introduces rather large uncertainties for the climatological mean values, which cannot be reproduced with this approach. Furthermore, the assumption of an incompressible flow does not hold for the small scales. Mach numbers above M>0.5 are frequently observed (see Qiao et al. (2025).

Reviewer Comment: Temporal and spatial resolution sensitivity. The analysis of temporal resolution (15, 30, 60min) and domain radius (200–400km) is well motivated.

In Fig.9, the vertical wind regression slopes deviate markedly from unity (1.965 for 15 vs 30min, 0.530 for 60 vs 30min). The authors suggest this reflects the gravity-wave spectrum, but they do not investigate whether this scaling is linear or whether a "true" reference exists. A more systematic treatment—e.g., power spectral density comparisons or an estimate of the gravity-wave contribution—would elevate the discussion.

The optimal radius (200–250km) is justified by the 3DVAR+DIV measurement response (Fig.15). However, the measurement response shown is for the 3DVAR+DIV retrieval, not for SVVP. It would be more direct to show, for the SVVP, how the number of meteors and the condition number of the least-squares matrix vary with radius. The current argument is indirect and should be clarified.

Response: *We will add more information on the scaling behaviour. So far, it shows a linear scaling dependency. An absolute 'true' reference is not available. A detailed analysis of the Stokes drift can only be obtained from the 3DVAR+DIV by analyzing spatial gravity wave spectra. Averaging 60 minutes leads to a smearing of fast (ducted) gravity waves, which induces larger deviations. The 15-minute retrieval suffers from data sparsity and likely does not capture all the small-scale features with their maximal amplitude. The 30-minute appears to be somewhere in between. Figure 3 shows the frequency spectra for all three temporal resolutions. From Lidar observations, it is known that the smallest scales are related to the largest vertical velocities (Chu, Xinzhao et al., 2026).*

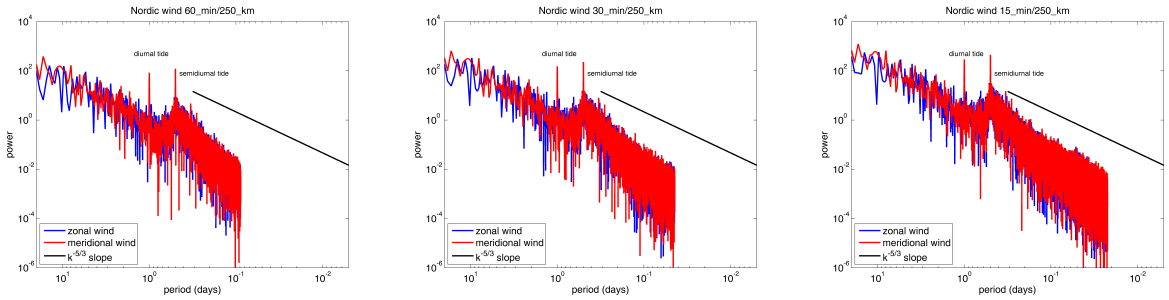


Figure 3. Atmospheric Wave spectra for the horizontal wind components for 15, 30, and 60 minutes.

Review: Minor Comments

Reviewer Comment: Figures 4 and 5 (monthly and seasonal composites) are redundant; consider merging them or moving one to supplementary material.

Response: *We move long-term composites to the Appendix material.*

Reviewer Comment: In Fig.6 (correlation plots), the robust regression method should be named (e.g., Theil-Sen, iteratively reweighted least squares).

Response: *We used iteratively reweighted least squares (IRLS) with a bisquare (Tukey) weight function. The figure caption has been updated accordingly.*

Reviewer Comment: The comparison of information content / Shannon entropy is only briefly mentioned and not shown. Either remove this remark or add a figure/table that supports it.

Response: *We use the measurement response in Figure 14 to justify the circle of influence. We will link this paragraph to this Figure to support its relevance.*

Reviewer Comment: The paragraph about the influence of the circle of influence on vertical wind is very insightful. It would be even stronger if the authors could show a simple plot of the retrieved vertical wind (or its bias) as a function of radius for a fixed time/altitude.

Response: *We already added Figure 1 in a previous reply. A detailed discussion is available in a companion paper. We revised and expanded the information in the revised version.*

Reviewer Comment: The conclusion that SVVP is "more physically consistent" is well supported, but the limitations of the method (e.g., inability to capture non-linear gradients, reliance on a single reference point) should be acknowledged more explicitly.

Response: *We will point out the limitations of the method concerning the underlying assumptions.*

Review: Review Report

This manuscript presents an innovative extension of the traditional VVP technique by explicitly accounting for spherical geometry and Earth curvature effects. This is an important and often overlooked issue in large-scale radar wind retrievals. The mathematical derivation is rigorous, and the analysis is comprehensive. The study represents a valuable methodological contribution to MLT wind retrievals.

However, I have several concerns that should be addressed before publication.

General Reply: *We thank the reviewer for the thorough assessment of our manuscript and appreciate the constructive feedback. In response, we have revised and clarified all concerns. We have submitted a companion paper that addresses the validation of the novel method, SVVP, and de-biasing methods. However, we note that the sensitivity experiment is computationally intensive and may not yield robust results. The Tromsø and Alta stations provide the most recorded data, and removing them would compromise the reliability of the estimation. We have included Figure 14 to illustrate optimal coverage for the selected reference point. Detailed responses to each comment are provided in the attached PDF. The revised manuscript will be prepared with LaTeX diff tracked changes.*

1. *Expanded the method section to address bias correction*
2. *Added an Appendix on wind time series*
3. *Moved Figures 5 and 8 to the new appendix*
4. *Added Figure 14 to illustrate the influence of the circle radius*
5. *Added tables 2 and 3 to summarize correlation results*

Comments:

Review: Major Comments

Reviewer Comment: The primary conclusion of the manuscript is that SVVP provides more physically realistic vertical winds than the conventional VVP approach. However, the current analysis only demonstrates differences between retrieval methods and does not establish which solution is closer to the true atmospheric state. Therefore, the current results demonstrate that SVVP produces different retrievals from VVP, but do not yet conclusively demonstrate that SVVP is more accurate. If possible, comparisons with independent observations (e.g., nearby Na lidar) would substantially strengthen the manuscript. At a minimum, the authors should discuss the current limitations and opportunities for validating MLT vertical wind retrievals.

Response: *We submitted a companion paper addressing the above-mentioned concerns. Figure 4 presents a comparison between our retrievals and the Japanese Meteorological reanalysis (JAWARA) and a free run of SE-WACCM-X. In the revision, we will include a table showing the sensitivity of the vertical winds on the heating/cooling rates to clarify which numbers are more physically realistic. There are already two publications with a comparison of the VVP method and lidar wind measurements (Qiao et al., 2025; Ban et al., 2026).*

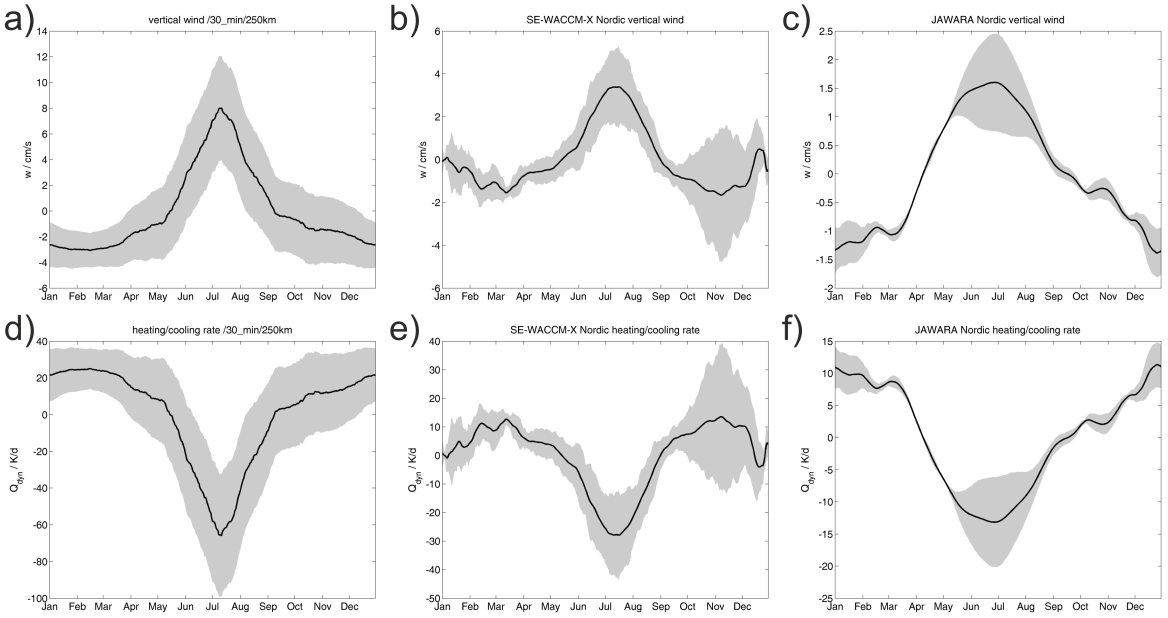


Figure 4. Vertical winds (top row) and heating/cooling rate(bottom row) for Nordic Meteor Radar Cluster with a 250 km diameter domain (left column), the free-running SE-WACCM-X (middle column), and the JAWARA reanalysis (right column).

Reviewer Comment: I strongly encourage the authors to perform a leave-one-radar-out sensitivity experiment by excluding each radar in turn and repeating the retrieval. Such an analysis would provide a quantitative assessment of the robustness of the derived vertical winds and help determine whether the results are strongly dependent on the network geometry.

Response: *Such an experiment would require substantial additional CPU time on our HPC Ubelix. The best and most robust results are obtained using all 4 systems. As soon as Tromsø or Alta are removed, the number of meteor detections drops significantly, and a robust estimate is no longer feasible. Sodankylä or Kiruna MRs are less critical, and the loss of one system is still visible, but will not lead to a major degradation of the results. On the other hand, Alta and Tromso show the largest phase disagreement and, thus, the largest position errors for the localization of individual meteors. However, this position uncertainty is still a factor of 2 lower than assumed in the non-linear error propagation. We added some sentences discussing this point.*

Reviewer Comment: The vertical wind bias correction plays a central role in the final results. However, the manuscript does not sufficiently discuss the assumptions behind this procedure. It is unclear whether the correction is derived from the entire multi-year dataset or shorter temporal intervals. Different choices may influence the resulting climatology and long-term variability, and it remains unclear to what extent the apparent improvement in vertical winds originates from the SVVP formulation itself versus the adopted bias-correction procedure. A sensitivity analysis of the retrieved vertical winds to the adopted bias-correction strategy would strengthen confidence in the conclusions.

Response: *This issue is discussed in a companion paper. The bias correction is performed using the entire data set. As the reviewer noted, shorter periods would affect the results. We will expand this section to clarify our procedure. The underlying reason for the de-biasing goes back to the textbook of Rodgers (2000). The most precise and accurate information on the long-term vertical wind is obtained by tracking the mean altitude of the detected meteors over decades (Stober et al., 2014; Dawkins et al., 2023, 2024). We found a long-term change of a maximum of 850 m over one decade. This translates into a vertical velocity of* $2.6 \cdot 10^{-6} m/s$*, which is essentially zero for one year. Thus, the mean Doppler and divergence-derived vertical wind should reflect this value at all altitudes of the meteor layer. We added a paragraph discussing this aspect in the revised manuscript.*

Reviewer Comment: As a broader perspective, the authors may consider discussing whether the method could be adapted to weather radar networks and tropospheric wind retrievals, where more independent observations are available for validation.

Response: *The SVVP and 3DVAR+DIV algorithms are applicable to all multistatic network data that provide range, time, angle of arrival, and line-of-sight velocity. Weather radar networks often perform VAD scans with overlapping volumes, which is suitable for applying both algorithms. However, very often the Doppler velocity is aliased due to the fast scanning mode. Meteorological wind lidar networks for the boundary layer could also be analyzed, although the full Earth geometry might be less relevant at such low altitudes close to the surface.*

Reviewer Comment: The manuscript concludes that radii between 200 and 250 km provide the best performance. Is there a quantitative criterion that favors one radius over the other, or are both considered equivalent within uncertainty?

Response: *This conclusion is based on Figure 2 and Figure 14. We calculated heat maps, meteor detection density plots, and individual meteor detection plots. Both radii show the best coverage for the selected reference point. Larger radii are often biased due to asymmetric sampling (almost monostatic geometry or only certain sectors are covered). Smaller radii would result in much lower detection rates and, thus, the data sparsity would cause other issues. However, both radii result in equivalent results within the error budget.*

Review: Minor Comments

Reviewer Comment: Figures 7–10 contain a substantial amount of overlapping information. The authors may consider combining these figures into one or two multi-panel figures to facilitate direct comparison between VVP, SVVP, and 3DVAR+DIV. In addition, some of the quantitative comparisons could be more effectively summarized in tables.

Response: *We will revise the Figure content and move some of the panels to the appendix to avoid redundant information. We also summarize the correlation results for mean winds and some of the higher-order parameters in a Table.*

Reviewer Comment: The manuscript would benefit from a summary table quantifying the differences among VVP, SVVP, and 3DVAR+DIV retrievals (e.g., bias, RMSD, correlation coefficients), rather than relying primarily on visual inspection of multiple figures.

Response: *We will add a table summarizing correlation results.*

Reviewer Comment: The statement that the SVVP method produces "more realistic" vertical winds may be somewhat strong given the lack of independent validation. Consider using more cautious wording such as "more physically consistent" or "more self-consistent".

Response: *Agreed. This will be changed.*

Review: Review Report

This manuscript presents a spherical formulation of the Volume Velocity Processing (VVP) technique and applies it to four years of observations from the Nordic Meteor Radar Cluster. The topic is relevant, and the mathematical formulation is carefully developed. However, the manuscript does not provide sufficient evidence to support its central claim that the spherical formulation produces more accurate or physically realistic wind retrievals, particularly for the vertical wind component. The analysis demonstrates differences between retrieval methods, but does not establish improved retrieval accuracy.

Demonstrating differences between retrieval methods is not equivalent to demonstrating improved accuracy. Consequently, several fundamental issues are insufficiently addressed and require substantial additional work before the scientific conclusions can be considered adequately supported.

General Reply: *We thank the reviewer for the assessment of the publication and his comments. The manuscript includes a supplement with 49 Figures to outline the sensitivity of the retrieval to different radii and temporal resolutions. We added the leave-one-station-out experiment with a reduced analysis to the reply. To avoid redundancy in the supplement, we will not show the data in the manuscript, but add it as Figure 51 to the supplement. The main narrative of the paper is the differences between planar geometry and the spherical implementation of the higher-order kinematic terms. As the data curation and all retrieval steps are identical between both implementations and only the fitting function was adapted, our results sufficiently demonstrate the impact of the geometry on the quantities. Furthermore, we included full non-linear error propagation and also performed various analysis experiments with synthetic data (Stober et al., 2021a, 2022). Our conclusion related to the vertical wind is based on the results from SABER trace gas observations presented in Yue and Wang (2025a). We will weaken otherwise the conclusions to avoid overly strict claims on the reliability. However, the basic question related to spherical or WGS84 approximation was already discussed in Stober et al. (2021b, 2018) and is widely accepted. Here, it is just implemented for volume quantities. The manuscript is revised according to all reviewers' suggestions. Please find point-by-point replies to the raised concerns in the attached PDF. The revised manuscript is prepared with latexdiff.*

Comments:

Review: Major Comments

Reviewer Comment: 1. Lack of rigorous validation

The primary weakness of the manuscript is the absence of a validation framework capable of assessing retrieval accuracy. The manuscript repeatedly concludes that the spherical formulation yields more realistic vertical winds and reduces biases present in conventional VVP. However, these conclusions are based on comparisons between retrieval methods rather than comparisons against a known reference. Agreement or disagreement with VVP or 3DVAR+DIV does not establish which solution is closer to the true atmospheric state.

Response: *The main narrative of the publication is to highlight the importance of spherical geometry compared to planar geometry for the volume quantities of horizontal divergence, relative vorticity, stretching, and shearing deformation. Considering that Earth is often represented as a sphere in GCMs, it is natural that this should be considered in observations and inferred quantities. We present a detailed comparison of the horizontal winds, which agree with previous studies and reflect the currently accepted climatological behaviour. Our conclusion is based on a more realistic physical representation of the geometry compared to the previous version. In particular, the horizontal divergence results in a much more realistic seasonal pattern. We clarified in the revision to point out that VVP and SVVP are calculated on identical data curation pipelines, which also means that synthetic data will not change the results regarding these differences. Concerning the vertical winds, we explicitly state in the revision that the SVVP vertical wind velocities show a better agreement with trace gas measurements from SABER (Yue and Wang, 2025a) to underline the improvement.*

Reviewer Comment: If independent observations are unavailable (e.g., rockets, MST radars, and/or satellites), the authors should perform validation using realistic synthetic atmospheric fields generated from direct numerical simulations or global circulation models. Such approaches are standard in the atmospheric remote sensing community and allow retrieval biases, uncertainties, and resolution limits to be quantified under controlled conditions. Examples include the Observing System Simulation Experiments (OSSE) framework described by Cucurull et al. (2024), which provides a systematic methodology for retrieval validation, and its application to MLT wind retrievals by Urco et al. (2024).

Response: *Zonal and meridional winds have been evaluated within the OSSE framework very recently (Hsu et al., 2026). MR winds are indeed improving the model skill when assimilated. The higher-order kinematic terms derived in this study are more of a diagnostic quantity and hold only as long as the linear approximation is valid (this is explicitly stated in the revision). Urco et al. (2024) present an Informed Machine learning approach. The resulting cost function is very similar to the 3DVAR+DIV with the generalized Tikhonov regularization. Just the way the regularization terms are derived differs. However, the paper also outlines the limitations when synthetic data is used for the performance evaluation of retrievals. The manuscript presents a single case study of 24 hours, and equations (16) to (21) refer to Cartesian coordinates; it remains unclear in which geometry the cost function is actually implemented and whether spherical geometry was used for the volume quantities used in the neuronal network.*

Figure 14/15 (Hunga-Tonga case) shows entirely different structures compared to the DNS simulations and the synthetic meteor radar data. It's also unexpected that the vertical wavelength is rather short in the vertical cross-section (other observations indicated a very long vertical wavelength of the HTHH GW), but very consistent in the latitudinal section. There is also a clear offset in the daily mean vertical winds of 1-2 m/s, which translates to a mean upwelling of 129.6 km in 24 hours. This outlines that synthetic data mainly helps to verify the mathematical implementation of an algorithm, but still generates open questions when applied to real data. Furthermore, there are low-latitude observations in which the HTHH wave was clearly in the transonic flow regime, and compressibility effects played a major role (Qiao et al., 2025). It appears to be an unexpected choice to select such a case as a reference to test an incompressible model.

Otherwise, the method is rather similar to the 3DVAR+DIV approach (non-stationary compressible solution). On the other

hand, the incompressible Boussinesq approximation has additional assumptions that are not constrained by the data. A rigorous validation would imply that all variables are cross-compared. However, the paper confirms previous results related to the 3DVAR, 3DVAR+DIV, and SVVP.

Reviewer Comment: Such experiments would allow the authors to quantify retrieval bias, random error, uncertainty propagation, and the effective spatial and temporal resolution of the proposed method.

Response: *Such experiments have been conducted before (Stober et al., 2021a) to validate the Reynolds-stresses. This was further verified in Lieberman et al. (2025) for a Quasi-two-day wave study. Later, we used synthetic data to derive Tikhonov regularizations (similar to Urco et al. (2024), but with a different approach) to get more realistic wind fields, including vertical winds. We also compared the vertical winds for seasonal data between the retrieval and ICON-UA (Stober et al., 2022). This regularization also includes corrections for the scattering geometry, which are not mentioned or included in other simulation studies. However, this results in a major issue when applying the retrieval to synthetic data. Equation (1) in Urco et al., 2024 assumes isotropic point scattering, as the Bragg vector k denotes the movement of the scattering center (specular point), which is not identical to the line-of-sight velocity for transverse-scatter meteor radars). In the past, this was addressed by implicitly neglecting the vertical velocity in the cost function, corresponding to a Tikhonov regularization with $||\infty||$ for the vertical component. Such a difference leads to very good agreement when synthetic data is used, but causes deviations when applied to real observations. We decided to investigate the geometric effect by using real data and only replacing the fitting function. The key Figures of the paper would remain unaffected by using synthetic data, but one could introduce arbitrary climatologies. Simulating compressibility effects and dissipation is beyond current availability. We have a simulation of the scattering effect (digital twin), but it requires more work to be applicable for such studies.*

Reviewer Comment: More fundamentally, the manuscript does not define an objective performance metric by which the proposed method can be judged superior to conventional VVP. Without a quantitative success criterion, the interpretation of "improvement" remains subjective.

Validation deferred to a companion paper or future publication cannot be used to support conclusions made in the present manuscript. The proposed method requires validation within the manuscript itself.

Response: *This study is designed to demonstrate the impact of the geometric implementation between the two Volume Velocity Processing algorithms. The focus of the VVP vs. SVVP comparison is designed to only outline the impact of the geometry. All other aspects of both algorithms are identical. We also show examples of residuals in the revised manuscript. The vertical winds are qualitative compared to the SABER CO2 trace gas measurements published in Yue and Wang (2025a). We did not claim a superiority of one algorithm over another. We conclude that there are differences, and qualitative comparisons indicate better agreement for the SVVP relative to the VVP implementation.*

Reviewer Comment: 2. Failure to demonstrate that spherical geometry is a dominant source of error

The central premise of the manuscript is that Earth curvature introduces significant biases in VVP retrievals and that these biases

are mitigated by the spherical formulation. However, the manuscript does not quantify the relative importance of geometric effects compared to other error sources.

Potential contributors include measurement uncertainty, meteor sampling variability, network geometry, temporal averaging, regularization choices, vertical-wind bias correction, and inversion assumptions. The current analysis, therefore, demonstrates that SVVP produces different retrievals than conventional VVP, but does not demonstrate that these differences arise primarily from the spherical representation.

The manuscript currently assumes that curvature effects are responsible for the observed differences, but this assumption is never tested quantitatively.

A quantitative error budget is needed to establish whether curvature effects are indeed a dominant source of retrieval error. Without such analysis, the attribution of the observed differences to spherical geometry remains largely qualitative.

Response: *We disagree with the reviewer. The comparison between both geometries is designed only to be affected by the spherical geometry for the volume quantities (horizontal divergence, relative vorticity, stretching, and shearing deformation). Both analyses leverage identical data curation pipelines, which include all filters, geometric WGS84 corrections for each meteor, the non-linear error propagation, the number of iterations, and all other aspects, such as the SVD solver or circle of influence. Only the fitting function is changed between the two versions. We are not aware that we can more clearly demonstrate the effect of the geometry on the volume quantities. The geometric comparison will not be different when applied to synthetic data. The main reason for considering SVVP to be more reliable is the vertical profiles of the horizontal divergence. The VVP results in an all-year convergence above 95 km. This appears to be unlikely.*

Reviewer Comment: 3. Insufficient sensitivity and robustness analysis

The robustness of the proposed retrieval method is insufficiently characterized. Several sensitivity analyses are required. In particular, a leave-one-radar-out experiment would provide valuable information regarding the dependence of the retrieved winds on network geometry and station availability. The argument that removing major stations degrades the retrieval is not a justification for omitting the experiment. On the contrary, quantifying the degradation is precisely the purpose of such a sensitivity study.

Similarly, robustness could be assessed through leave-random-measurements-out experiments, where a fraction of the meteor detections (e.g., 10–50%) is removed and the retrieval repeated. Such analyses would help quantify the stability of the inferred winds and their sensitivity to sampling density.

Response: *The supplementary material includes 49 Figures, presenting all correlations for all time resolutions and radii of influence. These Figures present a very detailed sensitivity study for all retrievals used in the manuscript. Furthermore, Figure 3 in the paper shows the diurnal cycle, which is identical to the leave out 50% of the data from the retrieval as part of the diurnal cycle. We show the different resolutions to exactly address this issue. The 15-minute temporal resolution uses only 25% of the data compared to the 60-minute retrieval. However, for the sake of completeness, we have performed a leave-one-station-out experiment for the VVP and SVVP. The comparison is found below in the Figure 1 and as Figure S51 in the*

supplementary material.

Reviewer Comment: The vertical-wind bias correction also plays a central role in the manuscript's conclusions. However, no systematic sensitivity analysis is presented to determine how strongly the results depend on the adopted bias-correction strategy. It remains unclear how much of the apparent improvement originates from the SVVP formulation itself and how much originates from the subsequent correction procedure.

Furthermore, the need for an externally imposed bias correction raises additional questions regarding the robustness of the vertical wind retrieval itself. If the proposed formulation substantially reduces geometric biases, the manuscript should clarify why a subsequent empirical correction remains necessary and quantify its impact on the reported results.

Response: *The most accurate and precise measurement of the vertical wind with meteor radars is obtained by tracking the peak altitude of the meteor detections. This peak altitude follows a constant density surface, and the width of the meteor layer indicates the stratification. Previous studies have monitored the long-term changes of the meteor layer (Stober et al., 2014; Dawkins et al., 2023, 2024). Based on these studies, the maximum change in altitude of the meteor layer was 850 m per decade. This translates into a vertical wind of approx.* $2.6 \cdot 1'^{-6} m/s$*, which is essentially negligible. A perfect system should observe this mean velocity in the Doppler or divergence-derived vertical velocity. Thus, we calculated over 5 years a mean bias value for each altitude, which is subsequently subtracted from the data. The nature of the bias is likely related to tiny pointing offsets of the system. These radars were installed almost two decades ago, and at this time, no one ever thought about absolute pointing accuracy. Such bias corrections are common in remote sensing and often are instrument-related. We expanded this section and added these details to the revised manuscript. We also compared the bias correction for the climatology and obtained the same numbers as for the 5-year data. The magnitude of the vertical winds as well as the seasonal behaviour is discussed in the context of trace gas measurements presented in Yue and Wang (2025a).*

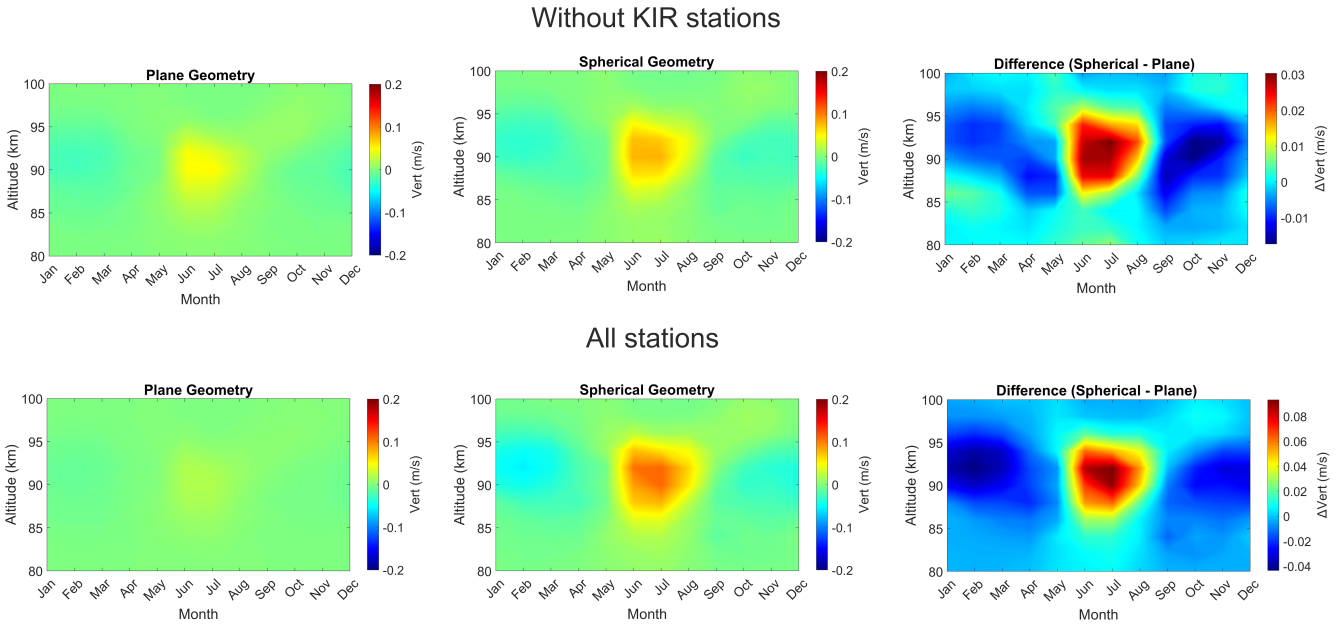


Figure 5. Monthly mean vertical wind components retrieved using the Volume Velocity Processing (VVP), showing the spherical geometry, plane geometry, and their corresponding differences for the experiment with all stations (bottom row) and without the KIR station (top row).

Reviewer Comment: 4. Lack of uncertainty characterization and scale-dependent analysis

The manuscript does not adequately characterize retrieval uncertainty, effective resolution, or recoverable scales.

Many conclusions are based on differences in vertical wind magnitude, smoothness, divergence, and temporal variability. However, the manuscript does not establish which spatial and temporal scales can be reliably reconstructed by either method. Consequently, it is difficult to determine whether the reported differences reflect improved physical reconstruction, changes in effective filtering, or different regularization behavior.

In particular, the manuscript does not establish the smallest recoverable scales, the scales dominated by regularization, or the scales for which the retrieval becomes unreliable.

Several issues raised by the analysis—including the interpretation of vertical-wind amplitudes, the dependence on temporal averaging, the selection of the optimal domain radius, and the comparison with 3DVAR+DIV—ultimately require a quantitative assessment of retrieval uncertainty and resolution. Without such information, the physical interpretation of the results remains ambiguous.

Response: *The manuscript presents long-term observations (5-years) and climatologies of mean winds and higher-order kinematic terms. The SVVP algorithm is meant to provide a much less computationally costly approach to derive mean flow properties. The main goals are to infer seasonal patterns and the climatological behaviour. Vertical winds are not the main narrative, but have been mentioned to discuss previous results. All small scales, including atmospheric tides and gravity waves, are filtered through the ASF2D (Baumgarten and Stober, 2019; Stober et al., 2020). Furthermore, only daily mean values are*

used to calculate the climatologies and long-term time series. This bandpass filter essentially removes all small scales and keeps only the mean flow and planetary waves. The manuscript does not make any claims about small scales or resolvable waves. Small-scale features will remain the key aspect for the more advanced retrievals, such as 3DVAR, 3DVAR+DIV, or sub-versions of these approaches, including machine learning techniques. The seasonal patterns are reproduced for all resolutions within the error margins. The error scales with $1/\sqrt{(n)}$*. Where* n *is the number of independent measurements. In this paper, we have no intention to investigate small-scale gravity waves or tides. The 3DVAR+DIV algorithm and also the standard retrievals have proven to reproduce the Hunga Tonga gravity wave and its global propagation (Stober et al., 2023, 2024; Vadas et al., 2023b, a). However, this is not a topic here and beyond the scope of the paper. We further illustrate in the revised manuscript the error propagation by showing examples of residual fits with error bars for each meteor within one time-altitude bin for planar and spherical geometry.*

Review: Review Report

This manuscript presents a useful methodological contribution to multistatic meteor radar wind retrievals by reformulating Volume Velocity Processing in spherical coordinates. The topic is timely and relevant for Atmospheric Measurement Techniques, since multistatic meteor radar networks now cover domains of several hundred kilometers, where plane-geometry assumptions may introduce projection errors. The multi-year Nordic Meteor Radar Cluster observations and the comparison among plane-geometry VVP, spherical VVP, and 3DVAR+DIV provide a valuable basis for assessing geometric effects on retrieved MLT winds and higher-order kinematic quantities.

However, I think the manuscript needs substantial revision before publication. My main concern is not the usefulness of the spherical formulation, but the interpretation and positioning of the method. The discussion sometimes gives the impression that SVVP is a more reliable alternative to 3DVAR+DIV, especially for vertical winds. This is not sufficiently justified. SVVP is an important geometric improvement of VVP, but it remains a reduced-order, first-order representation of the wind field. By contrast, 3DVAR+DIV is theoretically a more general tomographic framework that can resolve spatially heterogeneous three-dimensional winds and include physical constraints. Therefore, the differences between SVVP and 3DVAR+DIV should not be interpreted simply as evidence that SVVP is superior. They may instead indicate limitations in the present 3DVAR+DIV implementation, including geometry, continuity constraints, regularization, and sparse-sampling treatment.

I recommend major revision. The paper has clear potential, but the conclusions should be more balanced, and the retrieval uncertainty, validation, and method comparison need to be strengthened.

General Reply: *We thank the reviewers for a constructive review of the submitted manuscript. The developed SVVP complements the more advanced 3DVAR+DIV retrieval method. The main goal was to develop a computationally less expensive retrieval method that uses a multistatic geometry and infers higher-order kinematic terms that are not accessible with monostatic retrievals. The new algorithm targets climatological studies and longer-time series analysis.*

We appreciate the review and will revise the manuscript according to the suggestions, clarifying some aspects pointed out by the reviewer. We have performed a reduced run to store the condition numbers of the SVD singular values. We added some residual plots to demonstrate how planar and spherical geometry differ, and also found cases where the first-order approximation is no longer sufficient to underpin the importance of the 3DVAR+DIV method. Below, we provide point-by-point replies in the attached PDF. The revised manuscript is prepared with Latexdiff track changes.

Comments:

Review: Major Comments

Reviewer Comment: The manuscript should more clearly distinguish between the improvement of SVVP relative to plane-geometry VVP, the broader theoretical capability of 3DVAR+DIV, and the limitations of the current 3DVAR+DIV implementation. SVVP is a geometrically improved version of VVP, but it does not remove the reduced-order nature of VVP, which still represents the wind field using mean winds and first-order spatial gradients within a finite volume. In contrast, 3DVAR+DIV is designed to retrieve spatially varying three-dimensional wind fields with physical constraints. The manuscript should therefore avoid implying that SVVP supersedes 3DVAR+DIV. The larger vertical winds from 3DVAR+DIV should be discussed in terms of spherical/ellipsoidal geometry, continuity assumptions, Tikhonov regularization, and sparse meteor sampling. A better framing is that SVVP provides a geometrically self-consistent benchmark for reduced-order retrievals, while an improved 3DVAR+DIV remains the more general framework for future multistatic meteor radar tomography.

Response: *The developed SVVP provides a computationally less costly alternative to infer mean winds, including seasonal vertical wind variability and higher-order kinematic terms such as horizontal divergence, relative vorticity, stretching, and shearing deformation. The algorithm is supposed to complement the much more advanced 3DVAR+DIV retrieval, which can deal with arbitrary wind fields. We will explicitly refer to this in the revision. It was not our intention to generate the impression that the new analysis is better than the 3DVAR+DIV. This will be made clearer in the revised draft.*

Reviewer Comment: The observational comparison shows that SVVP, plane VVP, and 3DVAR+DIV produce different results, especially for vertical winds and higher-order kinematic quantities. However, this does not fully isolate the role of spherical geometry. I recommend adding a synthetic or closed-loop retrieval experiment. The authors could prescribe known wind fields, generate synthetic line-of-sight velocities using the actual NORDIC radar geometry and meteor sampling locations, and then retrieve the winds using both plane VVP and SVVP. Test cases could include uniform horizontal wind with zero vertical wind, known divergence/vorticity, prescribed vertical velocity, and simple gravity-wave-like perturbations. Such experiments would directly show whether SVVP reduces projection errors and whether plane VVP produces spurious vertical wind or divergence.

Response: *The comparison between SVVP and the planar geometry targets to demonstrate only the effect of the geometry. Both algorithms are embedded in the same script with identical data curation, non-linear error propagation, all quality control filters, and a singular value decomposition solver. Hence, the remaining differences are solely attributable to the geometric implementation of the fits. This will not change by applying it to synthetic data. The 3DVAR+DIV retrieval is designed to resolve arbitrary wind fields and small-scale gravity wave fluctuations. However, the implementation of the continuity equations resulting in vertical winds depends on a lower or upper boundary for vertical integration. This limits the applicability for*

seasonal comparisons, as the underlying assumption is already biased. Synthetic data was already used in Stober et al. (2022). The algorithm is reliable for what is presented in the above-mentioned manuscript.

Reviewer Comment: The paper retrieves not only mean winds but also horizontal divergence, relative vorticity, stretching deformation, and shearing deformation. These quantities are sensitive to sampling geometry, station distribution, and the conditioning of the least-squares problem. The manuscript mainly presents retrieved fields and correlations, but it does not sufficiently show whether all fitted parameters are well constrained. The authors should include diagnostics such as condition numbers, singular values, parameter covariance, and correlations among retrieved coefficients. This is particularly important for vertical wind and gradient terms. The contribution of each radar station should also be quantified, since uneven spatial overlap and different meteor counts may cause the retrieval to be dominated by certain stations or viewing directions.

Response: *The reviewer mentions an important point. In the supplementary material, we present 49 Figures showing all temporal resolutions and radii and the corresponding correlations to address this aspect. The different temporal resolutions correspond to different data sparsities. As this point was brought up by other reviewers, we also performed a leave-one-station out experiment (see Figure 6, added to supplementary material as Figure S51).*

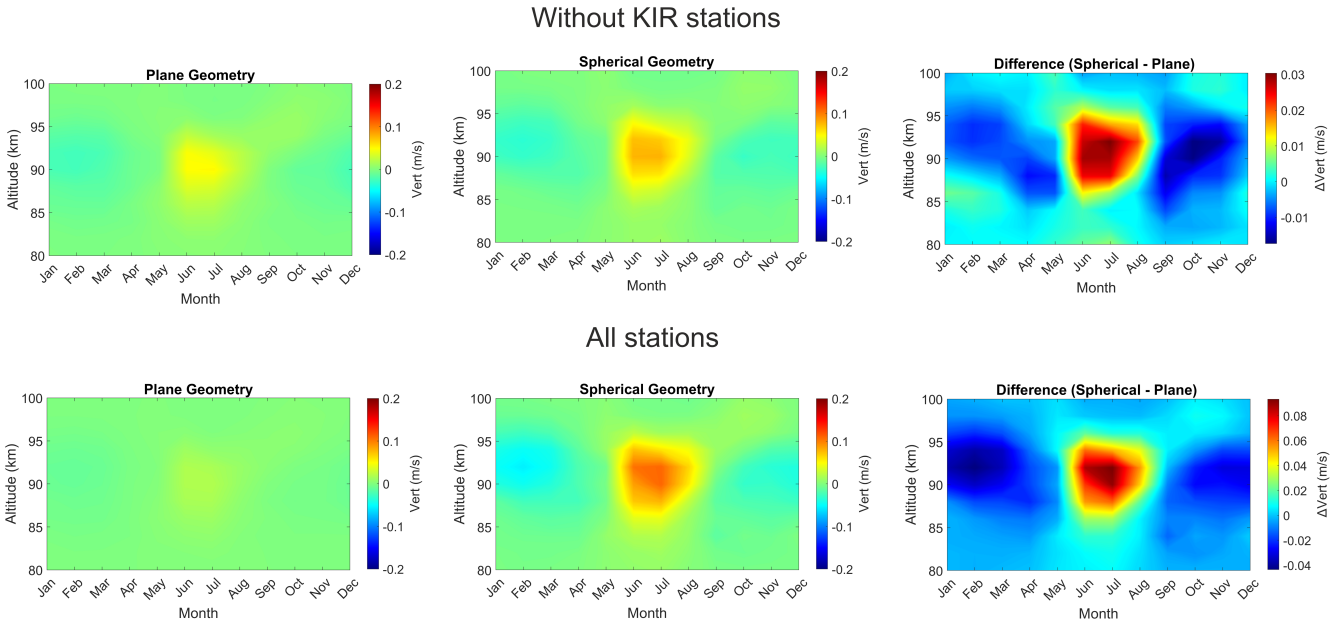


Figure 6. Monthly mean vertical wind components retrieved using the Volume Velocity Processing (VVP), showing the spherical geometry, plane geometry, and their corresponding differences for the experiment with all stations (bottom row) and without the KIR station (top row).

We also performed an additional run and stored condition numbers and singular value thresholds of 10^{-4} *and* 10^{-6}*. This had basically no impact on the significant digits given by the non-linear error propagation. The spatial sampling appears to be less critical as long as at least three systems form a triangle shape. Tromso and Alta have higher data rates than Sodankylä or Kiruna, but due to the long distance, a few measurements there are sufficient to mitigate most sampling biases in the mean quantities. A binary system of meteor radars suffers from sampling bias much more than tristatic or higher-order networks. The results are robust within the total statistics. When the problem becomes too sparse, our solver tends to produce zero vertical winds. This is the case at the upper and lower edges of the meteor layer. We added in the revision a meteor count density plot to illustrate the distribution of the number of detections. We also performed a reduced run and added the condition number to the stored data* $k(A) = \frac{\max \sigma_A}{\min \sigma_A}$*. A potential ill-conditioning is cured due to the spatio-temporal Laplace filter, which estimates a certain smoothness between neighbouring grid cells in altitude and time. For good least square fits (well-conditioned), the impact is negligible. The condition number ranges from log10(k(A)) 2.4 to 3.8, indicating a sufficient numerical stability for double precision. The minimum number of meteors for a fit is 15, but we only use data with more than 50 meteors.*

Reviewer Comment: The manuscript subtracts a long-term or seasonal vertical-wind bias based on the assumption that the long-term mean vertical wind should vanish. This correction is critical because its magnitude is comparable to the retrieved vertical winds. The authors should describe how the bias is estimated in much greater detail. It should be clarified whether the correction is derived separately by altitude, radius, temporal resolution, season, station combination, or from the full dataset. The authors should also test whether the seasonal upwelling/downwelling pattern remains under different bias-correction strategies. The uncertainty associated with this correction should be propagated into the final vertical-wind climatology.

Response: *We revised the manuscript and added a detailed description of the bias removal. The bias is estimated for each altitude using the full 5-years or the climatology. Both methods result in almost identical bias values for the Doppler-based vertical winds. The largest biases are observed at the edges of the meteor layer, and there is a scaling behaviour with the magnitude of the Doppler-derived vertical winds. The order of the bias ranges from a few cm/s to about 10 cm/s. For very large radii and long averaging, the assumption of the linear wind field approximation seems to be no longer valid and results in even larger biases.*

Reviewer Comment: The comparison between SVVP and 3DVAR+DIV needs a more careful interpretation. Since 3DVAR+DIV is a more general framework, the comparison should not simply emphasize that SVVP gives smaller or more physically plausible vertical winds. The authors should analyze why the two methods differ. First, the current 3DVAR+DIV implementation may not include the same spherical geometric treatment as SVVP. Second, vertical wind in 3DVAR+DIV is constrained differently from the Doppler-derived vertical wind in SVVP. Third, the influence of Tikhonov regularization should be quantified

more systematically, rather than illustrated mainly with selected cases. This would help clarify whether the discrepancy reflects method hierarchy, geometry, physical assumptions, sampling sparsity, or regularization choices.

Response: *This is an important comment, and we will revise the discussion accordingly. The first two points will be discussed as recommended. We already expanded the discussion about the Tikhonov regularization and its impact on the results. We found a rather wide plateau ranging from 0.4 to about 5 for the regularization strength, which led to very similar wind field solutions. Our best choice, so far, has been optimized with the Hunga Tonga gravity wave. The Lagrange multiplier of 150 was used to illustrate that, in principle, the smoothness constraint could enforce solutions very similar to the VVP or SVVP. However, this results in the loss of small-scale features of the arbitrary wind fields and, thus, minimizes the benefits of the 3DVAR+DIV retrieval. Furthermore, the divergence-derived vertical wind starts to be systematically deflated.*

Reviewer Comment: The manuscript discusses the relation between horizontal divergence and vertical velocity using a simplified continuity equation. However, the MLT is strongly density-stratified, and an incompressible continuity equation may not be appropriate without qualification. An anelastic form involving density stratification may be more relevant. Even if the main SVVP climatology is based on Doppler-derived vertical velocity, divergence, and 3DVAR+DIV results are still used to support the interpretation of vertical motion. The authors should clarify how the continuity constraint is used in 3DVAR+DIV, whether density stratification is included, and how this affects the comparison with SVVP vertical winds.

Response: *We thank the reviewer for bringing this up. This is indeed a major difference in the vertical wind retrievals between SVVP (Doppler) and 3DVAR+DIV. We have estimated how compressible small-scale gravity waves could become using linear theory (Stober et al., 2022). We have estimated isentropic Mach numbers and found often values above Mach 0.3. Flows exceeding this threshold are considered compressible (the transition is not a step function). However, Mach numbers beyond Mach 0.5 and occasionally Mach 0.7 have been observed (Qiao et al., 2025). The stratification comes on top of that. The vertical contribution of $u\nabla\rho$ with $\rho = \rho_0 \exp -z/H$ is proportional to $-w/H$. Considering the magnitude difference between the scale height H and a typical velocity w, it contributes, but is not yet a leading-order effect compared to the horizontal compressibility effects. We plan to revise the retrievals to account for all compressibility effects. The horizontal gradients in the density also depend on temperature, which is not derivable from the data with the current systems without further assumptions. Compressibility effects are the main reason why we did not include the divergence-based wind in this manuscript, which represents more of an upper limit for vertical velocities.*

Reviewer Comment: The SVVP method still assumes that the wind field within the analysis volume can be represented by a first-order expansion. This may be restrictive over 200–400 km domains, where gravity waves, tides, planetary waves, and polar-region dynamics can produce nonlinear spatial structures. The manuscript should better quantify the representation error associated with this first-order approximation. For example, the authors could show line-of-sight residuals as a function of radius, altitude, local time, or season. Synthetic tests using sinusoidal or gravity-wave-like wind fields would also help identify

the spatial scales over which SVVP remains valid. This is important because some SVVP–VVP differences may arise from model truncation errors, not only from geometric effects.

Response: *Thanks for the suggestion with the residuals. We have looked at thousands of residual plots to check proper fitting. Below you find some examples. We show some representative examples and one case where the first-order approximation starts to break down. We have added a few examples below. We did not find a case that shows anomalous distributions of the residuals indicating a misfit. Concerning the differences between VVP and SVVP, this will have no impact as both analysis algorithms have the same data curation, including the circles of influence.*

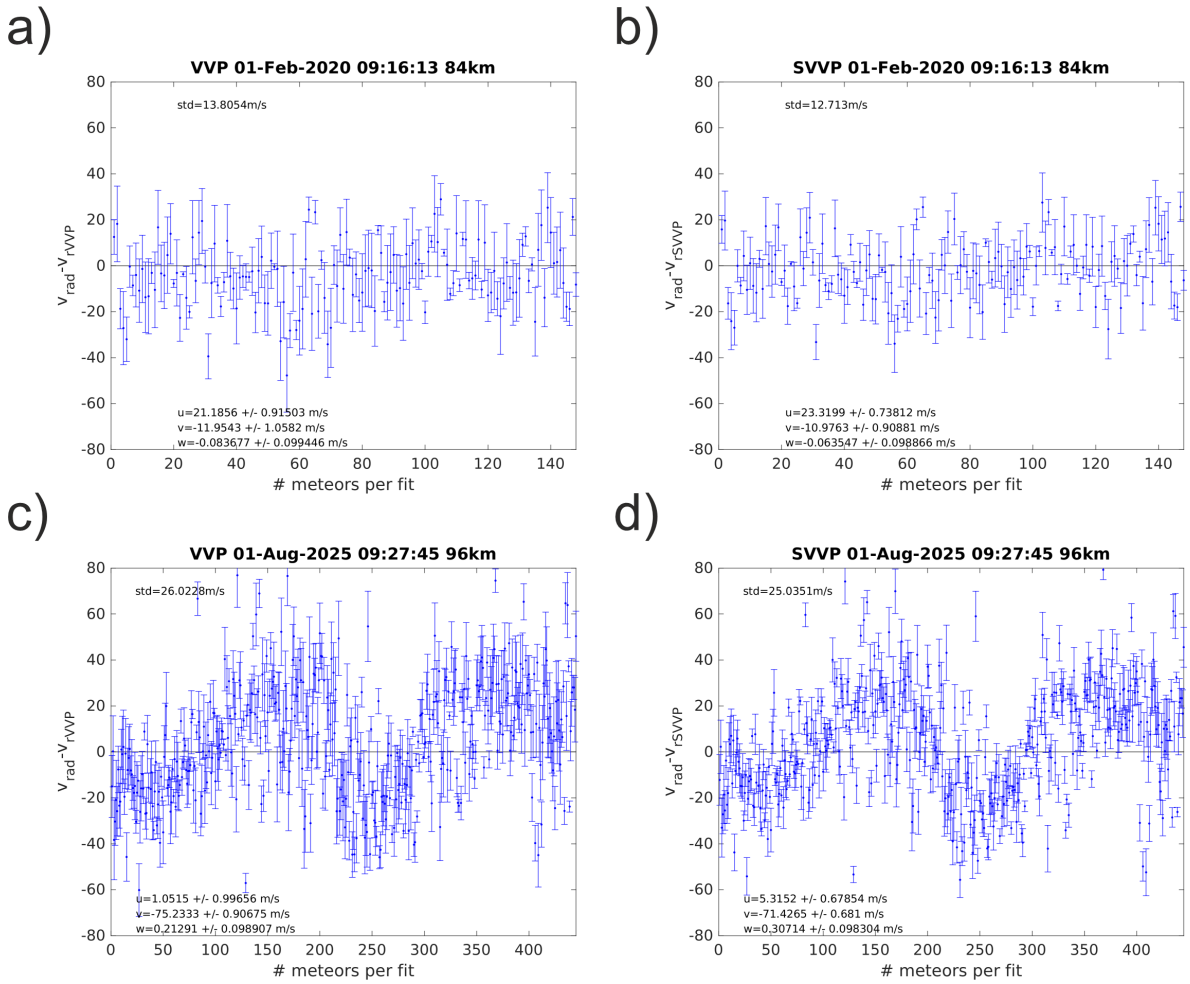


Figure 7. Fitting residuals for planar and spherical geometries for individual fits. The x-axis shows the number of meteor detections for this time-altitude bin. This is equivalent to time, but does not represent constant time steps. Panels a) and b) compare planar and spherical geometry for a typical scenario. Panels c) and d) highlight a case where the first-order approximation fails, and the residuals indicate an almost sinusoidal oscillation around the zero line.

We added Figure 7 to the revised manuscript to showcase some examples outlining the mentioned effects. Panels a) and b) show a more typical example of the residuals obtained with the VVP and SVVP for the same time-altitude bin. Panels c) and d) illustrate a case where the assumption of the first-order approximation is no longer valid, in the sense that the residuals show an oscillation. This oscillation is due to the long averaging and is indicative of a transient event moving through the domain (e.g., GWs).

Reviewer Comment: The manuscript concludes that reliable vertical winds are obtained for 15–30 min temporal resolution and a 200–250 km spatial radius. This is plausible, but the selection criterion is not sufficiently quantitative. The authors should state whether the optimal range is based on meteor counts, radar overlap, retrieval uncertainty, condition number, residual error, comparison with 3DVAR+DIV, or physical plausibility. These criteria are not equivalent. A larger radius may improve sampling but worsen the first-order approximation, while a smaller radius may reduce representation error but degrade conditioning. A summary table or figure showing these metrics as a function of radius and temporal resolution would make the conclusion more convincing.

Response: *We have added Figure 13, presenting measurement response, and Figure 14, presenting daily meteor count (30-minute resolution) to illustrate optimal coverage for the selected reference point in the revised manuscript. These plots show the ring coverage (radius 200:50:400), justifying the circle of influence. We have added similar meteor count density Figures for both 15-minute and 60-minute intervals in the supplementary material as Figure S50.*

Reviewer Comment: Because the paper focuses on geometric implementation, the definition of the vertical direction must be precise. The manuscript should state whether the retrieved vertical wind is relative to the geocentric radial direction, the geodetic normal, the local ENU up direction at each meteor location, or the reference point. Over a 200–400 km domain, local vertical directions are not parallel, and small projection differences may matter for cm s^1 vertical winds. The authors should explain how line-of-sight velocities measured in local radar coordinates are transformed into the common retrieval coordinate system, and whether residual differences in local vertical directions can project horizontal wind into the retrieved vertical component.

Response: *We will revise the definition of the vertical coordinate in the manuscript to explicitly define the directions. The vertical winds are representative of the volume segment spanned around the reference point following a constant altitude surface provided by the WGS84 ellipsoid. The value is given in ENU for the reference coordinate. Each meteor enters the retrieval corrected for its local ENU projection. This was already implemented in Zeng et al. (2024). We added that the volume quantities are now also represented with the spherical volume correction relative to the reference point. This also represents a limitation for very large domains, as the approximations start to become imprecise, resulting in an inflation of the vertical velocity. Considering that the volume quantities are derived using the mean WGS84 parameters of the reference point, when*

the domain becomes too large, they are again affected by the projection residuals arising between the Earth ellipsoid shape and a sphere. Generalized elliptical coordinates might be more universal, but it's not clear whether it would be desirable to describe very large domains with just means and 4 kinematic terms. The internal variability might be too large, and the linear approximation is no longer holding.

Review: Review Report

This paper proposes a new Volume Velocity Processing method in spherical coordinates to improve the wind retrieval in MLT. Its performance is compared against the conventional VVP method in plane geometry and the 3DVAR+DIV method, which result shows that the proposed SVVP method generally resolves more variability than the conventional VVP, and provides a more robust framework than 3DVAR+DIV for the study of MLT dynamics in terms of longer time series. The manuscript is generally well written. My main concerns are as follows:

General Reply: *We thank the reviewer for the review of the submitted manuscript. The major contribution of the manuscript is the implementation of the VVP in spherical coordinates as SVVP, addressing geometric limitations of the conventional planar VVP approach for wind retrieval in the MLT region. We have revised the manuscript, addressing the concerns in detail below. We have added a figure presenting the error distribution for VVP and SVVP, which demonstrates the improvement in retrieval error achieved by SVVP. Furthermore, we revised the 3DVAR+DIV section concerning the vertical wind to emphasize the difference between instantaneous spatially resolved vertical winds and climatological patterns inferred from SVVP and VVP. Below, we provide point-by-point replies in the attached PDF. The revised manuscript is prepared with Latexdiff track changes.*

Comments:

Review: Major Comments

Reviewer Comment: The caption of Table 1 should appear on top of the table. Moreover, it would be nice to see the relevant parameters of the radar antenna beam as well.

Response: *We thank the reviewer for pointing this out. We have moved the caption to the top of the table. All systems use circularly polarized Yagi antennas. We also added a column for the baseline of the Jones cross in λ.*

Reviewer Comment: The authors demonstrate the difference between SVVP and VVP clearly, notably for the retrieval of the vertical wind component. However, their superiority cannot be intuitively judged due to the lack of comparison with the 'actual' wind field.

Response: *We have submitted a companion paper in which the SVVP retrievals are validated against the Japanese Meteorological reanalysis (JAWARA) and a free run of SE-WACCM-X. The present manuscript instead focuses on geometry implementation in the SVVP and VVP differences, and on comparing SVVP with VVP and the potential to improve the 3DVAR+DIV. We only argue that the vertical climatology appears to be more reasonable compare to the VVP.*

Reviewer Comment: The comparison against 3DVAR+DIV is not straightforward, since they are two different frameworks. The advantages/disadvantages of SVVP w.r.t. 3DVAR+DIV should be highlighted.

Response: *We thank the reviewer for the comment. We have stated the underlying differences in the 3DVAR+DIV and SVVP frameworks. We revised the manuscript to clarify the advantages and disadvantages of each approach, which has been included in the revised manuscript. Furthermore, we used the opportunity to add analysis for the 3DVAR+DIv data to avoid too much misinterpretation.*

Reviewer Comment: The authors demonstrate the importance of geometric effects. The contribution of geometric effects to wind retrieval errors is not well addressed.

Response: *We thank the reviewer for raising this point. We have added figure 8 to show the contribution of geometric effects to the wind retrieval errors. It compares the per-fit error distributions of VVP and SVVP for both the zonal and meridional wind components. Both algorithms achieve almost similar error distributions after 8 iterations. Zonal winds tend to show smaller errors in the SVVP, while meridional winds show smaller errors in the VVP. However, the distributions exhibit only marginal deviations. This is understandable, as the quality of the fitted model coefficients does not depend on the geometry, but their physical values do.*

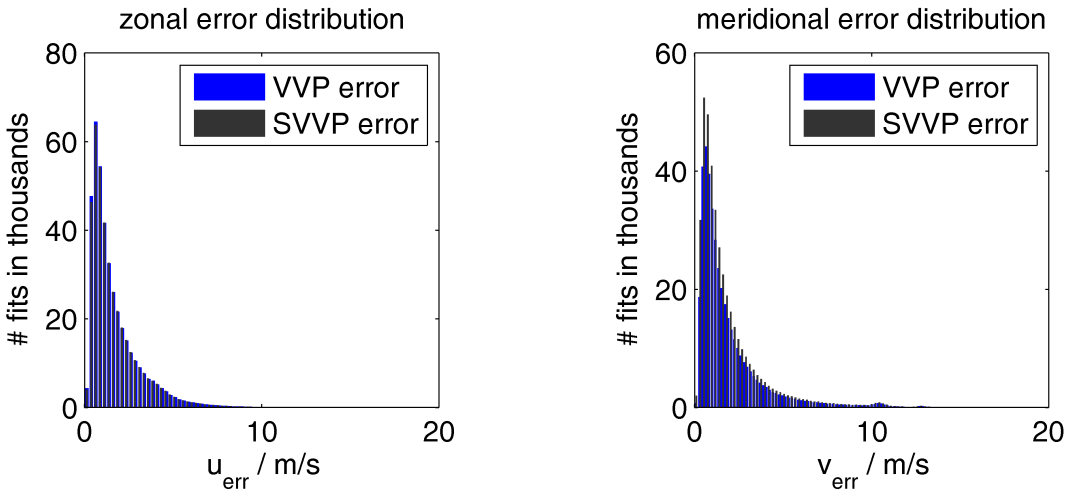


Figure 8. Comparison of fit-error for the zonal (left) and meridional (right) wind components retrieved by SVVP and VVP. The top row shows scatter plots of per-fit error for SVVP and VVP, while the bottom row shows histograms of the corresponding error distributions: VVP (blue) and SVVP (gray).

3 Review Comments and Detailed Authors: First and second CO Editor reports

Review Report

The authors have made a commendable effort to address the reviewer comments through extensive supplementary material and a detailed point-by-point response. The mathematical formulation of the spherical VVP (SVVP) remains a valuable methodological contribution, and the comparison with the plane-geometry VVP convincingly demonstrates the importance of accounting for Earth's curvature when retrieving higher-order kinematic quantities from multistatic meteor radar networks. However, after careful examination of the revised manuscript and the authors' response, I find that several critical issues raised in the original review remain insufficiently addressed. Furthermore, several methodological clarifications promised in the response do not appear to have been fully incorporated into the revised text. I recommend major revision before the paper can be accepted for publication in Atmospheric Measurement Techniques. Below, I detail specific concerns and provide suggestions for improvement.

General Reply: *We thank the reviewer for recognizing our efforts to address the previous concerns. In response, we have added an analysis comparing SVVP and VVP with JAWARA and SE-WACCM-X. We have also included vertical bias and measurement statistics, as well as station removal analyses for zonal and meridional winds and all higher-order kinematic variables. Furthermore, we now present the spectral analysis of horizontal and vertical winds, and provide error distributions for zonal and meridional winds, horizontal divergence, and high-order kinematic variables. Below, we provide point-by-point replies to each concern in the attached PDF. The revised manuscript is prepared with latexdiff.*

Major Comments

1. Validation of Vertical Winds Remains Inadequate. The central claim that SVVP yields "more physically consistent" vertical winds is still based only on inter-comparison between methods, not on independent observations or models. The SE-WACCM-X and JAWARA comparisons shown in the response document (Figure 2) should be included in the manuscript, or the conclusions must be substantially tempered. A companion paper cannot serve as the sole validation for this study.

Response: *In the revised version, we will add a simplified comparison between JAWARA and SE-WACCM-X for the vertical wind. This also means that we will add co-authors. However, a detailed discussion will be left to the companion paper, and the models will be only briefly described. In this comparison, we maintain the SE-WACCM-X and JAWARA at 80* km *and 90* km *and compare with VVP and SVVP at 80* km *to 90* km *altitude layers on the left panel and 84* km *to 94* km *altitude layers.*

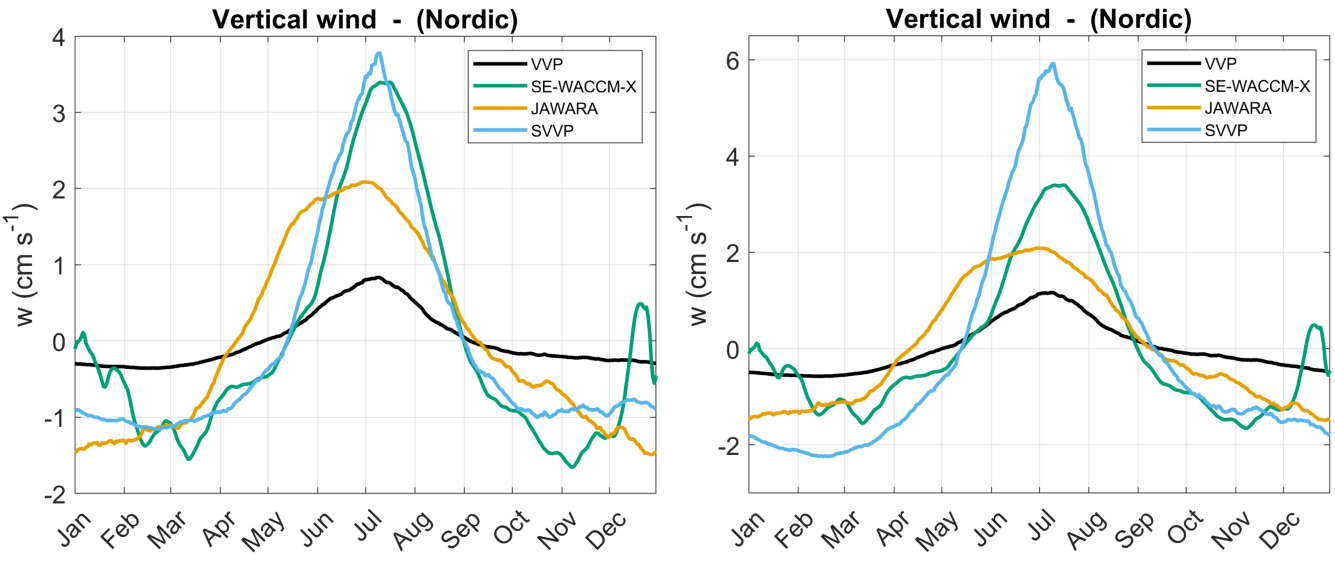


Figure 9. Vertical winds from the JAWARA reanalysis and the free-running SE-WACCM-X model averaged over the altitudes 80 km to 90 km (both panel) with VVP and SVVP analysis averaged over 80 km to 90 km (left panel) and 84 km to 94 km (right panel).

2. Vertical Wind Bias Correction Remains Insufficiently Documented. The manuscript states that a bias of 1–8 cms^{-1} is subtracted but does not provide the estimation algorithm, uncertainty quantification, or demonstration that the seasonal pattern is robust to different bias strategies. A dedicated Methods subsection with the formula, uncertainty, and justification (referencing meteor-ablation trends) is needed.

Response: *To outline the robustness of the Bias determination, we have added Figure 10 to present vertical wind bias for 15 and 30 minutes at 200 km and 250km along with the corresponding measurement statistics.*

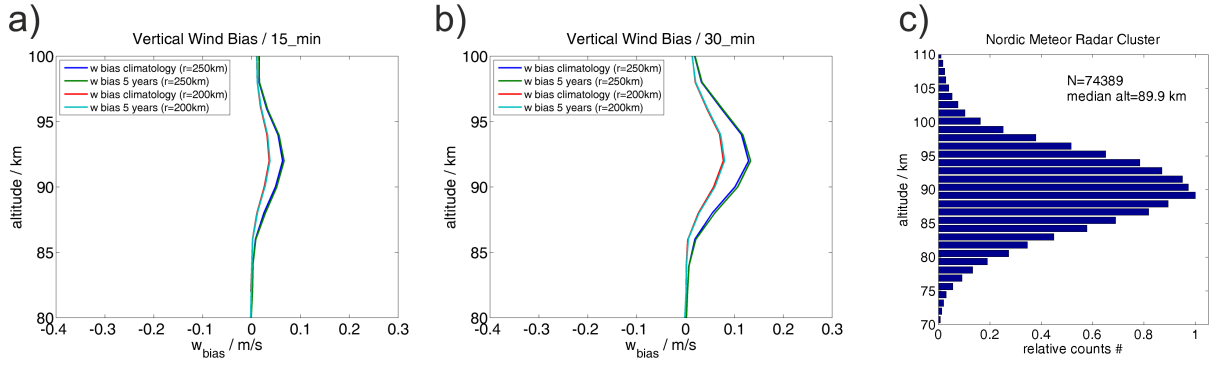


Figure 10. Vertical wind bias at 15-minute and 30-minute temporal resolutions for 200km and 250km is shown in panels a) and b), respectively. For comparison, panel c) presents the normalized altitude distribution, serving as a proxy for the measurement statistics.

This is also included in the revised manuscript. As we really only subtract a mean value for each altitude either from the time series of 5 years or the climatology, we don't believe a formula is necessary. The bias correction does not affect the seasonal pattern independent of the applied technique (see Figure below). There is only a scaling behaviour related to the temporal resolution, which is also found for the vertical winds themselves.

3. Temporal and spatial resolution sensitivity lacks quantitative support. The vertical-wind scaling with temporal resolution (Fig.8) is attributed to gravity waves, but the promised spectral analysis (response Figure 3) is absent. Similarly, the 200-250 km radius is justified indirectly via 3DVAR+DIV diagnostics; condition-number results for SVVP should be shown, or the indirect argument clearly explained.

Response: *We have added the spectral analysis as Figure 10 in the revised manuscript. Vertical winds reflect tidal peaks, and the truncation of the GW spectrum becomes visible. The spectral slope follows roughly a -5/3 law.*

4. First-order approximation errors are not quantified. The residual analysis is only illustrative; a systematic assessment of when the linear approximation fails (by radius, altitude, season) is missing. Including residual statistics or a supplementary figure would help bound the method's applicability.

Response: *We will present the fitting errors for all quantities of the instantaneous analysis of the main observables. The vertical winds (Doppler or divergence-based) are obtained using the retrieval approach with Tikhonov regularization. Therefore, the least squares returns an error distribution that is balanced between the pure least squares and the Lagrange multiplier of the regularization term. The Lagrange multiplier is about* $\lambda = 1.e-2$*. The vertical wind error is therefore given by a covariance-weighted superposition of the least squares uncertainty and the Tikhonov regularization. The vertical wind errors shown in the histogram only reflect the contribution of the least squares.*

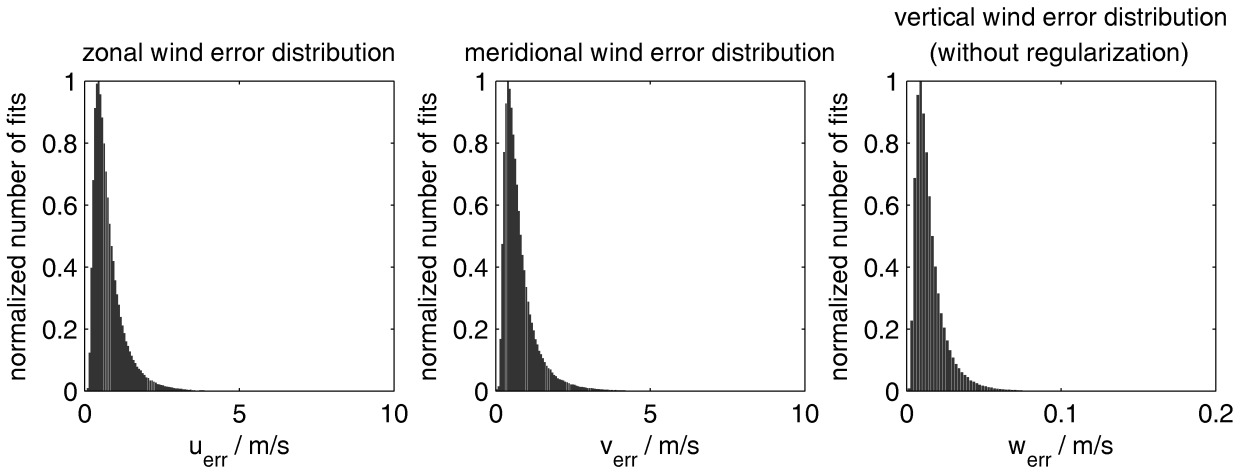


Figure 11. SVVP analysis covariances for the zonal, meridional, and vertical wind obtained from the retrieval for the instantaneous fits.

The combined errors will have a sharp edge around the strength of the Lagrange multiplier and a tail towards smaller values. Vertical winds with errors close to this barrier indicate low measurement response and that the solution is dominated by the regularization term. Although we do not explicitly define a zero vertical wind a priori, the solution converges to zero for those cases (edges of the meteor layer). The Tikhonov regularization is also implemented for the other fitting parameters, but has basically no impact on the final error distribution. All statistical uncertainties for zonal and meridional wind and the higher-order kinematic terms are much smaller than the typical values of these quantities. The Tikhonov regularization uses the continuity equation (horizontal divergence) as a constraint for both vertical winds (compressible nonstationary and incompressible solution similar to the 3DVAR+DIV). The radius of influence is simply defined by the measurement density or the number of detections with overlapping volume between the radars. We did not investigate at which point the linear approximation becomes invalid. For seasonal mean characteristics as presented in the manuscript, this is a minor point considering previous comparisons between each monostatic system of the Nordic Meteor Radar Cluster.

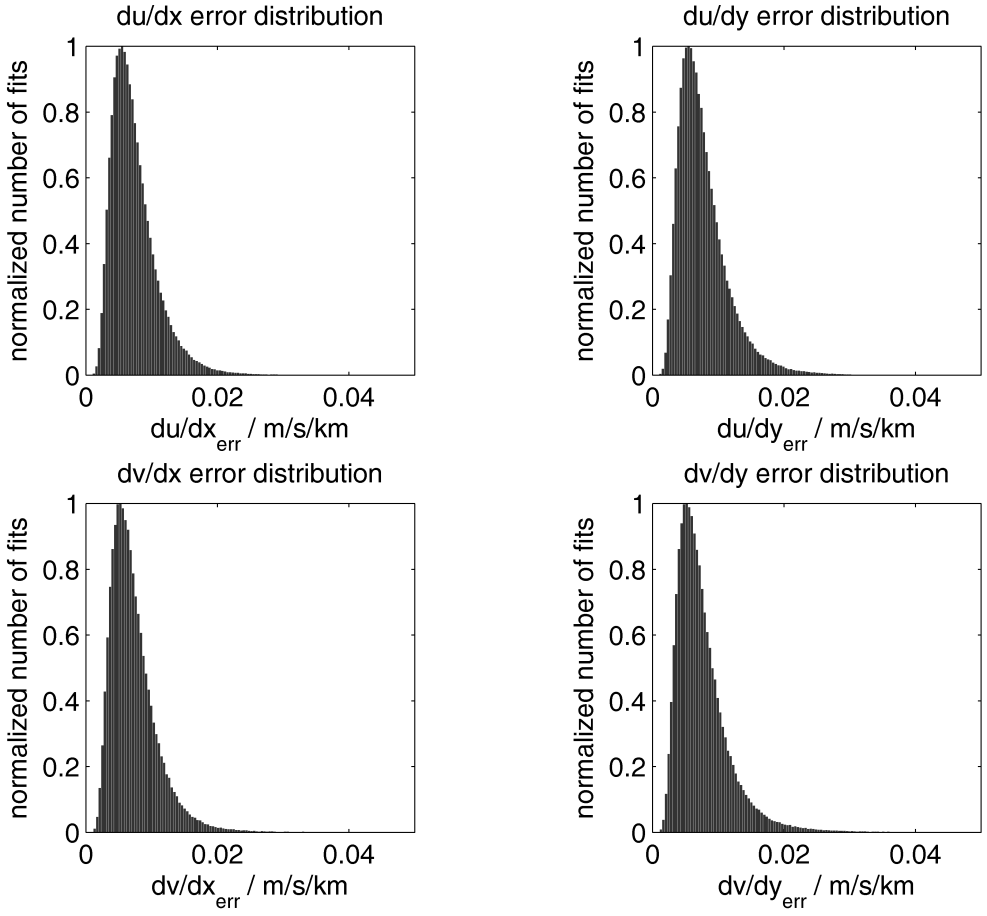


Figure 12. Same as Figure 11, but for the gradient terms.

Review Report

The revised manuscript has improved substantially. The authors have clarified the complementary roles of SVVP and 3DVAR+DIV and added residual examples, meteor-count distributions, measurement-response diagnostics, condition-number information, and a station-removal experiment. However, several of my major concerns remain only partially addressed.

General Reply: *We appreciate the reviewer's acknowledgment of the improvement in our revised manuscript and recognition of our efforts to address the previous concerns. In response to the concerns raised, we have included an analysis comparing the retrieved vertical wind from SVVP and VVP with JAWARA and SE-WACCM-X. Furthermore, we added vertical wind bias for 15 and 30 minutes at 200 km and 250km along with the corresponding measurement statistics. We have also added the station removal test for zonal and meridional winds and the high-order kinematic variables to the supplementary material. Below, we provide point-by-point replies to the raised concerns in the attached PDF. The revised manuscript is prepared with latexdiff.*

Major comments

1. Using identical observations and processing settings demonstrates that the differences between VVP and SVVP originate from their geometric formulations, but it does not show which retrieval is closer to the true wind field. A simple experiment with known wind fields and the actual NORDIC sampling geometry would directly quantify whether SVVP reduces spurious vertical wind and divergence. Previous validation of 3DVAR+DIV does not validate the newly developed SVVP.

Response: *We have added the figure below to the revised manuscript to compare the vertical winds from both VVP and SVVP with those from JAWARA and SE-WACCM-X. In this comparison, we maintain the SE-WACCM-X and JAWARA at 80*

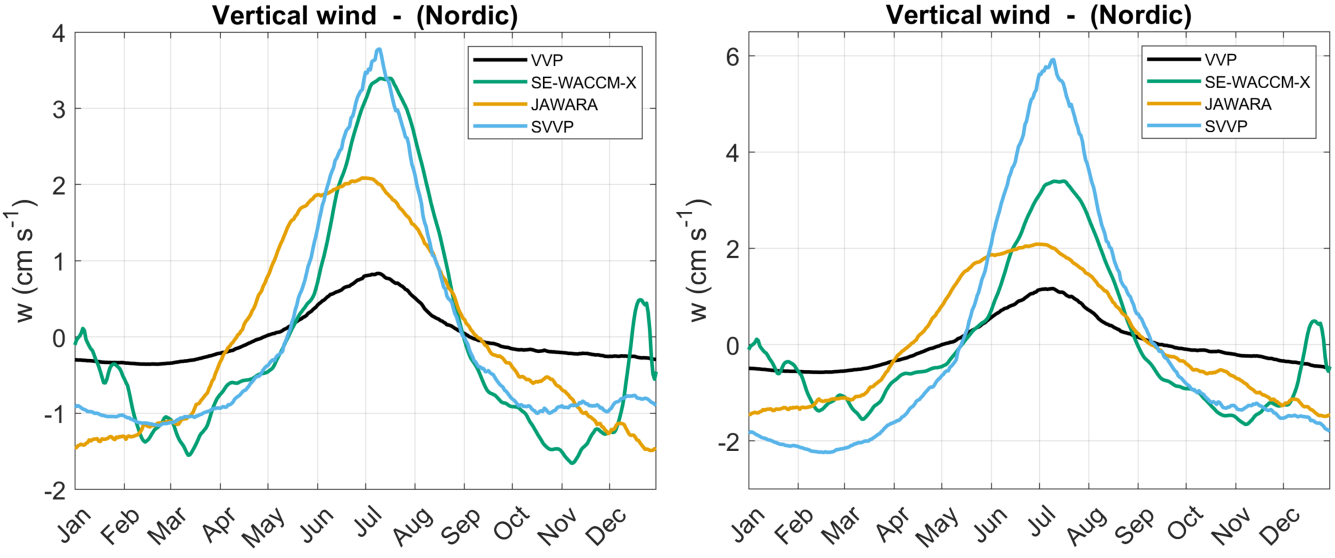


Figure 13. Vertical winds from the JAWARA reanalysis and the free-running SE-WACCM-X model averaged over the altitudes 80 km to 90 km (both panels), with VVP and SVVP analysis averaged over 80 km to 90 km (left panel) and 84 km to 94 km (right panel)

km *and 90* km *and compare them with VVP and SVVP at 80* km *to 90* km *altitude layers on the left panel and 84* km *to 94* km *altitude layers.*

Synthetic wind fields won't help to distinguish which algorithm is more 'true'. Higher-order kinematic terms cannot be directly used to generate synthetic wind fields, as they themselves depend on the geometry (as the comparison shows). Using spherical geometry to synthesize these terms leads to better agreement with the SVVP. Using the VVP geometry will lead to better agreement with the VVP. Hence, such an approach is not helpful to distinguish which algorithm is closer to the true state. It's only possible to check the correct implementation of the least squares. We have no access to a fully compressible fluid dynamic model, which includes gravity waves, tides, and planetary waves, either as free-running or driven by reanalysis, that provides control on these quantities either.

2. The correction is comparable in magnitude to the retrieved seasonal vertical winds. The authors should show whether the seasonal pattern remains under alternative bias estimates and provide uncertainty associated with the correction. The different bias ranges reported in the response and manuscript should also be reconciled.

Response:

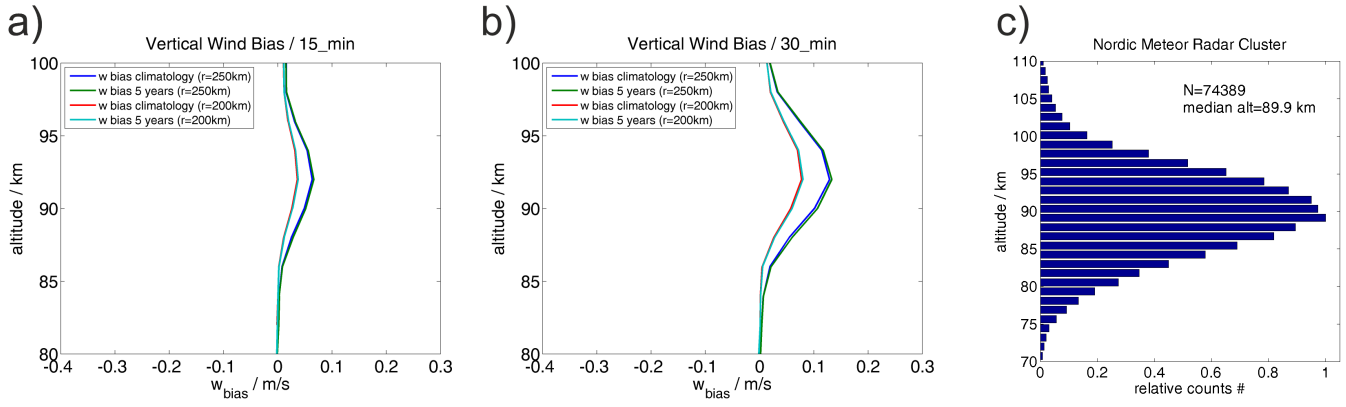


Figure 14. Vertical wind bias at 15-minute and 30-minute temporal resolutions for 200km and 250km is shown in panels a) and b), respectively. For comparison, panel c) presents the normalized altitude distribution, serving as a proxy for the measurement statistics.

We have added Figure 14 to present vertical wind bias for 15 and 30 minutes at 200 km and 250km along with the corresponding measurement statistics. The different bias removals have not been shown to impact the seasonal pattern. Although the bias is similar in magnitude, it is constant for most altitudes, which is stable for several years and for the climatology.

3. The reported condition numbers and meteor-count threshold are useful, but they do not establish that vertical wind and gradient terms are independently constrained. At least one additional diagnostic, such as parameter covariance, coefficient

correlations, formal uncertainties, or normalized singular values, should be provided. The station-removal test should also address the higher-order kinematic terms, not only the mean vertical wind.

Response: *The revised manuscript shows the covariances for all fitted parameters, singular value statistics, and we have added the zonal and meridional winds, along with all higher-order kinematic terms for the station-removal test below and in the supplementary material as Figures S52 to S54.*

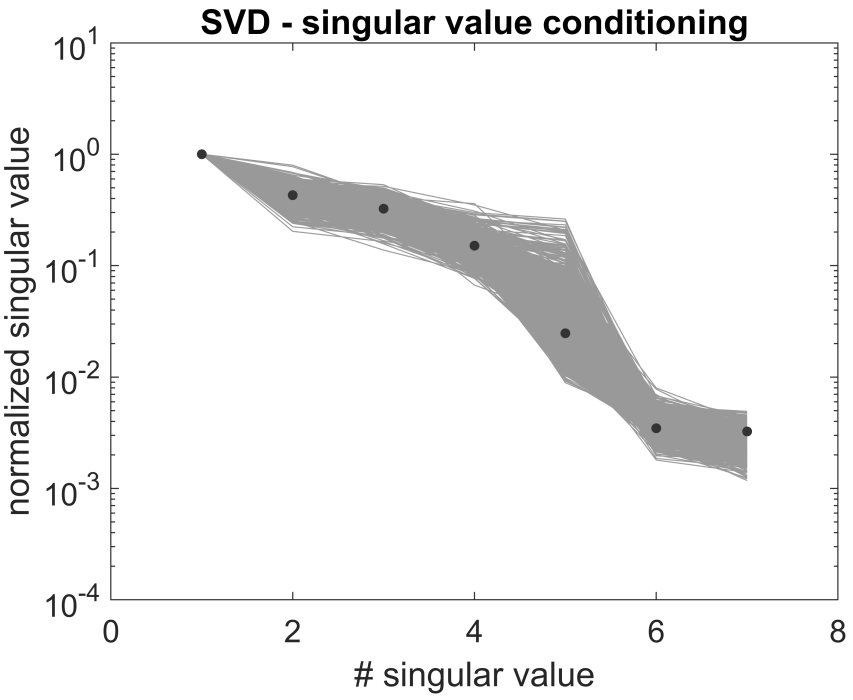


Figure 15. Singular values for one day and all fits, shown as a grey-shaded area and mean values (black dots)

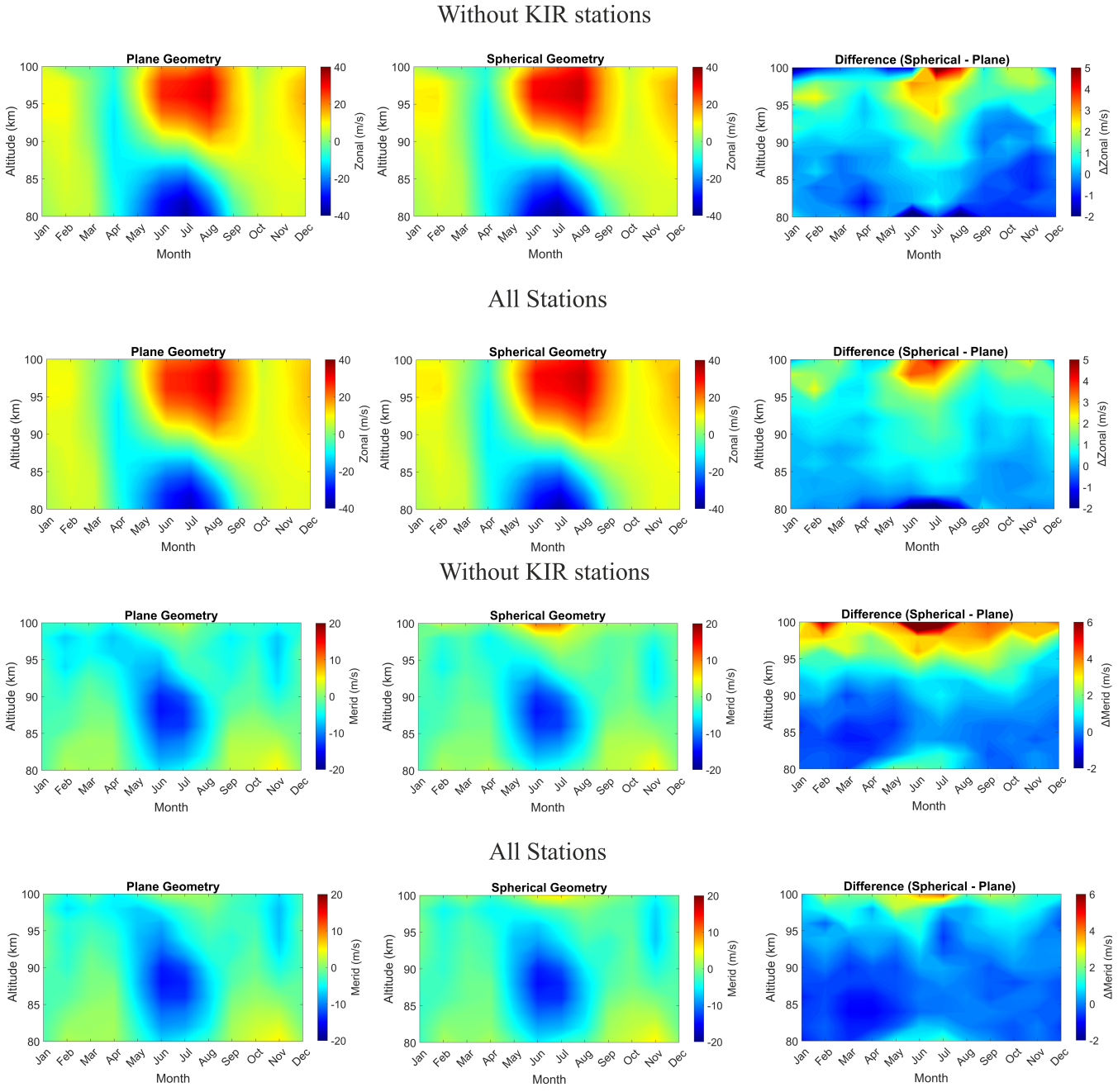


Figure 16. Monthly mean zonal and meridional wind components retrieved using the Volume Velocity Processing (VVP), showing the spherical and plane geometries and their corresponding differences for the experiment with all stations (bottom row) and without the KIR station (top row)

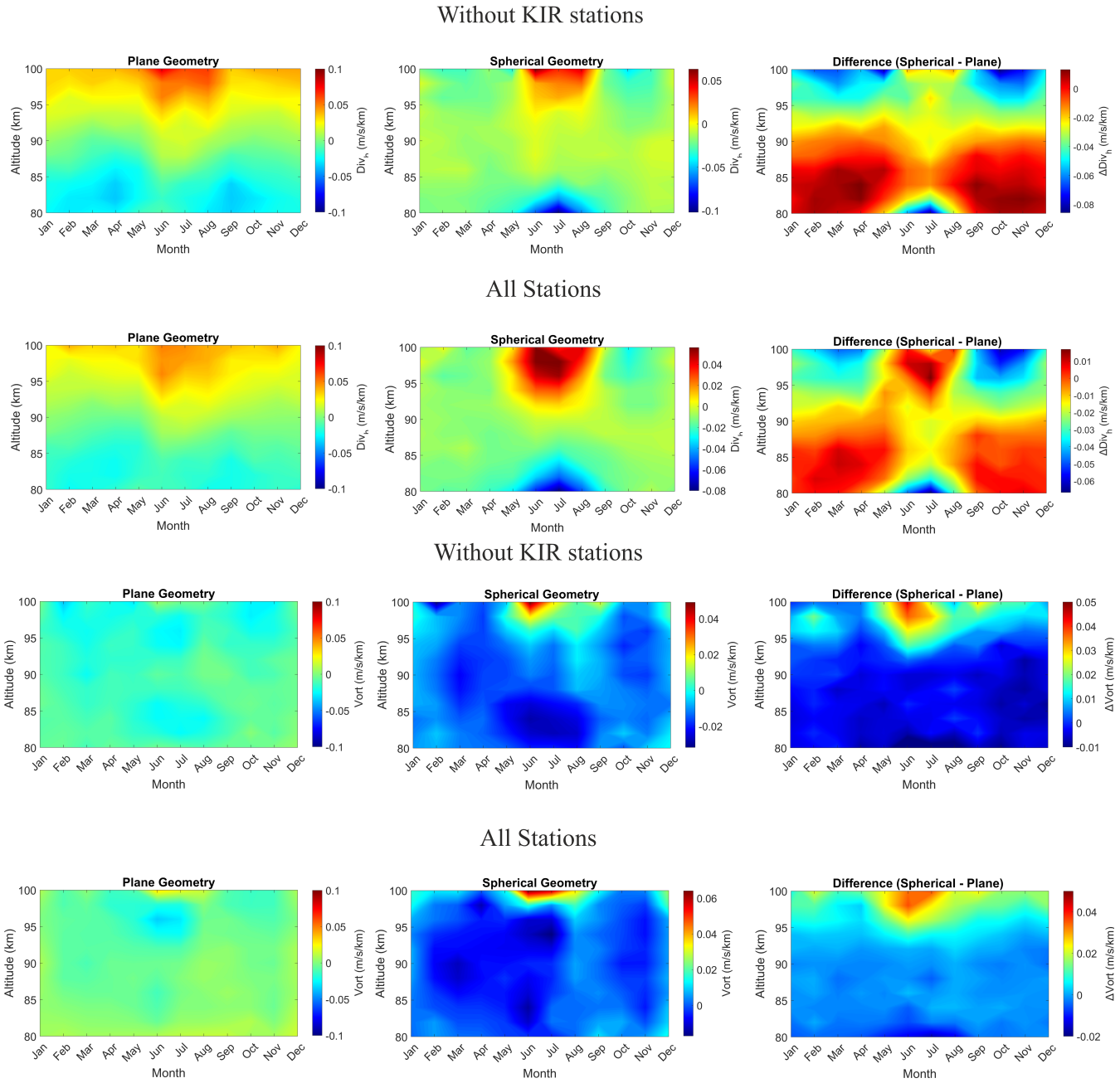


Figure 17. Composites of horizontal divergence and relative vorticity using Volume Velocity Processing (VVP), showing the spherical and plane geometries and their corresponding differences for the experiment with all stations (bottom row) and without the KIR station (top row)

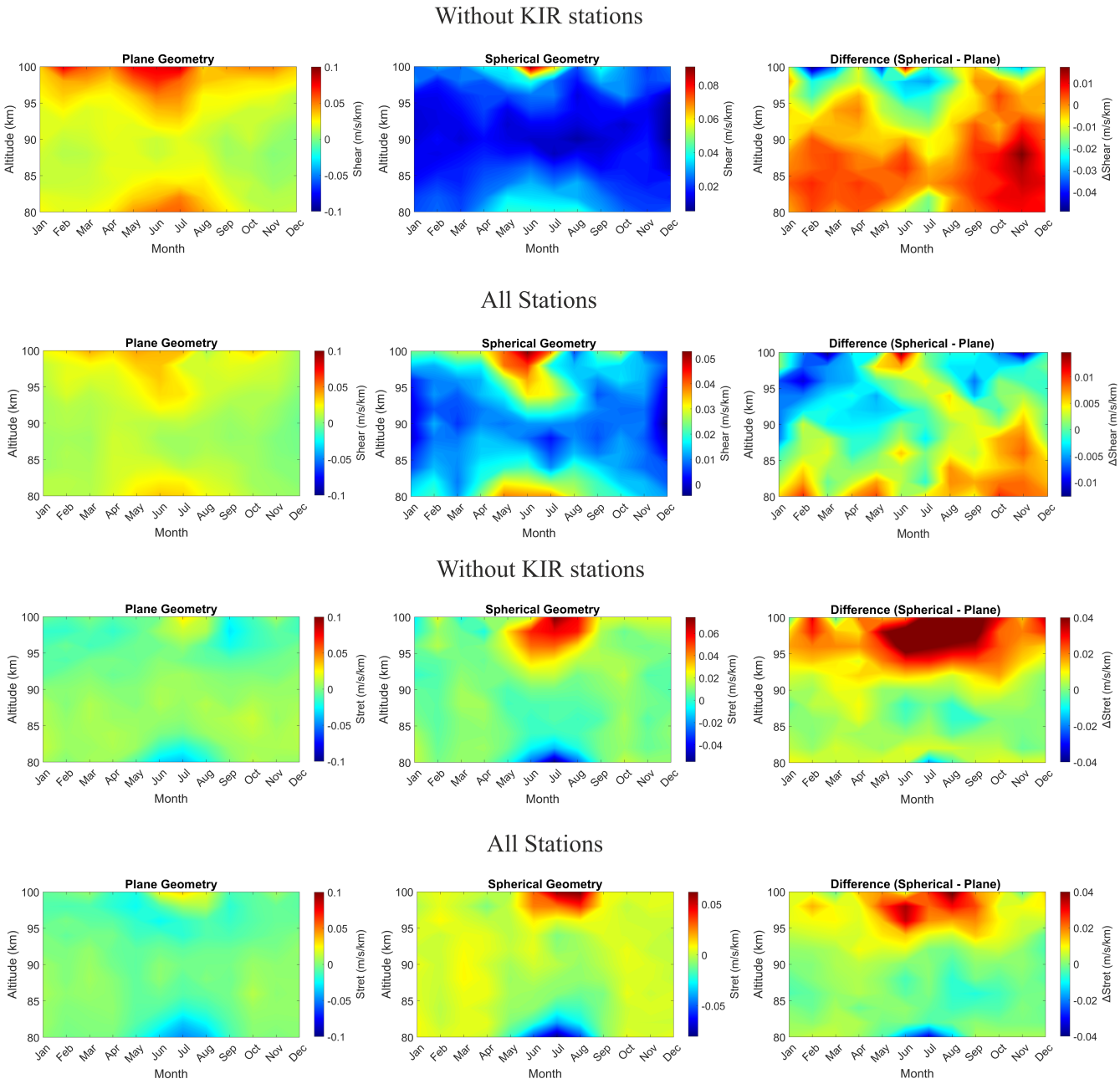


Figure 18. Composites of shearing and stretching deformation using Volume Velocity Processing (VVP), showing the spherical and plane geometries and their corresponding differences for the experiment with all stations (bottom row) and without the KIR station (top row)

4. The new measurement-response and meteor-count plots support the choice of 200–250 km as a reasonable coverage range, but they do not establish that the corresponding vertical winds are the "most reliable." This wording should be moderated unless the authors jointly evaluate residual error, conditioning, bias-correction uncertainty, and first-order representation error. A more appropriate conclusion would be that 15–30 min and 200–250 km provide the best compromise between sampling density and spatial averaging for the present network.

Response: *We have revised our conclusion according to your suggestion.*

4 Review Comments and Detailed Authors: Third CO Editor reports

Review Report

The authors have made substantial and commendable efforts to address the concerns raised in the previous review rounds. The addition of the JAWARA and SE-WACCMX comparison, the vertical wind bias analysis with measurement statistics, the spectral analysis, the station-removal experiments, and the covariance diagnostics have significantly strengthened the manuscript. The mathematical formulation of the Spherical Volume Velocity Processing (SVVP) remains a valuable methodological contribution, and the demonstration that Earth's curvature must be accounted for when retrieving higher-order kinematic quantities from multistatic meteor radar networks is now more convincingly supported.

However, several issues identified in previous reviews remain either insufficiently addressed or have introduced new concerns that require attention before the paper can be accepted for publication in Atmospheric Measurement Techniques.

General Reply: *We thank the reviewer for acknowledging our efforts in the revision. We appreciate the reviewer's patience. We answered the remaining questions with concise replies and made corresponding changes in the manuscript. We also cross-checked quantitative numbers to be consistent throughout the manuscript. Please find our replies to the raised concerns in the attached PDF below. The revised manuscript is prepared with latexdiff.*

Major Comments

1. The authors have added Figure 14 comparing vertical winds from VVP/SVVP with JAWARA and SE-WACCM-X. While this is a welcome addition, the comparison is presented with minimal context. The authors state that "a detailed discussion will be left to the companion paper" (Response, p. 1), but this is insufficient for the current manuscript. The reader is left to wonder why the altitude layers differ between the left and right panels, why JAWARA shows substantially more negative winter values than all other datasets, and what the implications are for the reliability of SVVP versus VVP. Therefore, I suggest expanding the discussion of Figure 14 in the main text. Specifically:

- **Comment:**

  Explain why the 80–90 km and 84–94 km altitude layers were chosen and what this reveals about the altitude sensitivity of each retrieval method.

  Respone:

  The altitude windows were selected to match the altitudes with a distinct peak in the vertical velocity for SE-WACCM-X (90-90 km*) and the NMRC (84-94* km*). JAWARA shows less pronounced altitude variation in the vertical velocities and, thus, we did not perform a similar procedure due to the lack of a distinctive peak in the vertical velocities. We explicitly add these statements to the appropriate passage as reasoning.*

– **Comment:**

Provide at least a brief interpretation of why JAWARA deviates from the other datasets in winter and autumn.

Respone:

Compared to both other datasets, JAWARA has a much coarser spatial resolution, filtering small-scale gravity wave variability, which plays a crucial role in driving the residual circulation and, thus, modulates the seasonal strength in the magnitude. On the other hand, the deviation is in the order of 1-2 $\mathrm{cm/s}$*, which seems to be acceptable also for model data comparisons of this type. Previous comparisons showed orders of magnitude differences (Chau et al., 2017; Urco et al., 2024). We added: Furthermore, the above-shown comparison is in first place a physical consistency check rather than an absolute validation. There are still idealizations in the data analysis, such as the assumption of incompressibility in 3DVAR+DIV or other divergence-based vertical winds. Currently, we speculate that the differences in vertical wind magnitudes in JAWARA are likely related to the much coarser spatial resolution of about 2 degrees in longitude and latitude, removing the gravity wave part of the spectrum. SE-WACCM-X is a free-running model and not yet tuned to match observational fields, and some physical processes are still parameterized, which can lead to changes in the absolute magnitude of the vertical velocity.*

– **Comment:**

State explicitly whether the SVVP agreement with SE-WACCM-X (which the authors note have "the highest and most similar peak amplitudes") constitutes a validation or merely a consistency check.

Respone:

A proper validation requires an alternate observational method, such as the trace gases from SABER or other instruments (Yue and Wang (2025b) – unfortunately not covering all latitudes). Therefore, we will explicitly mention that this is a consistency check. (see reply above).

2. The response states that "we don't believe a formula is necessary" (Response, p. 2) because only a mean value is subtracted. This is not an adequate justification for a manuscript in AMT, which requires methodological transparency. The authors have added Figure 4 (formerly Figure 6 in the response) showing bias values of 1–8 cm/s, but the estimation algorithm is still not described. Please add a dedicated subsection in Section 4 (or expand the existing text) that:

– **Comment:**

States explicitly the mathematical operation used to compute the bias (e.g., $w_{bias}(z) = \frac{1}{N}\sum_1(w,t)$ for the full time series or climatological period).

Respone:

We added the recommended formula.

– **Comment:**

Clarifies whether the bias is altitude-dependent and, if so, how the altitude dependence is handled.

Respone:

We added that the bias is subtracted from each altitude. However, it would also be possible to limit the bias subtraction to the altitude with significant measurement response (86-94 km*).*

– **Comment:**

Provides uncertainty estimates for the bias correction (e.g., standard error of the mean).

Respone:

We added standard errors to the Figure for the climatology-derived biases. These error bars are representative of the 5-year-long series as well. The vertical wind magnitudes are in the order of what one could estimate from the equivalent depth of the corresponding hough modes, although we cannot directly fit the hough modes to confirm.

– **Comment:**

Explains why the assumption of zero mean vertical wind over multi-year periods is physically justified, referencing the meteor ablation altitude argument but also acknowledging its limitations.

Respone:

We added to the paragraph (in bold): "Furthermore, we can justify our bias correction by considering previous results. The most precise and accurate measurements of the vertical motion of the meteor layer are obtained by tracking the mean ablation altitude (Stober et al., 2014; Dawkins et al., 2023). These studies suggest that the middle atmosphere is shrinking by a maximum of 850 m per decade. This translates to a mean vertical velocity of $2.6 \cdot 10^{-6} m/s$*. Thus, it is reasonable to consider any substantial deviation from this value over a period of 5-years or as the climatological mean at each altitude as bias, which can be subtracted.* ***This method is valid under the assumptions that the radars retain a constant sensitivity and that the statistical properties of the sporadic meteoroid population have no significant trend concerning the chemical composition, velocity, or mass of the meteoroids.***

3. The authors have added Figure 5 showing fitting residuals for two illustrative cases. While this is useful, it does not constitute a systematic assessment of when the linear approximation fails as a function of radius, altitude, or season. The authors acknowledge: "We did not investigate at which point the linear approximation becomes invalid" (Response, p. 4). This is a significant gap, as the validity of the first-order approximation is a core assumption of the VVP/SVVP method. Please add a systematic analysis of residual statistics (e.g., standard deviation of residuals, fraction of fits with significant residual structure) as a function of radius, altitude, and season, presented in the supplement; or explicitly state in the manuscript that the first-order approximation is expected to hold for the seasonal and climatological means presented but may fail for instantaneous retrievals, and temper the conclusions accordingly.

Respone:

We will explicitly mention that the first-order approximation is expected to hold for seasonal and climatological means, but might not be sufficient for some instantaneous cases. These effects are one of the main reasons why the 15-min and 30-min temporal resolutions are more reliable compared to the 1-hour data. The lowest temporal resolution indicated more of these extreme events with clear wave features in the residuals compared to the 15-min and 30-min data.

4. The authors have added a comparison with 3DVAR+DIV, which is valuable, but several aspects are unclear:

- The regularization parameter α is used in two different contexts (0.4 and 150), but the physical meaning of these values and why they were chosen is not explained.
- Figure 16 shows wind fields for two specific dates, but it is unclear whether these are representative cases or were cherry-picked.
- The statement that "the 3DVAR+DIV retrieval intrinsically uses the incompressibility condition and a simplified boundary condition... resulting essentially in the removal of all seasonal features in the vertical winds" (Section 5.2) suggests that the 3DVAR+DIV vertical winds are not reliable for climatological purposes. If this is the case, why include them in the comparison at all?

Comment:

Please (1) add a brief explanation of the Tikhonov regularization parameter and the rationale for choosing $\alpha = 0.4$ and $\alpha = 150$;

Respone:

The Tikhonov regularization parameter directly affects the covariance weighting inside the cost function. A value of 0.4 increases the apriori covariance of 25 $\mathrm{m/s}$ *for the horizontal winds and 5* $\mathrm{m/s}$ *for the vertical wind by a factor of 2.5. The value of 150 for the regularization strength results in a very small apriori covariance and thus enforces a very smooth solution. The volume mean remains not affected and is purely data-driven by an optimal solution of the least squares.*

Comment:

(2) Clarify whether the dates shown in Figure 16 are representative or selected to illustrate specific behavior.

Respone:

It is not really possible to perform cherry-picking approaches for the 3DVAR+DIV analysis for a 5-year-long record. Images are only calculated for selected dates or periods of a few days. A comprehensive review of all solutions corresponds to 1752000 pictures for each wind component. The dates were picked from the winter season, as during the Geminid meteor shower (14 December) the data quality is rather high, and the December winds are always good to look for features compared to more 'boring' meteorological situations in spring or fall. The January data is typical for a moderate/normal meteor detection rate, but with the typical winter circulation pattern.

Comment:

(3) Reconcile the apparent contradiction: if 3DVAR+DIV "removes all seasonal features," how can it be used as a benchmark for vertical wind climatology?

Respone:

The current version of 3DVAR+DIV indeed removes all seasonal features, as we are not able to get the error budget small enough within each grid cell. The covariances for the vertical winds (instantaneous) are in the order of 1-2 $\mathrm{m/s}$*. This is too large to get meaningful seasonal means. We added:* ***However, our choice of the boundary condition results essentially in the removal of all seasonal features in the vertical winds due to the larger errors for the retrieved quantities.***

Minor Comments

1. The authors have revised their conclusion according to my suggestion (Response, p.8), but the revised wording appears only in the response document, not in the manuscript. The manuscript still states in the abstract: "This configuration provides truly multistatic measurement and achieves optimal retrieval performance." While improved, this still implies more certainty than is warranted. Please ensure the manuscript consistently reflects the more cautious wording agreed upon in the response.

Response: *We have revised the wording.*

2. Several figure captions in the supplement have incorrect numbering (e.g., Figure S35 caption references "Figure S35" itself, Figures S39 reference "Figure S39"). The authors appear to have reused captions without updating figure numbers. Please carefully proofread all figure captions in the supplement to ensure correct cross-referencing.

Response: *We have corrected all the figure captions and references in the supplement material.*

3. Several references appear with minor formatting issues (e.g., inconsistent use of DOIs; missing page numbers). Please ensure all references comply with the journal's formatting requirements.

Response: *We have proofread all references.*

References

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