Authors Response

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 (**??**). 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 (**?** – 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 (**??**). 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.*