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-WACCM-X 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.

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:

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

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:

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

- Clarifies whether the bias is altitude-dependent and, if so, how the altitude dependence is handled.
- Provides uncertainty estimates for the bias correction (e.g., standard error of the mean).
- 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.

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.

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?

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

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

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

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.

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.

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.