Dear Samuel Kristoffersen,

Thank you for the positive comments on the submitted paper. We appreciate this feedback and will incorporate your comments and suggestions in the revised manuscript. Below we attach our replies to the raised concerns. We added and expanded the replies compared to the public discussion phase according to the final revised papers. All changes can be found in the tracked changes version.

Comment:

Given that the two algorithms provide different vertical wind results, it is not clear to me which are considered the 'correct' results. My understanding is that the 3DVAR-DIV is being demonstrated to show de-biased horizontal and vertical winds, and therefore this new algorithm is quite useful to the community. It is, therefore, my opinion that the paper is suitable for publication, pending some clarifications regarding, in particular, the Tikhonov regularization.

Reply:

We present three mathematical approaches to minimize biases related to the estimation of vertical winds from meteor radar observations. The first two methods are applicable to all meteor radars and provide the mathematical justification of neglecting vertical winds in the standard meteor radar wind analysis. The synthetic data set is used to demonstrate the importance of such a bias correction assuming the challenging scenario of zero vertical winds, which is a non-trivial solution to the problem.

The second aspect worth emphasizing is that the vertical wind magnitudes depend on the temporal and spatial scales that a sensor has (observational filter), which poses additional challenges in comparing different instruments and analysis methods. The first two methods of a Tikhonov and generalized Tikhonov are demonstrated for a 300 km diameter observation volume, which is much larger than the 3DVAR+DIV volume of a single voxel of about 30 km. Thus, the expected order of magnitude for the vertical winds is supposed to differ as well by an order of magnitude.

More details are provided in the replies below.

Here are my major comments/questions regarding the content and the results.

Comment:

1. Given that the two methods provide different vertical wind results (std~0.2 m/s for the Tikhonov regularization and ~1-1.6 m/s for 3DVAR-DIV), are the authors able to conclude which method is providing the correct (or most accurate) vertical wind results? This was not clear to me in the paper.

Reply:

The 3DVAR+DIV retrieval provides the most physically and mathematical consistent solution for the vertical winds and many sampling issues are resolved due to the much smaller voxel size. While there is no ground 'truth' that we can use to validate our retrievals to state which one is more 'accurate', we can conclude that the generalized Tikhonov retrieval for the monostatic systems provides reasonable statistical distributions compared to ICON-UA (large scales). For 3DVAR+DIV, there is no comparable independent observations at the small horizontal scales that it resolves (~30 km). Nevertheless, we consider both valid estimates of vertical wind but suitable for different spatial scales. The smaller vertical wind estimate with Tikhonov regularization is expected because of its corresponding larger horizontal scale (~300 km).

Comment:

1. The Tikhonov regularization is tested on real data, and synthetic data with no vertical wind. Given that synthetic data is created using tides, planetary waves, and gravity waves, which should have vertical components, are these synthetic data physically realistic? Additionally, I think it would be worthwhile to do a test on synthetic data which has known vertical winds. Currently, the assumption is there are no vertical winds, and that is observed. But can the known vertical winds of synthetic data be retrieved? This would help to clarify if the small vertical winds are real, or simply the result of the assumption that the vertical winds are small.

Reply:

The scenario with negligible vertical wind is the most challenging one. The retrieval of small/tiny parameters is much more demanding than the retrieval of larger parameters/quantities such as the horizontal wind. The synthetic model that is mentioned in this study, was already incorporated in previous work to test the momentum flux retrieval (Stober et al., 2021, AG). This early paper did include non-negligible vertical velocities for the <u’w’>, <v’w’> fluxes. So far, the model is physically realistic and pretty similar to what Fritts et al., 2010 demonstrated for SAAMER.

The algorithm is able to retrieve the correct vertical velocities for scales much larger than the observational filter. These vertical velocities are most likely in the order of a few cm/s and associated with atmospheric tides, planetary waves, and inertia gravity waves with horizontal wavelengths much larger than 500 km. We will refer to these waves as resolved scales. Small scale waves with 60 km horizontal wavelength and a vertical velocity magnitude of 1-2 m/s, will be recovered by the 3DVAR+DIV, but essentially result in w=0 m/s for the monostatic retrieval (generalized Tikhonov) with a 300 km diameter of the observational filter. The unresolved scales are treated as atmospheric noise and are included in the total error budget (temporal and vertical shear).

Based on the retrieval algorithm, we are able to identify the largest issues to obtain the correct vertical velocities for the resolved scales. The algorithm will return the correct solution under the following conditions:

- all observations within a time-altitude bin are taken exactly at time t0 and altitude h, which corresponds essentially to an infinitely small and short bin around our reference grid
- the radial velocity error must be much smaller than the nominal vertical velocity
- the vertical velocity field is homogenous within the observational filter
- a sufficient good observational statistics close to the zenith of a monostatic meteor radar

For the above-described conditions, the retrieval would recover the correct vertical velocity from synthetic data. However, this is basically never the case. Due to the binning in time and space, we usually tend to underestimate the vertical velocity in dependence on temporal and vertical bin size and the wave properties such as period, phase velocity, and horizontal and vertical wavelength.

The largest damage to the vertical velocity estimation occurs when large vertical bin sizes are used. Due to the strong shears caused by tidal waves larger, this procedure significantly increases the atmospheric noise caused by the vertical shear flow within a time-altitude bin and, thus, inhibits the ability to retrieve the correct values. Ideally, not more than 2 km vertical resolution with some small oversampling is advisable.

Furthermore, the retrieval tends to sustain the a priori state for large error observations and at altitudes with poor measurement statistics. The a priori is assumed to be w=0m/s.

Comment:

1. Since the Tikhonov regularization effectively filters the vertical winds, are these results different than making the 0 vertical assumption, which is typically made with meteor radar winds?

Reply:

The first method presented in the paper involving a Tikhonov regularization is meant to provide a mathematical framework to underline that the assumption of negligible winds is not only just a quick idea, or simplification. The assumption actually has a solid mathematical and physical reason considering the spatial and temporal sampling. It is possible to show that a least-square solution for horizontal winds with the assumption of a zero vertical component, could be used as a regularization constraint to fit in a second step for the vertical wind component. As zero vertical wind was assumed, it is expected that this procedure should return a small vertical wind magnitude as a solution. However, the histogram will be identical to the one presented in Figure 1. The observations provide not enough information about the vertical winds to avoid numerical instabilities and, thus, often result in artificially large vertical wind magnitudes. In summary, the assumption of a negligible vertical wind is better than applying an ordinary least square fit for all three components, which would not only bias the solution for the vertical wind, it also has adverse effects on the horizontal components.

Comment:

1. Regarding the apparent motions of the specular scattering point, you mention that the radial velocity measurements are representative of a short time period (line 472). For times of less than a second, I would expect the air parcel motion to be no more than a few 10s of m/s. If the size of a voxel is on the order of kilometers, do these scales result in significant deviations in the observed radial winds?

Reply:

The apparent motion of the scattering point is not critical compared to the voxel sizes. As mentioned above the trail drifts only a few meters, which is not adding an issue for the 3DVAR+DIV retrieval concerning the localization of a meteor echo within a certain voxel.

The main issue of the apparent motion of the scattering center is related to the estimation of the vertical velocity. For monostatic systems, the Bragg vector magnitude (radial velocity) is affected by only a few cm/s, which is not a big problem for the horizontal winds but induces a bias in the absolute vertical velocities. These are usually in the same order of magnitude or less.

However, for forward scatter systems such as CONDOR, the apparent motion can become much more significant and reach several meteors and, thus, the Bragg vector magnitude can be biased by up to several m/s, which leads to significant limitations deriving horizontal winds. At the lowest altitudes of Figure 3, we obtain a factor of 2 too large horizontal velocities.

The main reason is the specular scattering along the meteor trail. This is like looking into a mirror. If you are directly in front of the mirror small motions of the mirror towards or away from an observer won't lead to big changes in the image (represented here by the Bragg vector), while when looking at very shallow angles at a mirror, small changes of the mirror position can lead to rather significant changes of the image.

We thank the reviewer for all the technical corrections and will include them in the revised paper and provide a point-by-point reply on the changes.

Comment:

I also have a few minor questions/comments (typos etc.)

We thank the reviewer for the language corrections.

Comment:

The left quotation marks appear as right quotation marks. If you are using some version of Latex, you can generate the left quotation mark using `, e.g. `A'.

Reply: The manuscript will receive typesetting.

Comment:

Line 109: I think it would be useful to define what WGS84 is.

Reply: Added.

Comment:

Line 115: change 'visualizes' to 'visualize'

Reply:

Done.

Comment:

Line 116: should ‘radial velocity’ be ‘vertical velocity’?

Reply:

Changed.

Comment:

Line 134: should ‘standard variation’ be ‘standard deviation’?

Reply:

Changed.

Comment:

Line 164: change ‘later’ to ‘latter’

Reply:

Changed.

Comment:

Line 164: re-write sentence from ‘Due to the more slant incident radiowave, the scattering section along the trail is much longer’ to ‘Due to the larger slant of the incident radiowave, the scattering section along the trail is much longer.’

Reply:

We rephrased the sentence.

Comment:

Line 138: change ‘to consider’ to ‘considering’

Reply:

Changed.

Comment:

Figure 3: What year are these winds from? If these are means of several years, what years?

Reply:

Information added.

Comment:

Line 200: include ‘the’ before ‘end of April’ (i.e. ‘the end of April’)

Reply:

Done.

Comment:

Line 203: similarly, include ‘the’ before ‘end of May’

Reply:

Changed.

Comment:

Line 219: Referring to equation 2, you mention ‘superscripts denote the Euclidean norm’, however, no superscripts appear in this equation.

Reply:

There are superscripts around the vertical lines denoting the Euclian norm. $|| \ .||^2$

Comment:

Line 224: You state that you used the unit matrix for the Tikhonov regularization. What was the rationale for using the unit matrix? Do you get different results using a different Tikhonov matrix?

Reply:

The sentence was rephrased. The identity matrix (unit-matrix) is often used in damped least squares. However, we use the product from a covariance and the identity matrix.

Comment:

Line 237: change the line ‘The larger the statistical uncertainties the stronger and more important becomes the right hand term, which often results in smaller vertical velocities.’ to ‘The larger the statistical uncertainties, the stronger and more important the right-hand term becomes, which often results in smaller vertical velocities.’

Reply:

Done.

Comment:

Line 246: remove ‘already’

Reply:

Done.

Comment:

Line 248: add a comma after ‘the scale analysis described above,’

Reply:

Done.

Comment:

Line 249: Remove ‘however’

Reply:

Done.

Comment:

Line 260: Define ‘R2B4’

Reply:

We added an explicit reference for the grid. However, there is no simple and short answer or explanation. This is a generic expression of an icosahedral grid and associated derivatives.

Comment:

Line 308: change ‘… which permits to obtain …’ to ‘… which permits us to obtain …’

Reply:

Changed.

Comment:

Line 339: Change ‘… which allows to achieve high spatial resolution.’ to ‘… which allows for a high spatial resolution to be achieved.’

Reply:

Changed.

Comment:

Line 327: change ‘physical’ to ‘physically’

Reply:

Done.

Comment:

Figure 9: The difference between the left and right panels is not clear. Does (div) mean this is the incompressible case? Please include a comment in the caption describing the difference between the left and right panels.

Reply:

Information was added.

Comment:

Line 393: remove ‘as well’

Reply:

Done.

Comment:

Line 449: Change ‘to be considered’ to ‘consideration’

Reply:

Changed.

Comment:

Line 452: Change ‘We tested also domain means and other options.’ to ‘We also tested domain means and other options.’

Reply:

Changed.

References:

Stober, G., Janches, D., Matthias, V., Fritts, D., Marino, J., Moffat-Griffin, T., Baumgarten, K., Lee, W., Murphy, D., Kim, Y. H., Mitchell, N., and Palo, S.: Seasonal evolution of winds, atmospheric tides, and Reynolds stress components in the Southern Hemisphere mesosphere–lower thermosphere in 2019, Ann. Geophys., 39, 1–29, https://doi.org/10.5194/angeo-39-1-2021, 2021.

Fritts, D. C., Janches, D., Hocking, W. K., Mitchell, N. J., and Taylor, M. J.: Assessment of gravity wave momentum flux measurement capabilities by meteor radars having different transmitter power and antenna configurations, J. Geophys. Res.-Atmos., 117, d10108, https://doi.org/10.1029/2011JD017174, 2012a.

Dear Chen Zhou,

Thank you for the feedback on our manuscript. We provide detailed replies to the raised points and will revise the manuscript accordingly. Due to the comments of the third reviewer, we revised the manuscript, which also lead to small changes in the replies below. A tracked changes version includes all revisions done to the initial submission.

General Comment:

This paper brings a thorough insight into the Meteor Radar vertical wind measurements and provides mathematical justification for the general assumption of zero vertical wind velocity in classical horizontal wind analysis. The intrinsic bias in the meteor radar system is analyzed in detail. The two debiasing algorithms presented in this paper show a significant improvement compared to the least-squares method. The 3DVAR+DIV algorithm can produce horizontal divergence and relative vorticity to identify coherent structures, which has good research potential.

The only problem is that the correctness of the vertical velocity retrieved by the algorithm can't be verified in an accurate and straightforward way. This is a difficult challenge for almost every observing method. Nevertheless, the author has tried several ways to indirectly prove the improvement in vertical velocity measurements. Moreover, some clarifications of equations and figures are not that clear in this paper. To avoid confusion and make the paper eminently readable, the author shall improve it in the revised version.

General reply:

Indeed, the validation of the monostatic retrievals involving the Tikhonov regularization and the Spatio-temporal Laplace filter are difficult to compare to other data. Due to the large observational volume, there are no other measurements available and only indirect comparisons are feasible. This is also the reason why we still keep the term 'apparent' vertical velocity and we make clear throughout the manuscript that we have no proof of their 'correctness' beyond the statistical moments concerning the ICON-UA model.

Major Comments:

Comment:

1. In Figure 1, large vertical velocities up to 10 m/s are obtained using the synthetic data with zero vertical wind. Thus, the author concludes that only sampling biases might explain it, considering that exact radial velocities and interferometric locations are well determined. Before drawing a conclusion, is it possible to take a detailed look at those extreme values in vertical wind velocity histograms? Because velocities up 10 m/s is far away from the zero vertical velocity setting. So checking these cases one by one might help you to rule out some other assumptions and provide more proof of your conclusion.

Reply:

The reviewer makes a good point. There is not that one reason why such high deviations from zero for the vertical velocities occur. The two main drivers are related to the random spatial and temporal sampling of small-scale atmospheric gravity waves within the beam volume and the poor geometric measurement response by low elevation meteors. The combination of both factors causes the deviations, but it is very difficult to make general statements for the monostatic systems. The 3DVAR+DIV retrieval already resolves some of the issues and, thus, provides the most reliable approach.

Comment:

2. The huge difference in the zonal wind between monostatic MR and passive receiver is worth noting. Based on the geographic map, three stations have similar longitude, but there exists a 1°latitude difference between each station. Will it possibly account for the less affected meridional winds and discrepancy in zonal winds? What's more, the comparison of the radiant map is not that clear. Because the source radiant is small compared to the relatively large map.

Reply:

The CONDOR system with its alignment of the transmitter and passive receivers is more prone to zonal wind deviations compared to the meridional wind component. This needs to be further explored with other setups also including passive receivers with a more zonal alignment.

The radiant maps are there to prove that the Bragg vector is correctly located. Any systematic localization error that could result in a factor 2 magnitude offset would be associated with a wrong zenith(off-zenith) angle and, thus, the shower would appear at a different location (higher declination or right ascension 15-30 degree) on the map. If the position determination is affected by random errors in the solver or the observations, the dispersion would lead to a vanishing source radiant and randomly occurring accumulation points in the radiant map would be visible. The angular probe size for the radiant mapping is about 4 degree to account for interferometric errors and the source radiant dispersion (Stober et al., 2013, Schult et al., 2018).

Comment:

3. The usage of the Spatio-temporal Laplace filter is pretty good in debiasing the measurement of vertical winds. Histograms of vertical wind velocities in Figure 5 and Figure 6 show a huge improvement compared to the standard least-squares method. Zero vertical wind synthetic data are used to prove that the new algorithm effectively mitigates the overestimation of the least-squares method. But I have a question about whether the newly debiasing algorithm is able to solve larger vertical wind velocity if the synthetic data is set with relatively large vertical wind velocity.

Reply:

The retrieval algorithm was already used in Gudadze et al., 2019 and doesn't contain a 'limitation' for a maximum vertical velocity, although we clearly have to state that the measurement response for the vertical wind for an HPLA radar is much different from a meteor radar. As the term ‘large’ vertical velocity is a bit unspecified, it is difficult to quantify. The more meteors we have in a time-altitude bin the more likely we can retrieve ‘larger’ vertical velocities. At the top and bottom of the meteor layer, the solver tends to stay close to the apriori. This is certainly a limitation. However, assuming a zero vertical wind might result in a smaller bias than computing a w=10 m/s with a least-squares when the likely true value is about 0.2 -1 m/s.

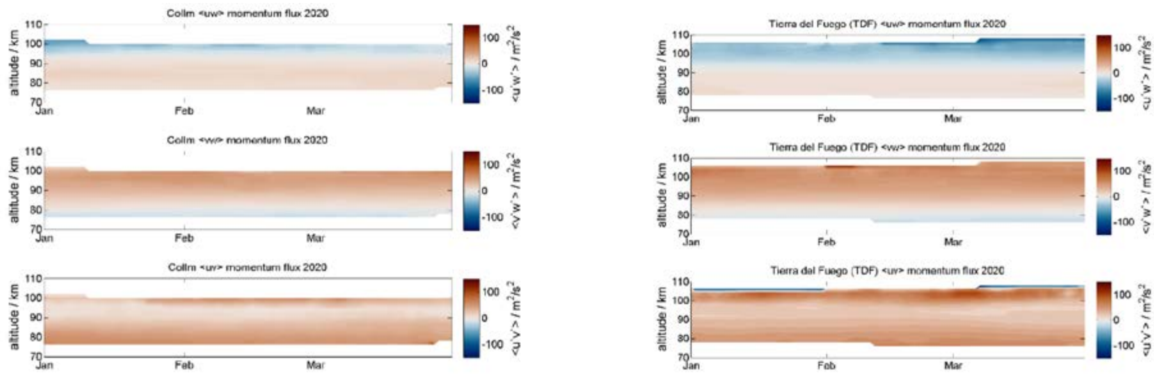
The most reliable solution is provided by the 3DVAR+DIV, where we found vertical winds up to 5 m/s values in some grid cells for 10 min temporal resolution data. There is no filtering done beyond the continuity equation or horizontal divergence.

Comment:

In Figure 6, a comparison of histograms of model results and retrieval algorithm might only illustrate that the distribution of vertical winds has similar statistical moments. Considering the comparison of individual observations is not feasible as the author has mentioned. If larger vertical wind velocity can be solved, it will provide much confidence in the correctness of this algorithm.

Reply:

We estimated the momentum flux to ensure that the gravity wave variability of the unresolved scales and resolved scales are well-reproduced (see Figures in the supplement material for Collm and TDF). The amplitude of the small-scale gravity waves changed with altitude by about 0.7 and 0.8 m/s per kilometer in altitude with opposite signs. The spatial and temporal sampling is kept, but the time averaging is done using 24 hours after subtraction of tides and planetary waves from each radial velocity using the adaptive spectral filter (Baumgarten and Stober, 2019 and Stober et al., 2020). The longer temporal averaging make a small mean vertical velocity even more likely and, thus, minimizes errors due to systematic offsets. Furthermore, the momentum flux retrieval indicates the small changes in the vertical velocity amplitudes of gravity waves with increasing altitude.



The 3DVAR+DIV retrieval resolves larger vertical velocities, although ‘larger’ cannot be quantified by a specific number. The monostatic solution depends on the total measurement statistics. The more meteors the better. Furthermore, the ability to observe higher vertical velocities is increased when using only meteors close to the zenith (<10-15°). However, very often there are not enough meteors directly above the meteor radars for such a narrow angular region to compute hourly winds.

References:

Stober, G., Schult, C., Baumann, C., Latteck, R., and Rapp, M.: The Geminid meteor shower during the ECOMA sounding rocket campaign: specular and head echo radar observations, Ann. Geophys., 31, 473–487, https://doi.org/10.5194/angeo-31-473-2013, 2013.

Baumgarten, K. and Stober, G.: On the evaluation of the phase relation between temperature and wind tides based on ground-based measurements and reanalysis data in the middle atmosphere, Ann. Geophys., 37, 581–602, https://doi.org/10.5194/angeo-37-581-2019, 2019.

Gudadze, N., Stober, G., and Chau, J. L.: Can VHF radars at polar latitudes measure mean vertical winds in the presence of PMSE?, Atmos. Chem. Phys., 19, 4485–4497, https://doi.org/10.5194/acp-19-4485-2019, 2019.
Carsten Schult, Peter Brown, Petr Pokorný, Gunter Stober, Jorge L. Chau, A meteoroid stream survey using meteor head echo observations from the Middle Atmosphere ALOMAR Radar System (MAARSY), Icarus, Volume 309, 2018, Pages 177-186, ISSN 0019-1035, https://doi.org/10.1016/j.icarus.2018.02.032.

Stober, G., Baumgarten, K., McCormack, J. P., Brown, P., and Czarnecki, J.: Comparative study between ground-based observations and NAVGEM-HA analysis data in the mesosphere and lower thermosphere region, Atmos. Chem. Phys., 20, 11979–12010, https://doi.org/10.5194/acp-20-11979-2020, 2020.

Thank you very much for the language corrections. These will be implemented in the revised version.

------------- The language corrections will not be included in the public discussion.--------------
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Minor Comments:

Comment:

Lines 59-61: Try to rephrase the sentence to avoid double ‘which’.

Reply:

Done.

Comment:

Line 80: In the title of Table 1, MRs of Nordic Meteor Radar Cluster better be remarked in parentheses.

Reply:

Done.

Comment:

Line 91: define any acronym before the first usage. The NORDIC is already used in the abstract.

Reply:

Done.

Comment:

Similar errors can also be found on Line 109, World Geodetic System (WGS84)

Reply:

Acronym introduced and citet.

Comment:

Line 97: ‘minimize the residuals of the projection’ is better?

Reply:

Changed.

Comment:

Line 97: is it better to change ’radial wind’ to ‘radial velocity?

Similar expression can be found on line 135, etc.

Reply:

Changed throughout the manuscript.

Comment:

Line 100: off-zenith angle is not often used, may be try zenith angle directly. This should define the same angle, although they seem different.

Reply:

We added a small line explaining that we use both terms in a similar meaning.

Comment:

Line 142: physically

Similar errors can be found through the paper, such as mathematical and physical consistent. Please correct them in the revised paper.

Reply:

Changed throughout the paper.

Comment:

Line 174: change to ‘monostatic meteor radar’ for the direct meaning and unified expression.

Reply:

Changed.

Comment

Figure 4: the missing of degree symbol on the declination axis label
Reply:

There is a degree label, but it is difficult due to the colors below and above the celestial equator. We will update the figure with a different color scale.

Comment:

Lines 244-245: Figure 5 also includes the results using TDF data. You should add it to the sentence.

Reply:

Added.

Comment:

Lines 314-315: ‘a vertical dimension of approximately 20-40 km’ ? I suppose the original meaning should be the horizontal dimension, i.e. the area of a grid cell.

Reply:

The statement refers to the vertical extension of the column above each grid cell. We run the retrieval within the vertical domain from 70-110 km. So the maximum thickness is 40 km, however, very often we can only retrieve winds between 80-100 km and, thus, corresponding to a 20 km thick layer.

Comment:

Figure 7: typos: change ’merid measurement response’ to ‘meridional measurement response’

Reply:

Changed.

Comment:

Second, zonal and meridional wind field are plotted separately in two panels, but with same 2D wind vectors. It's kind of confusing at first look. Why don't use the pattern of Figure 4 in (Stober,2021a), since no specific points are mentioned concerning the difference between zonal and meridional results.

Reply:

Figure is updated.

Comment:

Figure 9: the left column represents vertical and the right column represents vertical (div), but no explanations are given for the exact meaning. Though I notice it is mentioned after Figure 10 on line 363.

Reply:

We added this information in the caption.

Comment:

Line 394-395: try to rephrase the sentence. For example, the distribution of vertical velocities inferred by the meteor radar and the UA-ICON model

Reply:

Rephrased.

Comment:

Line 395-397: The meaning of this sentence is not that clear to me. Since the debiasing algorithm has been applied to the data to obtain a more statistically accurate vertical velocity, it's not that appropriate to say your results are 'residual bias vertical velocity'. After all, we can't solve the vertical velocity 100 percent right.

Reply:

The presented methods for the monostatic meteor radar data analysis reduce the bias. A 100% correction would also mean that we know the 'true' value of the vertical velocity. The 3DVAR+DIV algorithm is more robust in that sense and at least the variability should be very accurate for the scales we are sensitive to. However, the absolute statistical mean still depends on the lower integral boundary.

Comment:

Line 447: change 'associated due to' to 'associated with'

Reply:

Changed.

Comment:

Lines 495-496: Based on the assumption that the scattering center will change along the meteor trail, you have mentioned that forward scatter systems are more prone to this effect compared to monostatic systems. So why only monostatic meteor radars are faced with the additional challenge?

Reply:

Thanks for pointing out the remaining ambiguity in wording. Both techniques are affected and it is even more critical for forward scatter systems.

Comment:

Line 504: do you mean sigma, i.e. the standard deviation? sigma squared is the variance.

Reply:

Changed.

Dear Wayne Hocking,

Thank you for providing feedback on the submitted manuscript. The raised concerns are going to be included in the revised manuscript. We provide in this reply more details on how the manuscript was revised compared to the public reply. All changes are available in a tracked changes version. We really appreciate the constructive comments and changed the manuscript accordingly. In particular, we thank the reviewer for triggering the scale analysis presented in appendix A, which underlines the validity of the incompressibility assumption in the framework of the linear theory for medium frequency gravity waves.

General comment:

This paper attempts to use multi-static meteor radar networks to measure vertical velocities(*w*) in the mesosphere. It is hampered by the fact that there are so few useful measurements of *w* against which the data can be compared. The authors have done a reasonable job of using other available information to offer some insights into the effectiveness of their procedure. There are quite a few grammatical and typographical errors and even a couple of other missing references that could be useful.

I will start with more major points, then switch to grammatical issues.

General reply:

Vertical wind observations have been challenging at the MLT and each observation has its own sensitivity due to the observational filter intrinsic to all instruments. The main idea for this paper is to reduce the biases associated with meteor radar observations and to outline the potential of more complicated forward scatter models to at least get physical and mathematical sound solutions for the vertical winds, although a detailed comparison remains a future task.

SEMI-MAJOR ISSUES

These items are discussed in the approximate order in which they appear. Items 1and 2 are somewhat optional but should be considered. Items 3 and 4 are more important. Item 4 brings up a very important term not included in the authors' equation (4). Item 5 deserves more discussion.

Comment:

Item 1. Lines 50-51. The authors should perhaps be aware of the processes of "Stokes drift" and "Stokes diffusion" - especially Stokes drift - , which can also create artificial apparent vertical winds (e.g.https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/91JD02835.

This paper also discusses the potential role of the fall speed of charged aerosols. also see

https://doi.org/10.1175/1520-0469(1991)048%3C2213:SDBAIG%3E2.0.CO;2)

Reply 1.1:

Thanks for pointing at the ‘Stokes drift’ issue, which is important for the discussion and interpretation of the obtained vertical winds. We will add a paragraph explaining how Stokes drift has to be considered in understanding the retrieved vertical velocities.

Comment:

Item 2, Line 217. This discusses the "Tikhonov matrix", but not that well. I found the Wikipedia discussion better. But I am left confused. For example, in line 219, it says *"and the superscripts denote the Euclidean norm"*. WHAT superscripts? Are you talking about the "squared" terms? The double vertical lines apparently refer to "residuals" (which the authors should mention). I assume these are some sort of determinants - whatever they are, please define their meaning. And it is said that the Tikhonov matrix is "empirically determined", but not further explained - seems to give a lot of arbitrary selectivities. Maybe it is discussed in Stober et al., 2017, but I have not looked at that in a while, and do not have time to do so now. The authors say also in line 224 that *"The most straightforward approach is to use the unit matrix as Tikhonov regularization"*. What do they mean by a "unit matrix"? Is it a unit diagonal matrix? Or are all off-diagonal terms 1 as well? The off-diagonal terms seem to be key to the Tikhonov process, but I would have expected their contribution to be less than the diagonals.

Reply 2.1

We will put a more comprehensive description of the equations but want to avoid a review of the Tikhonov regularization formalism. The two vertical lines denote the norm. The simplest Tikhonov matrix is the identity/unity matrix with ones on the main diagonal and zeros for all other elements. This is often used in damped least squares. However, a bit more sophisticated approaches estimate the diagonal elements through machine learning or by other apriori knowledge.

The generalized case or Spatio-temporal Laplace filter for the monostatic retrieval is more complicated and we will put a schematic in the appendix. Stober et al., 2018 contains a scheme (equation 11) on how this matrix looks for the 2DVAR retrieval. This scheme is applicable to the monostatic case with one dimension as time and the other dimension as height. As assumed by the reviewer, the main diagonal elements take larger values than the off-side elements. However, instead of solving the complicated large matrix inversion as it is required for the 2DVAR, 3DVAR, and 3DVAR+DIV, we implemented a block diagonal approach and simplify the Laplace filter to take again a 3x3 form with only diagonal elements.

The 3DVAR and 3DVAR+DIV leverage similar approaches. There is even the possibility to retrieve wind fields involving more complicated structures of these matrices with off-side diagonal elements.

Comment 2.2:

** Further, does the "choice" of the matrix and multiplier depend on the data set? or on the nature of the radar network (e.g. CONDOR vs NORDIC)? Or is it somehow universal?**

Reply 2.2

The choice of the Lagrange multiplier is for both networks the same and pretty robust. We used values between 0.4 and 0.2. The standard analysis solves the equations with 0.4, which ensures a bit more numerical stability at the cost of a lower measurement response. We compared the wind fields and the differences between both solutions at around 90 km were smaller than 2-3 m/s for individual solutions in the grid cells for the horizontal winds.

Comment 2.3:

I also do not like the wording "which in consequence leads to a strong damping of the vertical velocities" in line 223. It gives the impression that you are damping real velocities. You could perhaps say the wording "which in consequence leads to a strong damping of artificially large vertical velocities", but that still leaves the reader open to asking "if it suppresses artificially large velocities, does it also suppress real ones?" And does it suppress artificially small speeds even more? Or can it make artificially small speeds bigger?

Reply 2.3:

Thanks for suggesting the changes to the wording. The last three points are to some extent true but with decreasing effect the more sophisticated the algorithms. The monostatic analysis certainly assumes a zero mean vertical wind as apriori, which appears to be a reasonable assumption. Both Tikhonov regularizations presented for the monostatic meteor radar data analysis permit now a deviation from the apriori information depending on the covariances. The more significant a solution of the least-squares is, the smaller the retrieved covariance is. However, this doesn't imply that the solution of the vertical wind is smaller or large. The next step for the monostatic systems is to compare to other data to quantify additional problems related to the last part of the reviewer comment and to extend the machine learning approach by introducing new metrics to estimate data-driven the Tikhonov matrix, instead of using statistical means.

The 3DVAR+DIV algorithm already overcomes these issues. In particular, as we permit a deviation from the incompressible solution.

Comment 2.4:

Seems there are too many "choices" for my liking.

I wonder if an appendix could be added that briefly expands on the technique, including concrete examples from this particular paper?

Reply: 2.4:

Every analysis includes 'some choices' on how things are implemented. A least-squares fit can be done with statistical uncertainty weighting or without. One could implement non-linear error propagation. The fitting can be done including a whitening filter or without and every choice will have some impact on the final result. The presented retrievals demonstrate how obvious issues can be mitigated and we outline on how these issues can be addressed mathematically.

Comment 3:

Item 3, Sections 2 and 3. These sections claim to cover all of the likely biases and errors encountered in the process of data extraction. Yet the paper doi.org/10.1186/s40623-018-0860-2offers insights not discussed here, and *should* probably be referenced. This especially looks at the errors associated with the common 5-antenna interferometer - introduced first in https://agupubs.onlinelibrary.wiley.com/doi/epdf/10.1029/97GL03048, and even earlier in Lightning studies (Rhodes et al., JGR, 99, 13059, 1994) - and is of value here (This structure is at times called the "Jones interferometer", though it was developed in multiple fields around the same time and Jones was not the first). Errors with this interferometer can be ~0.5 to 1 degree, so at a range of 150 km from the receiver, this is a possible error of ~ 3-4km - possibly in height -and this could have implications for the data. One other point of interest is the fact that for radar reflections along the line of sight between a transmitter and a receiver, with a meteor trail positioned halfway between them, the Doppler shift is dominated by vertical motions. Unfortunately, this configuration also requires a horizontally aligned meteor trail, and such trails may be rare. But if found, such a trail would be invaluable as a means of determining a "typical" vertical velocity. A large number of such trails, even if scattered in time and height over many years and many sites, could collectively make an important database for the expected variations of vertical velocities, independent of some of the assumptions and interpolations made within this text. Maybe not in this paper, but an idea for future applications. Nevertheless, the paper deserves a mention and should be referenced.

Reply 3.1:

The interferometric errors are included in the non-linear error propagation. We use a standard error of 2 degrees. As discussed above any error in the interferometric solution is related to an error in the altitude. This additional altitude uncertainty is computed in the spatio-temporal Laplace filter by estimating the vertical shear. We will add a paragraph on this source of uncertainty as suggested including the references.

The second aspect mentioned above is the possibility to use meteors trails that are detected in a small ellipse between the transmitter and receiver (halfway) as a proxy for the short-time and small-scale variability of the vertical winds. This is indeed possible. However, these meteors are representative of the small (few kilometers) and shortest time scales (seconds) comparable to MST-radar beam volumes (Gudadze et al., 2019) and do not provide too much information on the hourly mean of the entire observation volume due to the low statistics. We use this information for the momentum flux retrievals (Stober et al., 2021), but these are not a topic for this paper (see supplement material).

Comment 3.2:

In regard to comparisons with other distributions of vertical velocities, the authors do not make much use of PMSE and MSE velocities determined with narrow beam wind profiler radars. This is discussed briefly on pages 3 and 19-20, but concern is expressed about the role of falling aerosols. Yet if the mean is removed, the resultant stand deviation surely gives some idea of what sort of variances one might expect - is that not useful information?   It would seem that such a data set might be useful as a reference against which their data can be compared in a general sense. It is true that PMSE and MSE may be extra active, and so provide over-estimates, but at least they will place limits on the likely values. **Lines 404-411 are an important comparison and are well noted**.

Reply 3.2:

We will expand the MST-radar part on the vertical velocity variability based on the extensive data set of Gudadze et al., 2019. These results are not contradicting the meteor radar retrievals mainly as much smaller spatial and temporal scales are analyzed and, thus, the variability is supposed to be increased concerning our retrievals. We also agree that narrow beam MST-radars present a high-quality asset to measure reliable vertical wind variability, although the scattering physics has to be taken into account for middle atmospheric observations.

Comment 4:

Item 4. Line 293. This equation seems to have an important typo.

The typo is as follows: the "div" term must involve div (\rho u)

Or you can make the first term D(\rho)/Dt (advective derivative) and then you can retain the \rho div(u) option.

Reply 4.1:

We will define an operator and describe the other terms, instead of the mix of global and local derivatives.

Comment 4.2:

Something more of a justification of why the term \rho div(u)) is the dominant term is needed.

It would be of interest for the authors to use their UA-ICON computer model to create an artificial "meteor data-set" for a pure tide, and see how well their new wind-determination software reproduces the tide. Maybe it will work- I do not know. I find the arguments presented in lines 293 to 295 a little too glib. An alternative id to at least derive the relative strengths of th terms for the case of a propagating gravity wave e.g. use the equations 11.7 to 11.22 from https://www.cambridge.org/core/books/atmospheric-radar/49A1C21C0631CDE6AA5CA95AA91C39E5

to estimate the relative contributions of the terms,

Reply 4.2:

We will add some estimates for some examples outlining the order of the different terms. We intentionally compute both the incompressible and non-stationary/compressible to capture both cases. So far both solutions stayed within 5-10% in the derived vertical velocities. Typically, the non-stationary/compressible solution leads to higher values in the vertical speeds compared to the incompressible estimates. Although, we did not yet investigate, which of the non-stationary terms or spatial derivatives of advective terms is more relevant. Due to the temporal derivative in the continuity equation, we already prepared a pipeline of a 4DVAR+hybrid approach or when combined with a complete momentum budget of the Navier-Stokes equation a 4DVAR forward model.

Comment 4.3:

I'm actually a little concerned about this "merger" of experiment and theory. Assuming that the divergence theory works over such large scales is an assumption - in real life, sometimes the "balance" is achieved with vigorous small-scale events at scales like (tornadoes) much smaller than the grid size. Maybe it is OK, but it needs caution. I especially dislike lines17-18, which say " which is consistent to the values reported from GCMs for this time scale and spatial resolution". I personally am not prepared to accept that a model makes a suitable proof of an experimental result, though maybe it is more common these days. I will not insist in the removal of this line - but I do wish to record my concern.

Reply 4.3:

We will reconsider the wording and add that other observations are needed to provide an independent source of validation. These measurements should cover similar spatial and temporal scales or permit the application of an observational filter comparable to a meteor radar to ensure a meaningful cross-comparison.

Comment 4.4:

I appreciate the effort of the authors - but I also issue a warning about taking care here. There is much to be said to keep experiments free of theoretical bias!

Reply 4.4:

The monostatic data analysis including the Tikhonov regularization was implemented before UA-ICON was available. There was no tuning or anything comparable done to make the histograms fit better. However, UA-ICON is only one GCM and other models might yield different values for the vertical winds.

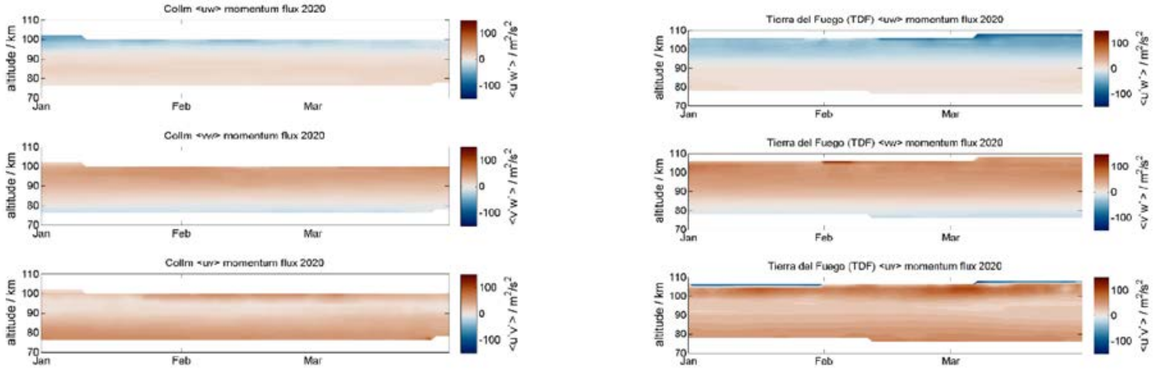
Comment 5:

Item 5. In Figs 1, 5, 6, and 10, there is a persistent theme that a narrower distribution of vertical velocities is more realistic, and the narrower, the better. Certainly, it is true that some of the original spread is partly due to random errors, but one has to wonder whether the Tikhonov process could go "too far" and "over-suppress" some points. I think the authors have tried to address this possibility, but there is room for doubt. The proposal is to use a select group of near-horizontal meteor trails (which selectively measure only vertical velocities due to their location close to the midpoint between the transmitter and the receiver), as discussed in item 3 above, might be of merit here.

Reply 5:

The 3DVAR+DIV algorithm does not suppress realistic velocities. The retrieved vertical velocities are physical and mathematical sound solutions within the assumptions of the forward model. If the continuity equation doesn't hold at all at the MLT, not even as a statistical mean then our retrieval will suffer from this as well. The small-scale variability that is again mentioned as a concern is also well-captured by the retrieval, but not attributed to a statistical mean vertical velocity. Embedded to the analysis routine is the momentum flux retrieval, which also requires reliable vertical wind variability to derive the Reynolds stresses (Stober et al., 2021). Although momentum flux and wind variance are not the main themes of this paper, they somehow reflect whether the small-scale gravity wave features are statistically well-captured.

Individual vertical velocities of meteors directly overhead the monostatic systems or at the mid-points of the forward scatter configurations provide some insight into the vertical wind variability for the smallest spatial and temporal scales similar to MST-radars with a narrow beam. Looking at line of sight velocities at the mid-point data was not very successful. We had to use filtering criteria of about 5-8 degree around the perfect mid-point. The resultant line of sight velocities was still substantially contaminated by horizontal winds. When putting more strict criteria on the filtering only a small integer number was left scanning data from March 2020 above CONDOR.



We thank the reviewer for correcting the language mistakes.

==

From here on, I turn to grammatical and typographical errors.

Comment:

Line 1 (abstract):

Abstract. Meteor radars have become a widely used instrument to study atmospheric dynamics, in particular in the 70 to 110...

radars is plural, so instrument should be as well. Also delete "a". Also I recommend "in particular in" --> particularly in...

So I recommend using

Abstract. Meteor radars have become widely used instruments for studies of atmospheric dynamics, particularly in the 70 to 110...

Reply:

Changed.

Comment:

Line 32

.... Altitude Mechanistic Circulation Model (HIAMCM) with a horizontal resolution of about 55 km, vertical wind velocities up to 3 m/s are....

velocity is a vector, so has a sign - recommend using "speeds" here viz.

.... Altitude Mechanistic Circulation Model (HIAMCM) with a horizontal resolution of about 55 km, vertical wind speeds up to 3 m/s are....

Reply:

Changed

Comment:

Line 62 - references - maybe add

https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/96RS03467

since this was the first paper to use the (now common) meteor wind technique that rejects outliers in data-clusters in finding mean winds. Optional.

Reply:

Reference added.

Comment:

L 109 - add a reference for "WGS84 Geometry" - not everyone will know what this is.

Reply:

The acronym is now explained and citet.

Comment:

L 118 - "substantial biases" - in what sense? I assume you are thinking about the wide "tails" in the distribution. Or are you talking about biases in the location of the peaks (there appear to be none)? Be more specific.

Reply:

Rephrased.

Comment:

** L133-134 - all "radial velocities and their interferometric locations are EXACTLY DETERMINED..."? (synthetic data). This seems to make Fig. 1 an unfair comparison.   Even the best interferometers have angular errors (0.5 to 1 degree or so), and pulse -lengths are normally 2 km or so, so it's NOT exact! Errors in location can be 3-4 km and more - that is especially important for HEIGHT errors (and the height error is NOT just the pulse length - there is also a contribution to the height resolution from the angular uncertainty, especially for off-vertical meteors). This is unavoidable - so it would be of interest to see what the right-hand synthetic data shown in Fig. 1 look like WITH such unavoidable errors included in the simulation. That would give a better feel for how necessary the Tikhonov corrections introduced later are.

Reply:

We added a paragraph how we consider the angular errors due to the antenna arrays. The recommended citations are included.

Comment:

In line 134, I'd remove the comma after "thus,"

Reply:

Done.

Comment:

L 148 - insert "radio" before "energy"

Reply:

Done.

Comment:

L 157 - t_0 point - sometimes the t_0 point can refer to the time at which the FRONT of the trail passes closest to the radar - this is especially true when examining pre-t_0 Fresnel diffraction patterns (used to find entrance velocities e.g. https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/1999RS002283, fig.2). Please check against McKinley - I'm not sure if there is some confusion here, or whether the same terminology is used in 2 different ways.

Reply:

The same terminology is used in two different ways. We added the citation.

Comment:

L 164 - last 2 words "more slant"?? Do you mean "more horizontal" or "more vertical"? In any case, please tidy this up - "more slant" is bad grammar. Maybe say "slanted more horizontally" or "slanted more vertically", as apropriate?

Reply:

Rephrased.

Comment:

Fig. 2. Might be worth noting (maybe in the caption) that the trail could be up to 5 km and more long?

Reply:
We added a sentence in the caption of the Figure.

Comment:

Fig. 2, 3rd line - is it worth changing "... location of the scattering center assuming it stays glued to the trail.." to "...location of the scattering center assuming it stays glued to the same midpoint of the trail..."??

Reply:

Changed.

Comment:

L 175 "basically overlapping" - can you be more specific? What is the separation of the centres?

Reply:

More quantitative numbers added.

Comment:

L 180 ".. commercial software.." - which commercial software? The reader deserves to know.

Reply:

Citation added.

Comment:

L 184 - remove "and" before "altitude"?

Reply:

Done.

Comment:

249 "Although, however, ..." -- maybe try

"However, we do recognize that while the generalized Tikhonov...".

Reply:

Done.

Comment:

L 276 - first word "the" - change to "other"?

Reply:

Done.

Comment:

L 354 - "Similar" --> "Similarly"

Reply:

Done.

Comment:

L 363. " The incompressible solution (vertical(div)) exhibits an approximately 20% reduced standard deviation for the same periods" - OK, but is this necessarily good, especially in view of Item 4 above, which questions the accuracy of eq. (4)? Being smaller is not always a good thing, especially in view of the "damping" suggested in line 223 (which I have already suggested needs to be reworded anyway).

Reply:

We reworded that statement.

Comment:

Fig. 7 - one has to look into the text to see whether "1" means a good or bad response! This should be mentioned in the caption. Also, the right-hand lower graph shows a position of expected poor performance half-way between the 2 red dots, (as expected) but the left hand one does not, which seems odd.

Reply:

Information added. Please not that the measurement response depends for each component on the geometry. For instance, the zonal wind component is sufficiently well determined between the stations Sodankyla and Kiruna, but the meridional component is more difficult as there is no good overlap to Alta and Tromso to compensate for the geometry.

Comment:

Fig. 9. The terminology "vertical (div)" on the right is unclear until one checks the text - it would be useful to say it is the "incompressible" model at least within the caption. Of course this depends on resolution of item 4 above.

Reply:

Text added.

Comment:

COMMENT ONLY: Lines 404-411 are an important comparison, and probably the most useful of all comparisons. Could be mentioned earlier.

Reply:

Due to some other changes in the revision, the paragraph remained unchanged. We agree that this passage is valuable.

Comment:

L 436 - I suggest using "The algorithm permits THE USER to ..."

Reply:

Done.

Comment:

L 439 - "expectable" is not a word - change

" it is also expectable to observe a higher variability and larger vertical wind magnitudes"

to

" it can be expected that a higher variability and larger vertical wind magnitudes might be expected."

Reply:

Changed.

Comment:

L 447 - change "Due to the large vertical shear often associated due to large scale waves such as tides this increases the tendency for numerical instability that has a negative effect on the reliability of vertical winds."

to

" Due to the large vertical shear often associated WITH large scale waves such as tides, this increases the tendency for numerical instability, WHICH IN TURN has a negative effect on the reliability of vertical winds.

I agree that horizontal shears can have a big effect on derived vertical winds.

Reply:

Reworded as suggested.

Comment:

L 457 - remove "a" before "good"

Reply:

Done.

Comment:

L 485 - "since" --> "for"

Reply:

Done.

Comment:

L 486-7 - refers to full-wave simulations, but no reference is given. At least a "personal communication" indicator should be added.

Reply:

Reference added.

Comment:

L 491-2 - change "IS DRIFTED" to "drifts with"?

Reply:

Done.

Comment:

L 622 - why is the title capitalized here?

Reply:

Latex entry repaired.