EGUsphere, author comment AC3
https://doi.org/10.5194/egusphere-2022-203-AC3, 2022

Reply on RC3

Gunter Stober et al.

Author comment on "Meteor radar vertical wind observation biases and mathematical debiasing strategies including the 3DVAR+DIV algorithm" by Gunter Stober et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-203-AC3, 2022

Dear Wayne Hocking,

Thank you for providing feedback to the submitted manuscript. The raised concerns are going to be included in the revised manuscript.

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 the same for both networks 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 was 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 the data-driven 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. For example, 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 supplementary 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. The statistics will be rather low, but we will try to define an overhead region and see how many meteors we will find satisfying the selection criteria.

We thank the reviewer for correcting the language mistakes.

Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-203/egusphere-2022-203-AC3-supplement.pdf