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Comment on egusphere-2022-203

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Referee 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-RC3, 2022

Referee report on the paper

Meteor Radar vertical wind observation biases and mathematical debiasing strategies including a 3DVAR+DIV algorithm

by

Gunter Stober et al.

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.

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.

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)

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.

** 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?**

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?

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?

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.

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**.

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.

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 contribtions of the terms,

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.

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!

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.

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

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...

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....

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.

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

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.

** L133-134 - all "radial velocities and their inteferometric 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, espciaally 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.

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

L 148 - insert "radio" before "energy"

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.

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?

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

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..."??

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

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

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

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

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

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

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

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).

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.

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.

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

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

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."

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.

L 457 - remove "a" before "good"

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

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

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

L 622 - why is the title capitalized here?

==========END==========