Final Response to reviewers' comments

`Ref: AMT-2022-159`

Author's response

We thank both reviewers for their valuable suggestions, which significantly improved the manuscript. The revision incorporates all the suggestions of reviewers 1 and 2 as mentioned in our point-by-point responses (AC1 and AC2) submitted in the discussion phase.

The revised paper addresses two major concerns of reviewer #2.

1. Quantitative validation of the method with synthetic data (Added sections 2.3 and 3.1).
   and
2. Comparison of motion estimated using phase correlation and optical flow methods (Added sections 2.7 and 3.5).

In this revision, the following major changes are made to the manuscript to incorporate the reviewer's suggestions.

1. Instrument photo is added as suggested by reviewer #1 (Fig. 1).
2. Constructed data section 2.3: detailing the steps for generating validation data.
3. Optical Flow section 2.7: Brief description of optical and the specifications of the method presented in this paper.
4. Validation section 3.1: contains a new validation figure (Fig. 3) and a quantitative table showing errors (Table 1).
5. Comparison with optical flow section 3.5: contains two new figures (Fig 12 and 13) comparing PC and optical flow methods and showing the effect of postprocessing.
6. The two figures from the appendix showing examples of raw motion vectors are removed. Figure 12a is now showing an example of the raw motion vectors for PC and OF both.
7. Figure 9 (Now Figure 11) is updated as per reviewers' suggestions.
8. Sections 1, section 2.2, and section 4 are significantly modified with the new discussions, references, and added mathematical details. (See attached diff.pdf)
9. Mathematical details of NMF methods as well as PC and OF methods are provided (Sec 2.2, 2.4, and 2.7).
10. Explanations of U and V components as well as software package information are given as suggested by reviewer 1 (Section 2.2).
11. The title of the paper has changed to reflect the new additions. "Cloud motion on the edge with phase correlation and optical flow"
12. We have also incorporated all the minor suggestions of the reviewers as mentioned in AC1 and AC2.

We hope that the revised version of the paper is suitable for publication in Atmos. Meas. Tech.

Reviewer 1

General comments:

The subject of the paper is relevant to the subject of this journal. The paper contains some new data relevant for publication. The presentation is clearly described, also the length of the paper is adequate. The title and the abstract are pertinent and understandable.
The authors give proper credit to related work and clearly indicate the own contribution.

We thank the reviewer for providing encouraging comments and valuable suggestions that improved the paper's readability. Our responses to specific comments are below.

Specific Comments:

> - Section 2: the tools applied are not explained (e.g. which programming language, libraries for image processing, etc.)

We provided the Python source code of the Sage plugin in the code availability section. For readers' convenience, we have added the following statement in section 2.2:
The above implementation of the PC algorithm is available in several programming languages, notably C++, Python, and R in packages openCV (mulSpectrums), SkImage (phase_cross _correlation), and imagefx (pcorr3d) respectively. For this study, we used the Python implementation of Picel et al. (2018) using NumPy and SciPy packages. (See code availability section).

> - Section 2.1: an image of the used instrumentation (TSI, Sage camera) would be instructive.

We have added Figure 1 showing a TSI camera system and a Sage node with sky facing camera and other sensors.

> Also, explain "CEIL".

Thank you for pointing this out. We have now changed to using the full name for the ceilometer. The description of the working of the Ceilometer is also rearranged for clarity (See section 2.1.3). We hope this change makes the text easier to follow.

> - Section 2.2: Explain in more detail the phase correlation and FFT methodology, not just refer to the paper of Leese et al.
>   C(mu, nu) is not explained.

The PC method is now described in more detail with added reference in section 2.2.

> Section 3: The U and V components are used widely in the manuscript but nowhere explained in detail.

We thank the reviewer for suggesting this. The meaning of U and V components and their association with the X and Y image axis is now described at the end of section 2.2.

Reviewer 2

Summary

> This paper by B. Raut, S. Collis, N. Ferrier, P. Muradyan, R. Sankaran, R. Jackson, S. Shahkarami, S. Park, D. Dematties, Y. Kim, J. Swantek, N. Conrad, W. Gerlach, S. Shemyakin, and P. Beckman discusses the retrieval of cloud motion vectors from distributed sensor systems (called sage nodes) equipped with a sky-facing camera. The paper discusses both total-sky imager (TSI) and Sage cameras. The idea is to use the phase-correlation (PC) method, which relies on fast Fourier transforms to obtain cloud displacements in predefined block cells, computing cross-correlations on successive images; a big advantage of this kind of method being that it is not computationally intensive. This very last point is strongly emphasised in the article, as the analysis is to be run on embedded computers. Discussions that follow are then meant to help decide how to optimise the retrieval algorithm to obtain the best results.
> The original idea is good and sound, and it is also good that the paper presents field data that have been taken at the ARM research facility: "To validate the estimates of the CMV in our work, measurements from the co-located ceilometer and the wind profiling radar (...) were used from the SGP C1 site from October 14, 2017, to August 14, 2019." (lines 88-90).

We appreciate a thorough review of the paper. The major suggestions by the reviewer are incorporated into the revised manuscript which considerably improved the content of the paper.

The reviewer's main concerns are related to the wind data comparison (sec 3.3) and the validation of the algorithm. We are showing validation of the algorithm using synthetic data in Section 3.1. We have also added the comparison with the optical flow method in section 3.5. We hope the issues highlighted by the reviewer are properly addressed by this.

> While this sounds promising, as it stands this work remains far too qualitative, however.
> Notably, when comes the time for validation, which should be of utmost importance, lines 194-195 actually read "(...) this comparison may not be interpreted as a quantitative validation of the algorithm (...)".

Lines 194-195 refer to the wind data comparison and the reason for this is given in the same paragraph of the paper and also above in point 3. In that section, we compared approximately 876 hours (selected for cloudiness) of valid wind data to show the long-term stability of the method. The uncertainties involved in such comparison are mentioned in the same paragraph of the paper. We have now clarified this in the following sentence.

"Therefore, this comparison may not be interpreted as a quantitative validation of the algorithm ***for wind retrievals****, however, significant correlations of the magnitudes indicate that the estimates of the instantaneous CMVs from the camera images are stable over a long period."*

> By the end of the paper, the reader is left wondering how good those retrievals actually are. The same remark holds regarding how one could meaningfully tune an algorithm that is never quantitatively compared to something else.
> More work and quantitative results are therefore needed before I can recommend its publication in Atmospheric Measurement Techniques.

We accept the reviewer's suggestion of adding a separate validation section in the paper.
However, the validation part is not completely missing in the current version of the paper. We stated in

section 3.2.1 that cloud motion should be stable on a minute-by-minute scale and hence random fluctuations in the CMV are due to the algorithm's errors. Zhen et al. also used the same assumption for validation and they counted large random fluctuations as an error. The lagged autocorrelation is a widely accepted method of quantifying randomness. In our case, if the algorithm were to produce completely noisy values (i.e. zero skill) the correlation (in then Fig 7 now Fig 8) would have fallen to insignificant levels at lag-1.

Allowing that the FFT-PC algorithm is widely used in cloud motion estimation for decades, several other studies have reported the results (e.g. Leese et al. 1971, Schmetz et al. 1993; Kishtawal et al. 2009) as well as a comparison of the method (e.g. Zhen et al. 2019, Sawant et al. 2021 and references therein).

_We have provided detailed validation in two parts in earlier the response (See AC2) and now added validation in Section 3.1 as well as comparison with optical flow in section 3.5.

Response to Reviewer's comments

> The main issue is that, beyond the mere existence of correlations, there must be more work to provide quantitative assessments in the validation (Sec. 3.3). How good are the retrieved wind components U and V? This must be clearly communicated. It cannot just end on a quick qualitative note. The strategy to derive trustworthy winds from this should be spelled out.
> Other similar peer-reviewed papers are not satisfied with autocorrelations and a qualitative comparison. They do compare against other methods, use synthetic datasets, use quantitative metrics, ...

Detail response to this comment has been provided in the author's comments AC2. We have now added a validation section 3.1 with a table to the revised version to alleviate this concern of the reviewer.

> Figure 9 is not really readable and not convincing. The units for both the x and y axes must be displayed. Moreover, in the text the attention of the reader should be brought to the fact that the ranges spanned in x and y are widely different, which can be misleading.

Done. See Figure 11 and the text in section 3.4.

> The accompanying discussion should address the issues that appear in Figure 9, e.g. if all was perfect, should these be comparable (i.e. essentially follow a y = x line)? If yes, then it should be discussed.

Detail explanation of this has been provided in the author's comments AC2. We have now clarified this in section 3.4.

> The rain points are deemed problematic, but the remaining points do not seem much better.

Detail explanation of this has been provided in the author's comments AC2. We have further clarified this in section 3.4.

> More quantitative information must also be given and discussed together with the figure, for instance in the form of root-mean-squared-differences and biases, applied to the difference between the value obtained from the algorithm and the expected one used for validation. This should come with a discussion, especially if the discrepancies are large.

Table 1 is added to address this concern.

> Several other peer-reviewed works similarly dedicated to cloud motion, sky-looking cameras, TSI, and using various techniques (both block-matching and optical flow; both from the images themselves, or going to Fourier space) do compare their results against other established methods. Those include for instance Zhen et al. (2019) (which is cited in the current paper) and Peng et al. (2016). Depending on the approach, a number of evaluation metrics are also given e.g. in Peng et al. (2016). Something along such lines is possible for the current paper. Such a comparison could be done on a more powerful machine, since the idea is to validate. To be recommended for publication, the paper should at least clearly show that the retrievals make sense.
> Alternatively, they could also use synthetic datasets, once again as done in other similar works such as Zhen et al. (2019) and Peng et al. (2016).

Thank you for making this suggestion. We have presented the validation results in section 3.1 and optical flow results in section 3.5, which have improved the manuscript. We hope that our response to the earlier suggestions clarifies this issue.

Response to further key issues and comments:

> 1. The paper should explicitly discuss the well-known issue of multilayer clouds, which is currently not mentioned though it is discussed among other problems in Leese et al. (1971) and Zhen et al. (2019), which are both cited as PC-method references in the current paper (lines 19-20).

We thank the reviewer for bringing up this point. This is an interesting issue that the tracking algorithms have to deal with. The introduction section of the paper referred to the clouds moving in different directions (multilayer). Therefore the block-wise algorithm is suggested to mitigate the issue. _We have added a reference to Peng et al in the introduction section and explicitly mentioned the multilayer clouds.

> -- This is from Zhen et al. (2019) (their page 2): "However, as shown in the prior work, FPCT [Fourier phase correlation theory] is unable to recognize multiple motion displacement vectors from different cloud layers because it can merely extract one displacement value for a global image [46].

This is true when FFTPC is used over the entire image (as in Zhen et al) and this issue can be effectively mitigated using the blockwise method which is discussed in this paper.

> Interferences such as sky background effect, pixel superposition, the motion of multiple cloud layers, and irregular cloud deformation can all cause random noise leading to significant displacement calculation errors.".

We agree with the reviewer. These issues are true for any cloud tracking algorithm. Such errors usually appear as fluctuations from the surrounding motion vectors and can be removed by various methods. Fig 10 in Zhen et al shows that the performance of the pure FFTPC on global images is as good as other preprocessing methods but with not consider the outliers that can be easily removed, except for the optical flow underperforming FFTPC. Their Fig 11 also shows outliers in transformed CMVs which are cleared thanks to the ensemble method.

_We used Westerweel & Scarano (2005), the details of which are now added in the revised version.

> -- Zhen et al. (2019) also add, on their page 3: "According to the algorithm principle of FPCT, the cloud displacement calculation result is either correct or unacceptable. The probability for correct

> result depends on the noise intensity." Note that they then move on and discuss the "low robustness of the FPCT method".

Thank you for quoting this from Zhen et al. We have seen similar behavior (See Figure 3). *This is now discussed in the validation section 3.1.* Please, check AC2 for a detail response.

> The second quote moreover emphasises the issue of not performing preprocessing in the current paper and seems to contradict the claim that "The PC method can be implemented without preprocessing images" (lines 36-37). Again, I do stress that Zhen et al. (2019) is a reference of the current paper for the PC method.

A detailed response to this comment is provided in AC2. We have now shown the comparison of optical flow and PC methods in Section 3.5.

> 2. The authors should also add some discussion in the paper on the issue of image distortion and how it can quantitatively affect their retrieved winds. Indeed, for TSI (also used in this work), "image distortion compromises the accuracy of the detected motion vectors, especially around the boundary of an image" (Peng et al. (2016)). With the PC method, the distortion issue does not vanish once we go to Fourier space. From the images and animations from the current paper, no filtering seems to be performed on the edges though (in fact, arrows are even derived for the TSI supporting arm).

We couldn't agree more with the reviewer's concerns. The distortion at the boundaries can not be simply removed by regrinding the hemispheric data to a square image. A more calibrated de-warping method may be helpful. In our case, the boundary of the images is excluded from the mean calculation for the same reason. The discussion is now added in section 4.

> 3. The need for a not-too-heavy algorithm is emphasised: e.g. 'computational overhead complicating their use in real-time applications' (line 23), 'computational efficiency of the algorithm is critical' (line 36), '[FFT] is computationally efficient, and hence a natural choice' (line 40). However, it would seem that the objective should at least be to obtain usable retrieved winds (a too simplistic algorithm could easily lead to poor retrievals, as already stressed). While the authors do not give the technical specifications of the sage nodes in the paper (e.g. CPU number of cores and clock speeds; RAM), the Sage website actually reads "Cyberinfrastructure for AI at the Edge" and the associated proceedings, Beckman et al. (2016), which is both cited in and shares authors with the current paper, talks about computer vision (5 times) and OpenCV (2 times), which are quite heavy tools. Therefore, following on the preceding remarks, it seems likely that a more sophisticated algorithm that would at least properly take into account the caveats commonly discussed and addressed in earlier peer-reviewed works on the subject might be both needed to obtain satisfactory results, while being achievable in practice. Has this been attempted, and if not why?

We agree with the reviewer. However, the Sage nodes run many applications using OpenCV and deep learning models, some of them are critical, for example, traffic estimators in the city and wildfire detection in the forests. These applications take the bulk of the processing powers due to the deep learning models. Therefore, it is important to try for low processing for most applications. In the future, we may use more complicated algorithms by adapting an advanced machine learning approach to estimate the cloud motion after accessing their value addition to the final product such as solar irradiance estimators.

> 4. Compared to similar peer-reviewed works, the paper is bit light on the mathematical front regarding the techniques employed. It would be good for the unfamiliar reader if the article was more self-contained.

Thank you for suggesting this. We have now added it in the revised version in sections 2.2, 2.3, 2.4 and 2.7.

> 5. Note: The size in megabytes of Figure 2 in the paper should absolutely be reduced. In the pdf, page 6 alone is indeed responsible for more than 20 MB in the final document file size.

Done. Thank you so much for bringing this to our attention.

> 6. For the references, it would be preferable to also include a peer-reviewed work whenever it makes sense and is possible, and not only proceedings as is currently the case for optical flow.

Thank you for the suggestion. References for Apke et al., 2022; Mondragón et al., 2020; Peng et al., 2016 are added in section 1.

References:

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