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Comment on amt-2022-159

Anonymous Referee #2

Referee comment on "Optimizing cloud motion estimation on the edge with phase correlation and optical flow" by Bhupendra A. Raut et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-159-RC2, 2022

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).

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 (...)". 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.

Review and 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, ...

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

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.

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).

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).
- 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]. 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.".
- 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".
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.

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).

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?

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.

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.

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.