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Atmospheric Measurement Techniques Discussions

Interactive comment on "Uncertainty Quantification for Atmospheric Motion Vectors with Machine Learning" *by* Joaquim V. Teixeira et al.

Anonymous Referee #1

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GENERAL COMMENTS

This paper presents a method of uncertainty characterization for Atmospheric Motion Vectors that is based on machine learning approach. The method has been applied to AMVs extracted from several water vapour layers of simulated data outputs from GEOS-5 Nature Run. The 2 months dataset used in the study has been split in two parts: training dataset (1.5 month) and testing dataset (0.5 month) for the validation. The results effectively show capabilities to filter skilled and unskilled regimes, and to produce some error estimates.

The paper is well written and well organized, presenting the method in section 3 and

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the results and validation in section 4 before the concluding remarks.

I was personally disappointed by this paper that sounded very promising at the beginning. My main criticisms are:

- I have a problem with the present title. Reading it the first time I thought that paper was about improving error/quality of AMVs during the extraction process, and not during the assimilation process. From my understanding a title like: 'Use of Machines Learning to improve Uncertainty Quantification of Atmospheric Motion Vectors assimilated in NWP models', would certainly match better the real content of the paper and be less confusing

- Test presented in this paper is limited to water vapour AMVs extracted on specific layers. This potentially corresponds to extraction of 3D winds from hyperspectral sounders, as mentioned in the introduction. However, there is actually no evidence that the results can be generalised to the common AMVs extracted from clouds tracking in infrared or visible channels. If the method is limited to hyperspectral winds, this must be clearly specified in the text and probably also in the title of the paper, and not let the reader supposed that it works for all types of AMVs. If the method is not limited to hyperspectral AMVs authors have to present results also with common cloud motion winds extracted from satellite imagery. I understand from the text that another paper is upcoming (line 325), but there is no description or information that can actually let me assume that common AMVs have been used, and that the results are positive.

- The algorithm seems to be too dependent on the user's choice of the number of clusters, and the paper does not discuss the dependence of the algorithm on the chosen training dataset. It is also very unclear if the different clusters identified could refer to kind of physical or geographical AMVs properties, or if they are only blindly resulting out of the numerical tests. - Authors must clarify/discuss if the results may depend on the AMV extraction model used (Mueller 2017). It is not clear if the same clusters can be used for operational AMV extracted from other schemes too (NOAA, EUMETSAT,

JMA. . .Etc). If it is not the case I guess this study must be repeated individually for every different AMV extraction schemes and maybe after every releases of these codes, which should represent an important limitation for operational use in NWP models.

Although the authors promise the possibility to distinguish different Âń geophysical regimes Âż, the application ultimately presented by the paper comes down to discriminating the AMVs that are null because they are tracking the ground radiance, which is much too simple to showcase the real benefits of the algorithm.

Therefore I finally decided to reject this paper. However I encourage the authors to submit a new version, including new inputs based on the comments above and additional results applied to real AMV observations.

I also mention specific comments below. SPECIFIC COMMENTS

1) Everywhere I would change the denomination "true wind" to "G5NR wind" throughout the text. No matter the quality of any dataset relating to physical quantities, it does not deserve to be called "true".

2) Line 144 It would be good to recall that this Figure relates to the first 1.5 months of the dataset, in the caption of the Figure.

3) Lines 144-145 This is disappointing. Given the use of a powerful tool like GMM and the possibility of identifying "geophysical regimes" (line 132), I expected far more than just discriminating two groups, one being functional AMVs, and the other merely being the AMVs tracking the ground radiance, when the water vapour layer is too thin.

4) Line 270 This parts misses a "is" between "xi" and "the".

5) Section 4 The term Continuous Ranked Probability Score should be mentioned at least once before the formula at line 278. The two acronyms CPRS and CRPS are used in this section. Please correct.

6) Line 309 You are referring to Figure 13, and not Figure 12 as written.

7) Lines 329-330 I find your conclusion a little daring, knowing that you had to try different numbers of clusters before actually managing to discriminate the null AMVs.

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