Geoscientific
Instrumentation
Methods and
Data Systems

Discussions

Open Access

**GID**

**Interactive comment**

# *Interactive comment on* "An Efficient Algorithm for Improved Doppler Profile Detection of MST Radar Signals" *by* Nimmagadda Padmaja et al.

**Anonymous Referee #1**

Received and published: 18 October 2017

The authors describe their experiments in using Hilbert Huang Transform in analysing data from atmospheric research radar. I consider this topic to be of interest to the intended audience of the journal and an interesting project in itself. The text in the manuscript is mostly of good quality, although some paragraphs require considerable re-phrasing to be clearer.

However, I have three main issues that need to be seriously addressed and a major revision is needed.

1. I feel that the manuscript reads more like something between a technical report and a scientific article. As the authors are referring to existing earlier work, it is unclear what their actual new contribution is. Are there any similar experiments with this type of data, is this new to the Indian radar etc.?

2. In my opinion, there are too many qualitative terms. For example, there is no justification for calling the algorithm "efficient" unless there is an actual comparison such as the time required to produce the results from raw data using the FFT versus HHT methods. Similarly, why haven't the authors used some, e.g. statistical method for assessing the similarity of the Doppler spectra output from using FFT versus HHT? Where is the support for claiming that the proposed method detects the Doppler more accurately? This is not "clearly visible" in the plots.

3. The methodology includes a number of parameters that the user needs to decide before processing the data and these choices affect the results. For all potential users, the understanding of which selections are critical is of immense value. For credibility, a comprehensive quantitative analysis should be carried out. What the authors have shown is that their method seems to work at least for this particular case. But could one do better? Is this the best or worst case scenario? The authors can – and should – use their expertise in their MST radar data to highlight possible problem areas.

I encourage the authors to dive a little deeper in their experiments as their proposed methods appear promising.

——————————

Here are my more detailed remarks:

4. There are a few inconsistencies in the text, so I would recommend going through the manuscript and ensuring all abbreviations are explained when they are introduced for the first time, all units are consistent ("km" not "Km"), EMD algorithm refers to steps (1) and (2) instead of (a) and (b) etc.

5. It would be good to have short paragraph describing what the MST radars are actually used for (and by whom) already in the introduction before discussing the processing techniques. Are the data analysed visually only?

6. In describing the Indian MST radar section, could you insert some real numbers

to provide a general understanding of the normal operations. For example, what does "high resolution" mean, field-of-view etc. Is the Indian radar a typical example of MST radars and what are the major differences? Number of range bins, beams, etc.? Why do you say it is "an excellent system"?

7. Section 3.1, line 58: the sentence "...represented by an IMF that satisfies the conditions of an IMF..." needs to be re-worked.

8. Section 3.1.1 EMD algorithm, lines 68-70: do I understand it correctly that in step (c ), the value of k is used only to keep track on how many times the previous steps were done? And the iterations are stopped when (not "until") the required conditions are met? Please re-phrase.

9. Section 3.1.1 EMD algorithm, line 72: when you talk about the residue $r_n(t)$ becoming less than a predetermined value (see my major point 3), what do you actually mean? The squared sum of the timeseries r(t) or what? I don't also understand what you mean by "monotonic" in this context. Please clarify.

10. Section 3.1.1 EMD algorithm, line 73: "no longer contains any useful frequency information". Please clarify how to quantify this.

11. Section 4 Denoising, lines 89-96: please clarify the whole paragraph, it needs some re-wording.

12. Section 4.2.1 Processing steps (lines 118-136): I think the step (i) and (ii) could be combined into a single step "Read data".

13. Please go through all of the equations, which are not properly typeset. This may be a pdf-conversion issue, but it needs to be checked.

14. Section 6 Results and discussion, lines 158-167: see my major points 2 and 3 and please revise.

15. Section 6, lines 166-177, tables 1-2: You are using four (!) decimal places, such

as 5.6587dB. Do the measurement uncertainties really justify this accuracy? What is a significant difference in SNR given the system noise levels in your radar?

16. Section 6, lines 164-165: I would expect there to be some differences in the "apparent noise" in the data when using either FFT or HHT methods. Did you notice anything, are you able to quantify your observations?

17. Figures 1-10: all plots are much too small to see the details and zooming in the pdf-file does not improve legibility. I would suggest selecting fewer representative plots and possibly highlighting key features (such as "true peaks" based on expert visual analysis) and differences in the outcome. The plots should not be screengrabs. Please revise.

18. References: Please check the journal style guide and go through the references. There were also references to "Donoho et al." and "Donoho and Johnstone" yet there is only a single author "Donoho" in the references. Please check for consistency.

---