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Atmospheric Measurement Techniques Discussions

Interactive comment on "Total variation of atmospheric data: covariance minimization about objective functions to detect conditions of interest" *by* N. Hamilton

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Please see attached annotated PDF for more review comments.

There is some good content and work here, with a generalized method to find conditions of interest for multivariate timeseries; and (perhaps more importantly) inclusion of responsible application of a metric (Mahalonbis distance) to evaluate sensitivity of the method to outliers.

The title is perhaps not quite appropriate; "Total variation of atmospheric data" is rather

vague and somewhat grandiose, not accurately capturing the essence of the work and connoting more results/applicability than demonstrated.

Some significant items of note, as a list:

- In the abstract, *'periods'* of interest is better expressed as '*conditions*', both for the sake of validation and for getting conditional statistics (and towards making fair comparisons of statistics given some conditions).
- Stationarity and conditional statistics underpin this written work; these concepts should be integrated (and referenced, as found in various texts for atmospheric flows), at least starting with the literature review.
- In your literature review, a key method/scheme for event detection (beyond wavelets) appears to be missing: i.e., reference-signal (or ideal signal) approaches based on Hilbert transform, as in Hristov *et al* (1998, PRL **81** no.23), used in various literature (e.g. Kelly, Wyngaard & Sullivan 2009).
- When you mention "direct comparison of statistical quantities", it appears that you are trying to refer to statistics based on *marginal distributions* (or marginal statistics), are you not? In statistical parlance, one contrasts between marginal and conditional statistics.
- The premise "*In lieu of a time series of Richardson number or the Monin-Obukhov stability parameter, turbulence intensity (TI) is used in the current demonstration as a proxy for stability*" is fundamentally problematic. That is, the balance of mechanical (shear) production, buoyant production or destruction, and dissipation ε (defining the 'simple' conditions where Monin-Obukhov similarity applies) results in TI being a proxy for stability only for flows/conditions with the same dissipation rate (Kelly, Larsen, Dimitrov & Natarajan, 2014). So your results per TI are conditional on ε, and do not act as such a proxy unless further constrained (e.g. via U assuming surface-layer similarity for ε.)

Since stability is not really used in the paper, I suggest that you simply keep TI, and change the justification for its use: σ_u and TI are important for driving turbine loads (e.g. Dimitrov, Kelly, Vignaroli & Berg 2018).

- In section 3, where you write "*without explicitly considering the evolution of atmospheric variables*" you should mention stationarity as well. In the atmospheric sciences and boundary-layer meteorology this is typically considered, whereas it is often neglected in wind energy applications.
- Figure 5: missing axis values/scales
- Section 4: can you interpret the total variation in terms of the multivariate components, to avoid obfuscation? Section 4.0 (p.8) is essentially taken from PCA; you should include reference to appropriate PCA text(s) and try to explain $\mathcal{V}$ for the reader. E.g., for readers not as 'fluent' in statistics, if the PC's ($\mathcal{P}$) are orthogonal, then how are the covariances accounted for?

  Is your $\mathcal{V}$ different than the 'overall' or 'total' variability found in literature?

  It could help also to point out the difference between summative variance and $\mathcal{V}$.
- Figure 8: suggestion: use logarithmic scale on y-axis to compare more sensibly
- Fig.9c: which "dimensionless slope" are you using here?
- Fig.11: captions are swapped between (c) and (d).

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2019-200/amt-2019-200-RC2-supplement.pdf

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