Review of: Full scale spectra of 15-year time series of near-surface horizontal wind speed on the north slope of Mount Everest, by Han et al.

General Comments

This paper presents and discusses long-term windspeed spectra based on data collected in a mountain valley 30 km north of Mt Everest – a result that is unique and interesting because of the location, but otherwise not representing little that is new. Moreover, there are some inaccurate statements and in interpretation and presentation, as described below. Thus, significant effort to make this paper suitable for publication should the results be deemed sufficiently novel.

1. The authors use the assumption that the high- and low-frequency portions of the spectrum are uncorrelated (quoting Larsén et al. 2016) to conduct a curve-fitting exercise that reproduces part of the observed spectrum using the characteristic 2-d slope for lower frequences and invoking a formula for Kaimal and Finnigan at higher frequences, presumably weighting it to produce the right result.

   Why is this flawed? First, it isn't obvious that a successful curve fitting is associated with lack of correlation. In a classic paper, Charnock (1957), he showed that a spectral gap had to be sufficiently large for terms containing products of high-and-low frequency terms to average to zero – i.e., for the average of the nonlinear terms to be zero. For the product of two variables – e.g.., vertical velocity and horizontal zonal wind, to be zero, the spectral gap has to be between $f_c/2$ and $3f_c/2$, where is the cutoff frequency for a filter separating the high- and low-frequency motions. Using Charnock's approach for products of three terms, the spectral gap has to be larger, i.e., between $f_c/3$ and $5f_c/3$ (LeMone et al. 1976, p 1315). Thus, the idea of no correlation – i.e., no interaction between the synoptic (2-D) and turbulence (3-D) motions is not necessarily valid even with a gap. In fact, Figure 5 shows that stronger synoptic-scale motions in winter are associated with stronger turbulent motions, -- a correlation, since the stronger turbulence results from the stronger winds driven by synoptic features.

2. Equation (1) doesn't really replicate the actual spectra with the coefficients being constant (except for fitting the regions with -2 and -2/3 slopes – which has to be the case, correlation or no correlation. The need for a varying function is demonstrated through use of the Kaimal equations (2) and (3).

3. The 12-hr peak in the windspeed data is to be expected for any mountain location, and not unique to the QOMS station. One need merely to refer to Chapter 11 of David Whiteman's *Mountain Meteorology*. (references at the end of this review). Likewise, atmospheric tides produce a 12-hr cycle in the surface winds over at least the tropical ocean.

Mountain Locations: There are several examples of mountain-valley, mountain-plains, etc. circulations that go upslope during the day, and downslope during the night. This is a 24-hour cycle in the up-and-downslope component of the wind, but a 12-hour cycle in the wind speed. (This is a classic example of the misleading nature of computing spectra of windspeed instead of dividing the wind into components). Moreover, the authors seem to emphasize only the daytime wind with the 12-hour peak (Line 230). Actually, if there were only a wind peak during the day with no wind change at night, that should show up mostly as a 24-h peak rather than a 12-hr peak. (I see that the observations in Sun et al. (2018) show a nighttime downslope wind peak as well as the daytime peak).

Atmospheric Tides. The authors mention (L23 in the abstract and elsewhere) that a 12-hour peak is rarely observed over the ocean. This is untrue. In fair weather over the tropical oceans, the sea-surface temperature doesn't change that much, muting the normal 24-h cycle. Thus, in fair weather, atmospheric tides can be revealed in the surface wind. I have four references, the earlier ones indicating that semidiurnal surface-wind variation was known for over 50 years. The first two are based on summer data from multiple ships in the 1974 GARP Atlantic Tropical Experiment, GATE, which took place over the tropical Atlantic – unfortunately in hard-to-access papers. The third is from the classic book on atmospheric tides by Chapman and Lindzen. The 4th, a more recent and accessible publication, provides a lot of useful information. While the data differ, all show atmospheric tides show up in the surface wind over the ocean in the same amplitude range.

Jacobs (1980, figure 3) used "all GATE data" (i.e., all ship data) to produce a composite diurnal plot showing semidiurnal wind *speed* peaks at an average maximum of 4.4 m/s and average minimum of 0.5*(4.2 + 3.9) = 4.05, yielding a range of 0.35 m/s and amplitude of 0.175 m/s.

LeMone (1980, Figure 17)) showed diurnal-average curves, based on seven ships (6 from booms, 1 buoy). The10-m zonal wind has a semidiurnal pattern (2 maxima averaging 2.24 m/s,2 minima averaging1.54 m/s) with a range of 0.7 m/s and an amplitude of 0.35 m/s for 30 August to 20 September. The meridional wind has no semidiurnal cycle.

Chapman and Lindzen (1970, table 2S.9 shows the influence of atmospheric tides on surface winds (amplitude of () = (0.08,0.21) m/s) using data from an island site (Terceira, Azores) and combines data for the whole year.

Ueyama and Deser (2008) annual average amplitude, TAO buoys, (u, v) = (0.14,0.06) m/s (Fig. 4); June-Nov amplitude: (u, v) = (0.15,0.07) (Fig. 7) (estimates rough). The Paper also has useful references. This paper also shows a diurnal cycle, comparably small.

4. The introduction is sloppy, mixing references of observations from fixed points (like the observations here, and those from aircraft, and mixing spectra of wind speed and spectra of the horizontal wind components, and not necessarily covering the range of frequencies of interest.

   For example, the Sun and Lenschow paper cited uses aircraft data, which weren't necessarily sampled parallel to the wind. Second, they compute spectra of the wind components rather than speed. Third, their frequency range is severely limited by the length of the flight tracks and thus does not sample the synoptic (2-D) scale. Rather, their lower frequencies, in TOGA COARE at least, are likely associated with precipitating convection. Thus, their results have little to do with the theme of this paper. Focusing only on the papers dealing with the gap between synoptic and turbulent motions would greatly shorten and improve the introduction.

   Since the current introduction deals with spectral gaps that involve only boundary-layer motions, it should be noted that the presence or absence of a gap is a function of the sampling strategy.

   That is, if you are sampling from a point (or fixed tower), you will likely get a two-peak spectrum in the convective boundary layer in the presence of horizontal convective rolls (which are nearly parallel to the wind and take of the order of 30 min to an hour to pass a point on the surface) that coexist with large 3-d convective eddies (which pass a point in more like 5-10 min). This separation in periods is useful for separating out the two structures for analysis, even though the structures of roughly the same scale in the crosswind direction. With only along-wind data, one might think that the spectral gap is big enough for zero nonlinear terms under Charnock's criterion.

   However, if you sample using an aircraft flying normal to the roll axis, you get a single spectral peak, corresponding to the roll/convective cell combination. As can be seen in clear-air radar echoes in the presence of rolls, the convective cells (large eddies) lie in the rolls' upwelling portion – i.e., they are tightly correlated. So, even if you get the spectral gap identified by Charnock as suggesting zero nonlinear terms with the along-wind data, the crosswind data tell another story.

   Thus, unlike what is stated on line 72, a spectral gap does NOT mean that motions are weakly correlated. In fact, they are strongly correlated and interact in this case. Clearly, the presence of the convective cells in the upwelling portion of the rolls indicates a correlation. A paper you cite, LeMone (1976) discusses the interaction of the rolls and cells.

5. Moreover, I don't think that a list of spectral peaks at different geographic locations is illuminating unless they are carefully grouped according to terrain and sampling strategy (length, seasons, fixed vs traveling platform and whether/how its track is related to wind direction).

Specific comments:

L65. Lenschow and Sun applies to aircraft data, not data collected along the wind. Also, they show spectra of the wind components, which is different from what is done here. Should check these studies to see if they correspond to what is done in this paper.

In the introduction, the discussion is longer than it needs to be. What is of interest here is the gap between synoptic (essentially 2-D) and turbulence (3-D) motions, rather than a gap between structures in the boundary layer.

L82-84. Height dependence depends on the variable observed. With rolls as an example, the spectral gap in vertical velocity would become MORE evident with height. Of course, the horizontal winds vary more near the surface. (don't understand why you need a reference for this – it's common sense).

L90. Studying a map of Beijing, a 12-hour peak in wind speed seems likely, associated with the 24-hour upslope-downslope wind cycle with nearby higher terrain.

Figure 1. A terrain map would be more helpful. It appears that the site isn't on a slope but is in a valley. Details are important – see David Whiteman's *Mountain Meteorology* (reference below). Also the valley location would make the comparison with the wind rose more meaningful.

L118. 30 km north of the peak of Mt. Everest in a valley is a significantly better description than "on the north slope of Mt. Everest."

L160-165. A reasonable summary of the wind roses would start with a statement about how much the winds are along the valley axis – I was surprised that this comparison isn't made until farther down in the paper and that it was not mentioned in the conclusions.

L193. Delete "changing."

L195-6. Are the increasing near-surface wind speeds over flatter terrain?

L225. This is hardly surprising and well-known that the diurnal cycle is stronger over land. Are references even needed? (i.e., this is discussed in most introductory meteorology books).

L230. Don't Sun et al. see the classic mountain-valley wind, with the daytime wind influenced by the westerly jet and a second night-time maximum associated with downslope wind? You have to consider the night-time wind as well. If there was only a maximum in the 12-hour period during the day influenced by the subtropical westerly jet, that would be a 24-hour cycle. (see general comments)

L236-8. Not necessarily. Perhaps they would be close because the average winds were close.

L253. As expected, the mountain-valley system is stronger in the summer, and it is not surprising that a 12-h peak is stronger than the 24-h peak in wind SPEED. For the wind component along the slope, the period would be 24 h. This is an example of the misleading aspect of using speed instead of wind components.

L270. Regarding Kang and Won, what was the length of the dataset used?

A cautionary note: The relationship between scale in m and frequency in Hz is often a function of the windspeed (if features are carried by the wind). For winter, and stronger winds, the same scale of eddy (thinking about km-scale) will produce a higher frequency. For winter, that eddy might be smaller since the boundary layer could be smaller than in the summer.

Eq (1) doesn't make much sense unless the coefficients are functions. Without functions, how can it match any observations "perfectly" (L312).

Figure 8. and Eq (1). The line labeled "Eq. (1)" is only the low-frequency part, along with Kaimal and Finnigan's spectral function at the higher frequencies. This does not show that Eq. (1) works if the coefficients are constants. And it is curious that you don't extend Kaimal's spectral power to the higher frequencies.

L328. It does appear that you can linearly composite the low- and high-frequency data, but this does not imply that the two are independent of one another.

Figures 5-8. The rising spectral density at the high frequency end reflects the presence of white noise and should be eliminated or at least pointed out.

Figure 8. Did you evaluate periods for stable ($L>0$) and unstable ($L<0$) separately in Eq. (2)-(3) and then combine? There should be a more complete explanation about how the calculation is done. Also, more precise to say 'added' rather than 'merged."

6. Summary and conclusions:

L342. Again, misleading to write "on the north slope of Mt. Everest," since the location of the station in the valley has strong control over the wind direction.

L346. Along the valley! (and not north-south)

L353-4. Not a result of this paper.

L354-5. More accurate to say that the 12-h peak is common in spectra of wind SPEED. This is an important distinction. “Some urban sites” -- are these urban sites close to/in mountains, like Hong Kong or Beijing? Again, not a result of this paper.

L356. The 12-h period is not unique; nor is it related to the single peak during the day, as explained earlier.

L363. That’s only 16.7 minutes, well within convective boundary layer frequencies! If you look at the curves carefully, the minima tend to be at lower frequencies, which makes more sense. You have a better estimate in the body of the paper. (L298)

L366-367. For the many reasons (and based on the results) the synoptic and turbulence spectra ARE correlated.

REFERENCES

Chapman, S., and R. S. Lindzen, 1969: Atmospheric Tides. D. Reidel Publishing Company, the Netherlands, 200 pp.

Jacobs, C.A., 1980: Mean diurnal and shorter period variations in the air-sea fluxes and related parameters during GATE, In Siedler, Gerold, and John D. Woods, 1980: Oceanography and Surface Layer Meteorology of the B/C-Scale, GATE, V1, Pergamon press, 294 pp. Supplement 1 to Deep-Sea Research Part A, v. 26, pp 65-98. https://doi.org/10.1016/B978-1-4832-8366-1.50009-1.

LeMone, M. A., 1980: The marine boundary layer, pp. 183-231, in *Workshop on the Planetary Boundary Layer*. J.C. Wyngaard, ed. American Meteorological Society, 322 pp.

Ueyama, R. and Deser, C., 2008. A climatology of diurnal and semidiurnal surface wind variations over the tropical Pacific Ocean based on the tropical atmosphere ocean moored buoy array. *Journal of climate*, *21*(4), pp.593-607.

Whiteman, C. D., 2000: *Mountain Meteorology: Fundamentals and Applications*. Oxford University Press.355 pp.