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Title: Novel approaches to estimating turbulent kinetic energy dissipation rate from low and moderate resolution velocity fluctuation time series

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General comments:

The authors propose and evaluate two techniques to retrieve estimates of the turbulent kinetic energy dissipation rate ϵ on the basis of the frequency of zero-crossings of time series of turbulent velocity fluctuations measured with sensors that do not have a frequency response that is necessary to resolve the smallest turbulent scales, which are on the order of the Kolmogorov length, η.

Section 2 ("State of the art") is a collection of equations that relate various turbulence characteristics with each other. These equations have been taken from a large number of different sources, and it is not clear what the underlying physical assumptions are and to what extent they are consistent across the various sources. For example, it is not explained whether the one-dimensional spectra $E_{11}(k_1)$ and $E_{22}(k_1)$ and the frequency spectrum $S(f)$ are meant to be one-sided spectra (such that the spectrum integrates to the variance when integrated from 0 to ∞) or two-sided spectra (such that the spectrum integrates to the variance when integrated from 0 to ∞). Moreover, it is not mentioned which of the relationships follow from Kolmogorov's theory of fully developed, locally homogeneous and isotropic turbulence Kolmogorov (1941a,b) and which are valid for any statistically homogeneous vector fields, regardless of whether or not they are isotropic (Monin and Yaglom, 1975, pp. 16-22).

Additionally, I find it worrisome that the authors sometimes confuse k_1 and k and that some of their equations contain transcendental functions with dimensional arguments; see details in the specific comments below.

While the paper is in general well written, I find it difficult to follow the flow of the authors' reasoning in detail. I am not surprised that the zero-crossing methods can provide ϵ estimates with a quality comparable to the ϵ estimates obtained with traditional spectral retrieval methods. The relative advantages and disadvantages, however, are less clear, and the authors do not discuss and explain them in sufficient depth from a physical point of view.

Recommendation:

The paper may be acceptable for publication after major revision.

Specific comments:

1. Page 2, line 23: "State of the art" — This section heading is unnecessarily vague and misleading. I would suggest to replace it with "Previous methods to retrieve the energy dissipation rate from measured velocity time series" or something similar.

2. P. 3, lines 5ff: "The energy-spectrum function in the whole wavenumber range can be approximated by the formula (Pope, 2000) . . ." — It is not correct that Eq. (2) is an acceptable approximation of the energy spectrum for the *whole* wavenumber range. In particular, Eq. (2) fails at wave numbers small compared to $1/L$, where the turbulence is usually anisotropic and is no longer universal.

3. P. 3, lines 12ff.: "Within the validity of the Taylor's hypothesis (1) can be converted to the frequency spectra, where $k = (2\pi f)/U$ and U is the mean velocity of the aircraft." — This statement is erroneous or misleading in two respects. First, Taylor's frozen-turbulence hypothesis converts the frequency f to the longitudinal wave number k_1, not to the magnitude $k = \sqrt{k_1^2 + k_2^2 + k_3^2}$ of the three-dimensional wave vector $\mathbf{k}$. Second, U is not the "mean velocity of the aircraft" but the magnitude of the vector difference between the aircraft velocity and the wind velocity. This magnitude is sometimes referred to as the "true air speed".

4. P. 7, lines 12-14: The integrals in Eqs. 25 and 26 contain the term e^{-k}. The exponential function, however, is a transcendental function and its argument must be dimensionless, such as the argument $\beta k \eta$ in Eq. 24. Because k is not dimensionless (its dimension is 1/Length), Eqs. 25 and 26 cannot be correct.

Bibliography

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