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Interactive comment

Interactive comment on "Novel approaches to estimating turbulent kinetic energy dissipation rate from low and moderate resolution velocity fluctuation time series" *by* Marta Wacławczyk et al.

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I was conflicted in how to approach my review: on one hand, there are no glaring technical problems in the authors approach, but on the other hand, they clearly do not make the case for any practical use of their method. Specifically, their approach is highly dependent on the form of the power spectrum in the dissipation range, and yet in their application there are no data points in that range (i.e., all the points are in the inertial subrange). So, why would one want to use their method - with no actual data in the dissipation range, and a potentially suspect model in that range - over a more-standard method based on a tried-and-true inertial subrange model - and where most of their data lie. They do not perform a sensitivity study on the choice of dissipation range model. They use a specific exponential model from Pope (2000), but if they

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Discussion paper

had read the discussion in that reference, they would have noted that Pope does not consider that model to be accurate. And as the authors point out, the dissipation range spectrum has a significant effect on the number of zero crossings. Furthermore, they do not address practical issues inherent in digital signal processing: spectral bias due to finite temporal windows, aliasing due to temporal sampling, as well as sensor bias and noise. It seems that these artifacts might be have a significant impact on a zero-crossing method. For example, it is not hard to see how sensor bias and noise, could significantly impact zero crossings, especially for low SNR data.

So, they need to address the question of why one would want to use their method over more standard approaches (unless of course, one had data with significant content in the dissipation range), and how their method is susceptible/tolerant to signal processing artifacts. I feel strongly that they need to perform a simulation analysis to answer these questions in a statistical sense (see for example, Frehlich, et al. JAM 2001); real data is not acceptable, except for case studies. As the paper stands, I would require significant revisions that address these issues, before accepting for publication.

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