Review about

Estimating the turbulent kinetic energy dissipation rate from one-dimensional velocity measurements in time

by: Marcel Schröder, Tobias Bätge, Eberhard Bodenschatz, Michael Wilczek, and Gholamhossein Bagheri

This paper addresses the fundamental problem of estimating "local" energy losses from one-dimensional velocity measurements as a function of time, with a focus on atmospheric conditions. Such observations can be made, for example, with single-component hot-wire anemometers and are critical to a better understanding of turbulent processes in the atmosphere. Since there are different methods and approaches to determine energy dissipation rates, there is also no doubt about the need to discuss in detail the methods and the sources of error associated with the measurement which has been done in great detail in this work.

Furthermore, there is also no doubt that an in-depth treatment of this topic is still missing in the literature and thus this work makes a very valuable contribution to atmospheric turbulence measurements and their accuracy. I can only congratulate the authors on this contribution and really have only one but important comment, which relates to its potential usefulness to a wider readership and should be easy to incorporate. I must also emphasize in this context that my background is rather experimental and I have not followed all derivations in detail. Therefore, as I said before, my main criticism also lies somewhat in the potential applicability of the material for experimenters. The discussions are for the most part very theoretical which is certainly absolutely okay, but from my perspective the applicability of the results suffers as a result. I think, especially in the summary, more clear recommendations for action could be formulated as to what is the most robust method for determining dissipation rates under given circumstances. My fear is that in future publications on experimental determinations of dissipation, this manuscript will be cited but rather generally and without direct application and that would be a pity because this manuscript definitely has the potential to serve as a reference for future measurements of dissipation.

After consideration of my comments, I unreservedly recommend the paper for publication. When I ask here for "major revisions", this does not refer to the scientific content but to the request to revise the paper for better applicability and partly also readability.

<u>Major comment (partly overlap with the introduction):</u>

The derivations and subsequent discussions of the various error contributions in the determination of the dissipation rate are described in great detail and quite formally - this is also absolutely fine and understandable, although the abundance of details reminds me partly more of a dissertation than a manuscript for AMT; however, you have chosen a meteorologically oriented, metrological journal with a corresponding audience. For this reason, I wonder how you can possibly get even more use for atmospheric measurements from this really great material. You already shifted a lot of details into the appendix which is highly appreciated but I feel that at several places there is still too much detail not necessary for estimating the uncertainty of dissipation. One prominent example in this context is the description of the active grid of your wind tunnel (in particular Eq. 12), this is very technical and does not contribute to the main topic at all - a reference would be more than sufficient. The same with some tables such as Tab 2 for example - do I really need all the details of the DNS?

I think too much detail can easily distract from the main topic and slow down the reading flow, and I suggest going through the manuscript carefully and - where possible - shortening it some more (although I know from own experience how hard and difficult shortening can be…).

A second way to make the material more valuable to a wider audience would be to include some sort of recommendation in the summary section. Such a brief discussion would be very helpful for application-oriented colleagues.

<u>Specific/minor comments:</u>

Abstract: I think it is not common to define abbreviations in the abstract if they are not used again in the abstract itself - I suggest avoiding it.

<u>Introduction:</u>

The introduction is general nicely and clearly written with only a few places where I suggest some more details and information:

Line 29ff: Maybe a short explanation what exactly is meant by "instantaneous energy dissipation field" is helpful at this place; what are the "high spatial/temporal scales" mentioned in line 29? Please specify!

line 32: if the averaging is over the instantaneous energy dissipation rate, why is the index "0" missing here?

line 36ff: can you please provide an example why the locally averaged dissipation is of importance (although I completely agree with this statement)?

line 49ff: this is probably true, but at least airborne turbulence observations with a sufficient high true airspeed yielding a low turbulence intensity where applying Taylor's hypothesis should be fine - right? So your comment is more related to ground-based observations or tethered systems (balloons, kites) combined with high turbulence intensities – should be mentioned.

<u>Chapter 2</u>

I don't understand why the strain rate tensor in Eq 3 is in capitols but in tab 1 not – please specify.

Title of Sec 2.2.3: not sure about how the subtitle compares to the subtitle of subsection 2.2.2. The spectral method is also an "indirect method" right? So, I think the subtitles should be similar and only differ for "spectral" and "structure function"?

Eq 11: maybe I missed it, but the autocorrelation function *f(r)* has not been defined/introduced yet – right?

Fig 1A and C: Symbols are partly poorly resolved and pixelated and therefore difficult to read.

line 261: please provide a reference for the given number range for R_λ under atmospheric conditions

line 262: maybe for the broader audience one or two sentences should be included about the basic motivation using SF6 at this high pressure and why not working at atmospheric conditions. I think the most of the readers do not know about the advantage of SF6 and what type of gas it is.

I'm not sure Eq 12 is necessary to understand what the experiment is about; the equation formalizes the text unnecessarily and a description in the text is more helpful and sufficient

Line 270: Why using three setups with quite comparable Taylor-Reynolds numbers?

line 272: what does NSTAP stands for?

Line 285/286: I am still not quite sure if I have correctly understood the difference between the "true mean" and the "ground-truth". Could you please explain these two terms (or the difference) again?!

Line 339ff: I think the sentences here are all correct in terms of content and technique, but it is very hard and complicated to read these sentences without losing the flow. You put information that could also be presented in a table (although you have already a lot of tables) into one sentence and you almost have to "study" these sentences to get the content completely. I fear that many readers will not be ready for that. This is only a prominent example and you should perhaps think about it at some other places whether one can represent the content for the reader not also somewhat more simply without neglecting thereby information / precision.

Line 343: Is the phrase “second-order dissipative statistics“ correct? I wonder about the word “dissipative” in this context because the statistics cannot be “dissipative” or did I misunderstand? Please comment on this, maybe I am wrong here.

Line 412: \citep => \cite (such as in line 410)

About Eq 17: It is not clear to me what the variable "y" in the equation means exactly and how I can apply it. I think a few more explanations would be helpful at this point.

Line 440ff: Such statements (or recommendations) are very useful for and should be placed or repeated at a more prominent place in the manuscript (=> summary)

Eq 23: I cannot technically follow this equation with its different underbraces, please at least double check!

About the summary:

I have no concerns about writing a summary in bullet points. However, I think that a pure summary in its present form could be improved with little effort by also summarizing again here the recommendations as formulated in the discussion. This would probably be a nice conclusion to the work and the reader would take away even more hints for own applications.

I am not sure if the following suggestion will work, but one could also think about discussing at the end of the manuscript (maybe in the discussion) using a measurement example from the atmosphere to exemplify at least some methods and estimate errors. This is just an idea/suggestion and does not have to be implemented at all but it would make the paper much more interesting for more experimentally oriented readers.

Literature:

Line 783: please check the author's list

Line 823: not sure about "grew" literature – will probably checked by the publisher; same with other pre-prints such as in line 839

A few more papers which might be of interest in this context and also might be considered:

Rod Frehlich's work about hot-wire calibration is somehow related to your work and definitvely should be included at a prominent place:
https://doi.org/10.1175/15200469(2003)060<2487:TMWTCT>2.0.CO;2

Andreas Muschinski, R. G. Frehlich, M. L. Jensen, R. Hugo, A. M. Hoff, F. Eaton, and B. B. Balsley. Fine-scale measurements of turbulence in the lower troposphere: An intercomparison between a kite- and balloon- borne and a helicopter-borne measurement system. Boundary-Layer Meteorol., 98:219–250, 2001.

See Fig 7 in H. Siebert, S. Gerashchenko, K. Lehmann, A. Gylfason, L. R. Collins, R. A. Shaw, and Z. Warhaft. Towards understanding the role of turbulence on droplets in clouds: In situ and laboratory measurements, and numerical modeling. Atmos. Res., 97(4):426–437, 10.1016/j.atmosres.2010.05.007 2010.

The last reference includes at least a rough intercomparison of direct estimates of epsilon and inertial subrange scaling methods although it is by far not as detailed as your work.