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Comment on amt-2021-161

Anonymous Referee #4

Referee comment on "Characteristics of the derived energy dissipation rate using the 1 Hz commercial aircraft quick access recorder (QAR) data" by Soo-Hyun Kim et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-161-RC3, 2021

This article investigates the applicability of Quick Access Recorder (QAR) in-situ aircraft data obtained at a 1 Hz sampling rate as a cost-effective alternative to derive energy dissipation rates (EDR). The authors compare 5 methods to estimate EDR using QAR data collected during 2012 from Korean-based air carriers (B737 and B777 aircraft). The study is overall well organized and written. Plus, the topic itself it is certainly of great value to the aviation industry and the turbulence research community at large. However, there are a number of major aspects that need to be addressed upon publication at the journal of Atmospheric Measurement Techniques. Given the magnitude of these issues, it may be more appropriate for the authors to consider resubmission in the future. Here below I provide both general and specific comments with the hope that these can help the authors get to a publication stage.

General comments

- ***Most of manuscript is devoted to the description of the different methods used to derive EDR***. While this is of value, none of these approaches are firstly introduced herein, so there needs to be significantly more weighing in the analysis and quantification of the usability of QAR (see other comments below).
- ***A main drawback of the current study is the lack of higher-rate data to use as reference in the estimation of EDR***. Even if the 5 methods produce similar EDRs, all estimates can be biased. It is impossible to use such comparison in the lack of a higher-accuracy reference (e.g., 10 Hz data) to determine the adequacy of 1 Hz QAR-derived EDR estimates. Otherwise, one can only comment on the differences between the 5 methods, which has already been reported for most of them in the literature, and that is not the aim of the current manuscript [Page 3, lines 21 – 22: "The main purpose of this study is to examine the feasibility of various objective EDR estimations using the 1-Hz (coarser frequency) flight data for possible sources of atmospheric turbulence in cruising altitudes."] If simultaneous retrievals of 10 Hz and QAR are not available, I would suggest to use 10 Hz data decimated to resemble the 1 Hz QAR. This will lead to

a fair assessment of the QAR data and that will help understand any systematic biases or errors at a particular range of EDR.

- ***The use of a climatological log-normal distribution as reference may not be adequate in this case neither is supported by the presented results***. There are reasons why the PDFs of EDR do not behave as a log-normal distribution. These can be related to the limited data availability (seasonal effects) and/or geographical considerations. To me, a higher probability of null-light turbulence that decreases as EDR increases makes good sense, as turbulence encounters are a rare event in a climatological sense. Therefore, the assumption that the QAR-derived PDF of EDR have to be log-normal is not necessarily a good way to assess the performance of this lower-frequency data.
  - Page 10, lines 5 – 6. This is questionable. In the majority of cases there is not a reasonable fit for the EDR values at the peak and below, so I do not think these types of distributions are log-normal. Also, the higher probability bins should have a larger impact on the fit, so the fact that some of these needed to be excluded appears to be the result of a clear departure from a log-normal distribution. Maybe there is some issue with the particular optimization method selected (Levenberg-Marquardt) or its implementation by the authors. Perhaps trying another least-square minimization algorithm will help understanding where the actual problem comes from. Some level of discrepancy among the PDF and the best is to be expected, specially toward the tails of the distribution, but not for the area of larger probability if the selected reference is a good descriptor of the observed quantity.
  - Page 10, lines 11 – 14. This claim is not supported by the data. I agree that the interest is more on larger EDR values, but the presented results do not justify the claim of a 'reasonable' log-normal distribution. This seems to be a key point of the manuscript to prove the validity of the QAR-derived EDR estimates, which makes me wonder about the value of these study lacking a reference (ideally 10Hz data) to compare to.
  - Page 10, line 18. Having similar statistics (mean and std) does not guarantee any skill in any of them (see general comment #1).
- ***The 'Results: Case analysis' section is highly subjective and does not allow to quantitatively assess the QAR-based EDR estimates***. These are interesting cases, including the discussion of the associated potential large-scale mechanisms leading to turbulence. However, without a reference EDR to compare to, they do not add much to the verification and establishment of QAR data as a reliable source of data to derive EDR, neither to the actual strengths/downsides of each of the 5 exercised approaches to estimate EDR.
- ***There are some details regarding the application of some of the techniques used to derive EDR that need attention, including the use of B737 data at 0.25Hz***. See the specific comments down below.

Specific comments

- Page 1, Lines 18 – 20. This statement is not supported by the results (see general comment #3).
- Page 1, lines 20 – 22. Or maybe inappropriate range of the spectrum being captured. This is just a speculation that needs further support.
- Page 2, line 24. What does the term 'off-line' refers to here? Post-flight? Please explain.
- Page 3, lines 13 – 14. What are the advantages/disadvantages of using QAR versus EHS and ADS-B? It would be good to put QAR in the context of data quality, availability and access compared to these other 1Hz sources, as well as elaborate on the

fundamental principles and assumptions used to derive EDR in each case. Maybe this discussion is more appropriate in the methods section.

- Page 3, lines 26 – 27. I would suggest removing this sentence from here since it is mentioned in the methods section.
- Page 3, line 30: “MOG-level turbulent cases”. Based on what? It should be an independent method... PIREPs?
- Page 6, line 22. This may be a bit too large of a scale for the aircraft to feel since wing span is less than 100 m (~50 m).
- Page 6, line 28. Should mention that the range is 4-5s for the B737.
- Page 7, line 1. How about the 0.25Hz?
- Page 7, line 10: “no window”. This seems contradictory. The next sentence indicates that a window has been applied. Do the authors mean 'no windowing'? If so, a 2-min FFT can be rather noisy (as clearly evident from Fig. 4), tending to over-estimate EDR (e.g., Munoz-Esparza et al. MWR2018 in the references). The same would apply to method 3 (von Karman spectrum). Please comment on this.
- Page 7, lines 16 – 17. The von Karman model is meant to capture larger scales, so it seems like the authors would need to consider a wider frequency range than 0.2 – 0.5 Hz.
- Figure 4. The labels on Fig. 4 are confusing. The two models can be -5/3 since the lines are not parallel in the plot. Also, it would be good to add what the actual EDR values are in the figure for the readers benefit. Also, the B737 will have a Nyquist frequency of 0.125 Hz, which is out of the time interval for the -5/3 fit that the authors propose (2 – 5 s).
- Page 8, lines 4 – 5. Not sure this is reasonable to state without further proof.
- Page 9, lines 9 – 12. These equations are redundant, as they are the exact same as in Eq. 11. Please remove.
- Figure 5: I recommend to plot the x-axis (DEVG) in log scale, since that will help evaluate the nature of the fits and PDFs better. Also, it seems only half of the distribution is plotted. The point of the log-normal distribution is that there are instances to the left of the mean of the distribution (lower values), which are symmetric to the larger values (see Sharman and Pearson 2017, their Fig. . However, this shows the peak occurs at the lowest DEVG values. It seems like there may be better distributions to use than the log-normal for DEVG, which may potentially lead to a better fit.
- Page 9, line 20: “wide coverage”. I assume you mean over time, but it would we good to specify.
- Figures 6 & 7. Here the x-axis needs to be in log-scale. Many of the distributions do not look log-normal, but maybe the x-log will help. Also, please use the same range of log(EDR) for all panels. Otherwise, it is very difficult to compared them.
- Page 15, lines 12 – 31. This appears to be out of the scope and context of the current manuscript, since forecasting is not the topic of this work.