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Interactive comment on "Characterization of a new perturbation system for gust generation: The Chopper" *by* Ingrid Neunaber and Caroline Braud

Anonymous Referee #3

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This work describes the construction and output of a, to my knowledge, novel turbulence generator in a wind tunnel. This device is called "the chopper" and is essentially a spinning blade that cuts through, or "chops", the wind tunnel inlet. The blade can be spun at different frequencies, resulting in different intermittently turbulent flows in the wind tunnel test-section. One-component hot-wire measurements were performed in order to assess the chopper's output. It is demonstrated that the chopper imparts sudden changes on the centerline mean velocity, which is particularly useful as most other turbulence generating techniques cannot create a flow which responds so rapidly. Homogeneity and spectra are also investigated, demonstrating that the flow becomes more homogeneous as it develops downstream and that the rotational rate of the chopper blade and its harmonics are present at the low frequencies in the spectra, with a turbulent looking spectrum at higher frequencies. Both of these observations are perhaps obvious, but nonetheless are shown for the first time here for this novel setup. One feature of the paper I particularly liked was that it often made me ask a question in my head, and I found that the answer was present later in the paper.

I think after some review, this work will be worthy of publication as it is essentially a proof-of-concept of a form of lab-scale turbulence generation that would be quite useful for the readership of this journal. I have divided my feedback below into comments on the work as a whole and small typographical or wording corrections.

COMMENTS:

1) The introduction is a little bit scant, particularly with respect to the literature review. While there is no minimum number of referenced works required for a paper, at 13 references (of which only 5 are full/non-conference journal papers), this is one of the works with the fewest references I have read in some time. As briefly pointed out in the introduction, active grids are likely the main competitor to this methodology but are hardly addressed in the text. Perhaps this is an area where more scope can be provided in terms of showing previous work or highlighting limitations of other methodologies; for instance, active grids struggle to create sudden changes such as those that can be made here. Numerous active grid papers address generating atmospheric/wind turbine conditions, e.g., Cekli & van de Water (Exp Fluids 2010), Knebel et al. (Exp Fluids 2011), Hearst & Ganapathisubramani (Wind Energ 2017), and active grids have been used in direct conjunction with model turbines in several studies as well, e.g., Cal et al. (J Renew & Sustain Energ 2010), Rockel et al. (Renew Energ 2017), Vinod & Banerjee (App Energ 2019), Li et al. (Renew Energ 2020). Moreover, limitations of using static grids can be made explicit, e.g., Devinant et al. (J Wind Eng Ind Aero 2002), Bartl et al. (Wind Energ Sci 2018). While I am not promoting that the authors must add these particular references, drawn from the top of my head, these or similar would certainly help provide context for the reader and multiple opportunities for comparison and contextualisation in the body of the text. The latter are somewhat lacking. Please

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do not just add these specific references to a list that is already in the paper but instead use them as a starting point to expand the introduction slightly.

2) The authors use the term “gust” to describe the event created by the passing of the chopper blade. However, from the results, e.g., Fig 3, it appears these events are net losses in momentum, although there is a short increase at the start. To me this is a “lull”, i.e., decrease in the wind speed, rather than a “gust”, which is often associated with an increase. I thus suggest the authors either modify their terminology or include a definition for gust that includes the net momentum loss events shown here as “gusts”.

3) Is 20 cycles sufficient for convergence of the results? Can some evidence be provided?

4) The stated frequency response of 38 kHz for a standard hot-wire seems quite high. Are the authors sure about this? See for instance Hutchins et al. (Exp Fluids 2015) where they suggest the true frequency response does not typically exceed $\sim$7 kHz.

5) In Figure 3a the red line has some non-zero thickness outside of the primary blade passing events. What does this signify?

6) I think some comments about the spectra are lacking a little bit of rigour. For instance:

a. Using the word “flat” to describe the low-frequency region is not exactly accurate. The discussion of the blade pass frequency and its harmonics is good though.

b. I am not sure there is in fact anything overly insightful about the spectral breakdown into the various components. This is well known, for instance, in the amplitude modulation community, c.f., the review by Dogan et al. (Fluid Dyn Res 2019).

c. There rigorously is no “fˆ-5/3” rule/law. This comes from Kolmogorov’s analysis of the second-order structure function in isotropic turbulence. From dimensional analysis, in wavenumber space this results in kˆ-5/3, which is typically converted to a frequency spectrum by invoking Taylor’s frozen flow hypothesis (e.g., Laizet et al. (Phys Fluids

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2015)). So, while looking for fˆ-5/3 isn't wrong, it is a few steps removed from the theory. I only bring this up because no reference is made to where fˆ-5/3 comes from. Simply adding a sentence pointing in the direction of the source would be more rigorous.

7) With respect to the turbulence intensity used, this appears to be defined relative to a global incoming velocity rather than the local mean. What do these figures look like when normalized by the local mean? I am curious to see them in a reply to this comment; the authors do not necessarily need to include them in a revision at this point, but I will reserve final judgement on this.

8) I am not sure the breakdown into "inner" and "outer" scales (ln. 206) is used rigorously here either. These words mean specific things in most turbulent flows; for instance, for turbulence in general the inner variables scale the dissipation range (e.g., Kolmogorov variables) and the outer variables scale the energy containing range (e.g., the integral scale and the TKE). In wall-bounded flows, viscous based units (e.g., the viscosity and the friction velocity) are the inner units, while the boundary layer thickness, pipe radius, or channel height and the mean centerline/bulk/freestream velocity are the outer units. So, while I do not disagree with the authors' breakdown in principle, I am not sure the terminology used is the best, as it has specific meaning in other flows where the integral scale is an outer unit not an inner one. Moreover, the inner units of a turbulent flow are also still inner units here.

9) The discussion section actually acts mostly as a summary, and, as indicated above, I am not sure the discussion of the spectra in this section is in fact novel. If the spectral discussion is removed here, then this section can be merged with the conclusions.

10) This facility can produce many different flows, but only two are shown. Why is that? I would be interested in seeing more. I don't know if more need to be included in the present study (it would be stronger if there were more), but I would like to know why the authors stopped here.

MINOR CORRECTIONS:

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1) (ln. 26) “extend” should be “extent”.

2) (paragraph starting on ln. 30) The Wester et al. (2018) experiment is described as using the same configuration as Wei et al. (2019b), but the Wester et al. experiment actually preceded the Wei et al. experiment. Perhaps the words used or the order they are presented in should be changed. 3) (ln. 38) The Makita paper should be referenced by the last name.

4) There are quite a few times in the manuscript (e.g., the sentence starting on line 55) where a preposition, e.g., “in”, is missing after “downstream” or “upstream”. These should be found and corrected.

5) (ln. 99) Use of word “over” here is confusing. That terminology most often is used to mean “divided by” but that isn’t what the authors mean.

6) (ln. 99) Word “of” is missing in front of “the chopper”.

7) (ln. 124 and ln. 152) “the to the flow…” is some sort of typographical error that occurs more than once.

8) (ln. 154) “a low chopper frequency of the chopper”, correct this.

9) (ln. 201) Repeated “into”.

10) In general, the structure and writing style are good, but there are a few typos and missing articles and prepositions that confuse things. Please review the writing in general and perhaps get another set of eyes.

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