Report on paper “The fractal turbulent/non-turbulent interface in the atmosphere”
by L. Neuhaus, M. Wächter, and J. Peinke

The authors present a detailed analysis of the geometrical, possibly fractal, properties of the atmospheric turbulent/non-turbulent interface (TNTI) by analyzing data from wind speed measurements at three different locations (FINO1, Cabauw, Borssele Alpha). I found the paper instructive, well written, and I appreciated the clarity of the graphs despite the large amount of information they provide. Overall, this study provides scientifically sound results and conclusions.

Here are few suggestions that aim at improving the clarity of the presentation and providing some simple, yet important, results about the TNTI. All the points below are given in chronologic order. Points 4 & 5 below is to me the most critical suggestions I would like the authors to address.

1. Section 2, it was not clear to me if the anemometers data issuing from either the FINO1 or the Cabauw were obtained simultaneously at different heights. If yes, that means that 2 dimensional maps of the wind speed as a function of time, *t*, and height, *z*, could have been constructed and used to tackle a 2D analysis of the fractal properties of the TNTI. Am I right? I agree that the resolution in z-direction is probably not sufficient for such an analysis to be carried out but maybe this could be written somewhere in section 2.

2. Section 3.1:
   a. In section 3.1, the authors present the state-of-the-art of TNTIs. When discussing the work by Sreenivasan and Meneveau (1986), the authors could add that Sreenivasan and Meneveau discovered that a fractal scaling can exist in an intermediate range of scales which is comprised between an inner cutoff (a small scale which they found to be of order of the Kolmogorov scale) and an outer cutoff (a large scale which is generally assumed to be proportional to the integral length-scale).
   b. the phrase at line 72 starting with “For a reduction…” could be deleted since this will be more clearly explained at the level of section 3.2, Eq. (2).
   c. I feel that it could be worth recalling that that a surface has a fractal dimension which is bounded, i.e. $2<D_f<3$. A surface with dimension=2 is smooth (e.g. a sphere has a surface which grows with power 2 of its diameter) while a surface with a fractal dimension of 3 is so tortuous that it fills the entire space. For scales below the inner-cutoff (see point (a)), viscosity tends to smooth out the interface and the surface becomes smooth (with dimension 2).

3. Section 3.2, to be more precise, it should be mentioned that the dimension which is measured using the box counting method is the "box dimension" or Minkowski-Bouligand dimension. This may differ with other measures of fractality using say the caliper technique, spectra or correlation functions.

4. Section 3.3, the authors have used a time window of 20s (90s for the Lidar measurements) for computing the moving average velocity and related TI. Could the

authors justify this choice? Are results sensitive to this parameter? Similarly, the authors analyze statistical results for the TNTI based on a 10 minutes window. Could you please justify this choice and provide material and discussions on how results change when this window is increased/decreased?

5. In section 4, I regret that the analysis the authors have performed is not able to answer the straightforward question of the height at which the TNTI is located. For doing this, the authors could have showed the number of crossings per unit time (or unit length given the Taylor hypothesis) as a function of height. This represents the probability of finding the TNTI at a given location. In 3D, this is the surface density. My opinion is that it is the first quantity that should be presented and discussed in section 4. In the context of wind energy production, I think it gives a good idea of the relative position between the atmospheric TNTI and the height of the wind turbine. Similarly, the authors could provide the portion of time the signal is in turbulent state versus laminar state as a function of z. In the fluid mechanics community, this metric is sometimes referred to as the intermittency coefficient as defined by e.g Townsend. Here again, my feeling is that this is worth being documented in the context of wind energy production.

6. In Figs. 10, & 12, it does not seem that the pdfs are normalized in such a way the integral is one. Am I right? Should not they be normalized?

Typos:
- Line 148, “superposition”
- Please rephrase “the results seem to get physical unreasonable“
- Line 184, maybe it should be added that the shear exponent will be defined later.
- Line 226, the word “astonishing” is an appreciation of the authors about their own work, “in good agreement” alone is more neutral.
- Conclusions, line 255, I think some words have been swept. Please reword.