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Atmospheric Measurement Techniques Discussions

Interactive comment on "Characterising vertical turbulent dispersion by observing artificially released SO_2 puffs with UV cameras" *by* Anna Solvejg Dinger et al.

Anonymous Referee #2

Received and published: 16 August 2018

The authors present an interesting new measurement technique with great potential of improving knowledge on turbulent dispersion. That being said, the data set is suboptimal to give a full demonstration of the power of the method (see my second comment below), and I wonder if this paper should be published at this stage, and not after obtaining better data. The analysis feels rather quick and rough, and I would prefer a more in-depth paper, because it is nearly impossible to judge the full quality of the method here. Furthermore, some of the turbulence theory need to be motivated in more detail.

Can you also specify what is the specific contribution of all authors to the actual results in the paper? I am not sure after reading the author contributions whether all of those

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Discussion paper

warrant co-authorship of the paper.

Specific comments:

P2, L7: What do you mean with the PDF has large fluctuations? Do you mean that the flow has large fluctuations, resulting in a wide PDF?

P2, L10: Your statement on direct numerical simulation is not correct. In order to produce meaningful DNS, it is not necessary to reach similar Reynolds numbers as in the atmosphere. Many of the statistics of the flow converge with Reynolds numbers far below those in the atmosphere, as can for instance be seen in van Heerwaarden & Mellado (2016, JAS), who show converging statistics in DNS a convective boundary layer. I consider the authors to have a look at the seminal paper of Moin and Mahesh (1998) and the appropriate interpretation of DNS as a research tool.

P3, L23-30: I get a little uncomfortable from this paragraph. Not all equipment was operational yet, your dataset contains a too small number of puffs for meaningful statistics, and later in the paper (P4 L8) you also refer to sub-optimal weather conditions. Why did you choose to publish this paper now, and not after you have obtained better data? As the method is so promising, why wouldn't you wait?

P6, eq 4: This theory only applies if the puffs have length scales far less than the production scales of the flow, as well as the dissipation scale. Can you give those numbers for the flow your experiment is embedded in?

P6, eq 5: Is the turbulence isotropic at the scales you are looking? Are the variances of the three flow components on the time scale of dispersion the same?

P12, L29-31: What is the exact interpretation of the t2 and t3 regime. You could make the link with eqs 4-6, and explain the physical meaning better.

Figures: Please make the figures such that they all have a consistent, and readable font size.

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