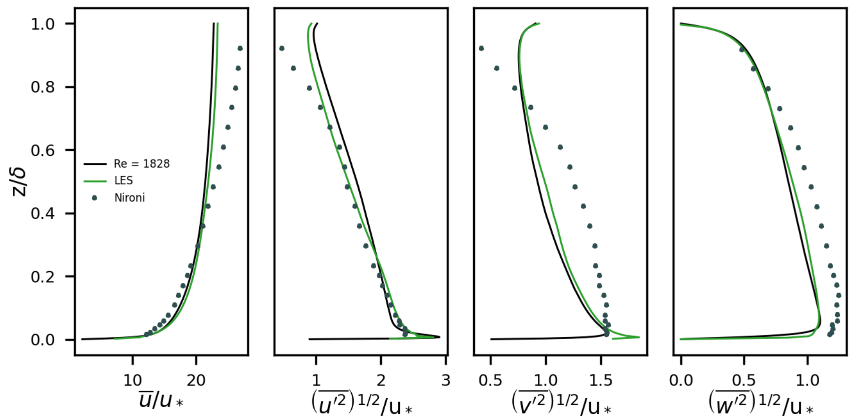
Authors responses to review 2

We would first like to than the reviewer for their kind words and time invested in our study. We will do our best to address all of their comments in the text below. In this document, we have repeated the reviewers comments in the *italics* and our responses are in the standard font.

This is an interesting study that uses the large-eddy simulation (LES) technique to test the validity of two common air quality measurements/models to estimate point source emission strength. Although the study finds substantial discrepancies between LES and measurements, it still highlights possible deficiencies in both approaches. I only have minor comments that I hope the authors would address.

1) I must note that the blame is mostly put on LES. On Line 245, the issue of the log-layer mismatching LES is discussed as a possible reason for overshooting the mean velocity profile, but this is essentially undermining the use of LES. There are several studies that addressed this issue (besides from Brasseur who reported both undershoots and overshoots depending on a variety of parameters). See for instance Bou-Zeid et al. 2005; Physics of Fluids).
We indeed did take the measured profile as the truth and blamed the deviation from the log-layer on LES. We attach here a figure that shows velocity and variance profiles (matching the first two panels in Fig. 2) for Nironis channel experiment, an LES with the same horizontal resolution as the one in this paper, and a DNS with moderate Reynolds number, $Re_\tau = 1828$. On this figure the LES and DNS match quite well and LES has slightly different mean wind profile than the one presented in the paper. The only difference between the LES from our paper and on this figure is the resolution in the z direction where the simulation in the paper has more grid points in the vertical (360) than the one in the figure (240). Following this, the blame for the overshoot in the mean velocity profile does seem to be with LES. However, after accounting for the difference in the mean winds between the LES and the experiment, the statistical profiles of the plumes (Fig. 3) match quite nicely. So yes, the LES has an overshoot in the mean velocity, but for our study that is not important since we use the experiment only for the validation of plume statistics and not for the analysis of the measurement techniques.



2) The authors argue in the introduction that DNS is becoming affordable and few paragraphs later mention that LES is expensive. It is fine to use LES in idealized conditions to test theoretical arguments, so I wouldn't under cut the approach.

Yes, we see how the way we formulated those sentences might raise a question of why are we not using DNS then. Now line 57 to the end of the paragraph reads: "DNS, as it resolves all details of the flow, would be ideal approach for studying plume dispersion, however, due to unfeasible computing costs involved, it cannot reproduce high Reynolds number flows (Pope, 2000). Nevertheless, in recent years it is becoming more affordable for atmospheric studies (e.g. Branford et al. (2011), Oskouie et al. (2017)), as computers have sufficient power to simulate atmospheric boundary layers with statistics that are slowly becoming Reynolds number independent."

3) The authors should at least comment/speculate on the effects of stability on their results. What do they expect in terms of statistics under unstable conditions?

Agreed. We have added the following paragraph to the Discussion section: "The plumes studied here were emitted into the neutral channel flow as it is the most canonical case of the atmospheric turbulence. Similar study should be performed for unstable and stable conditions. Based on our findings, we expect the unstable conditions to add to the variance of the plume as bouyancy effect adds to the production of turbulence. Conversely, for a stable atmosphere, we expect shorter averaging time (less plume transects) would be required to achieve 40 % accuracy."

4) The source characteristics need to be clarified. Emissions prescribed as a Gaussian distribution, as opposed to uniform source from one grid-cell mimicking a true point source, need to be discussed.

We did comment on the need of using a 3D Gaussian as opposed to emitting from a single grid cell in Section 3.1 where we discuss the implementation of sources. But we have opted for a dispersed source in order to avoid sharp gradient of scalar concentrations that would appear at the position of the source and could lead to numerical instabilities. Furthermore, Ardeshiri et al. (2020) in their LES study found that the sources have to be resolved over at least 4^3 grid nodes to have scalar variance statistics converge.