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Atmospheric Measurement Techniques Discussions

Interactive comment on “Sampling Error in Aircraft Flux Measurements Based on a High-Resolution Large Eddy Simulation of the Marine Boundary Layer” *by* Grant W. Petty

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Overview

In this study, the author uses an output from a Large-Eddy Simulation performed with a high-resolution, which explicitly resolved the major part of the turbulent structures. The domain-averaged turbulent fluxes can thus be considered as the "truth". This reference is then used to explore, with virtual track traversing the LES domain, the capacity of aircraft flux measurements to properly estimate the turbulent fluxes, the associated sampling problem and random errors. The results, compared with previous work on the topic (Lenschow and Stankov (1986), Lenschow et al., (1994)), lead to similar conclusions about the required length of the tracks to limit the sampling errors.

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Discussion paper

General comments

- Regarding the scope of AMT scientific questions, the question of the publication of this study in this journal may be raised. Indeed, even if the topic is about airborne flux measurements, the study is based exclusively on results from numerical simulations. It is regrettable that no observations are used in this study, either to be confronted with the simulation output or to apply the results obtained, for example on past measurement campaigns.

- The track definition used by the author can lead to flight tracks greater than the domain size thank to the cyclic boundary conditions of the LES. Nevertheless, as mentioned by the author, the finite LES domain is not able to reproduce structures greater than the domain size. Is a domain of 5,12 x 5.12 km^2 is therefore large enough to study airborne sampling and eddy correlation flux estimation? With a larger domain size, the characteristics of the simulated turbulent structures may be different. With this issue of the limited size of the domain, does a LES with a larger mesh grid and a larger domain have been appropriate? Taking the example of the University of Wyoming KingAir aircraft mentioned by the author, with a true air speed of 85 m/s and a measurement frequency of 25 Hz, the sampling spatial resolution is then about 3.5m. Thus, a grid mesh 3 times larger than the one used here could be adequate.

- There are many figures (19 in total), some of which seem redundant or could be concatenated. Several of them are simply mentioned in the text without being analyzed or discussed. The question of the relevance of these figures may arise, not helping to clarify the main message of the article. It obviously seems appropriate and necessary to present the simulation with the help of a few figures, however, it is only from figure n$^\circ$13 that the central purpose of the paper begins to be addressed.

Specific comments

Introduction:

- The works of Lenschow and Stankov (1986), Lenschow et al. (1994), and Mann and Lenschow (1994) were not only based on theoretical considerations and statistical models but also on observations. It might be useful to include in the introduction, some studies on experimental data and field campaign. In general, the introduction could be enhanced in terms of bibliographic references, such as Brooks and Rogers (1997) Cook and Renfrew (2015) or Brilouet et al. (2017).

- Line 34: The LES is able to resolve explicitly the major part of the turbulence but it remains a sub-grid contribution. Even if with a 1.25m resolution, this contribution becomes rapidly negligible with the altitude, it might be useful to mention that total turbulence = explicitly resolved + subgrid contribution. - After line 42, it is not clear if we are still in the introduction section or if the section "description of the method" has already started. It would be useful if the main goal of the study could be more clearly highlighted and if an outline of the article were provided at the end of the introduction before going into the details of the simulation and the method.

- Line 60: It is correct that using a LES to examine the aircraft flux sampling problem in MABL is unique. Nevertheless, it can be mentioned that previous studies compared LES outputs with airborne measurements such as Brilouet et al. (2020) even if the resolution was coarser.

Data:
- The case study is from the field campaign DYCOMS-II, Are there any observations that might be relevant to the study?

- The case study is a nocturnal cloud-topped marine boundary layer. When the author describes the environment, a few elements describing the main characteristics of this type of stratocumulus condition could be instructive for the reader (such as zi at the cloud top, the strong inversion with entrainment at the cloud top, . . .).

- Figure 2: the figure is rather small. The units of the power spectra are not mentioned. Does it might be interesting to present normalized spectra (by the variance:

$kF(k)/\sigma_X^2$)? Does the spatial wavelength is $\lambda = 1/k$ or $\lambda = 2\pi/k$?

- Line 95: It might be interesting to compare with previous works.

- Lines 96-98: At 40m height (0.05 zi), this is the surface layer. How much the turbulence is explicitly resolved at this height? What is the vertical profile of TKE resolved / total TKE? Also, the surface layer may have different characteristics than the layer above. Does the Monin-Obhukhov Similarity theory (MOST) is available? It would be interesting to enhance the discussion with some references on the turbulent structure inside the surface layer such as Katul et al. (2011) or Sun et al. (2016).

- Lines 99-100: Do the spectra of temperature and specific humidity reveal more energy at longer wavelength due to the influence of mesoscale on those parameters? If the domain was larger, would the wavelengths be longer?

- Line 101: What is the reason that the horizontal wind speed spectrum has no significant dependence on height?

- Lines 104-105: The author has chosen four representative heights, one at 10 m and another at 40 m. Are these heights characteristic of airborne measurements?

- Figures 3-5: 3 figures are considered for 4 lines. It would be interesting to concatenate them into a single figure. It will be easier to compare the characteristics of each parameters and their evolution with the height (for example with left panels at 10 m, middle panels at 40 m and 100 m and right panels at 400 m with a parameter by row).

- Lines 106-110: Also, a link with previous work would be valuable.

- Figure 6: Is this figure really essential to the article?

- Line 114: It might be helpful to define the sensible (H) and latent (E) heat fluxes. Commonly, the E notation refers to the surface moisture flux or evaporation ($E = \rho \times w'q'$). Perhaps the LE or LvE notation is more appropriate for the latent heat flux.

- Lines 114-119: Is the definition of sensible and latent heat fluxes and their expressions as a function of fluctuations valid at different altitudes in the boundary layer? Is it not defined only for surface exchanges? The sensible heat flux is the amount of heat exchanged between the surface and the atmosphere and the latent heat flux represents the energy released or absorbed during a phase change. I may be mistaken and in that case, I apologize for this unwelcome comment.

- Figures 7 and 8: These figures are not described or analyzed in the article. Are they essential to the article?

- Line 130: It would be interesting to explain the TKE profile and how this is expected, in terms of the processes involved, given the case study under consideration. Here again, a connection with previous studies on this subject would be appreciated.

Integral length scales

In this section, the work of Lumley and Ponofsky (1964) could enhance the bibliography as a pioneer on these issues.

- Line 140: It is the first time, since the introduction, that the random error is mentioned. As this is the main focus of the article, wouldn't it be a good idea to highlight it further? The current design of the article suggests that it is secondary to the integral scales.

- Line 149-150: To introduce the random error in a simplified point of view, is the equation 1 of Lenschow and Stankov can be relevant?

- The spatial correlation $\rho_{w\psi}$ is defined twice (line 156 and line 164).

- Line 158: In order to specify the experimental difficulties in estimating the integral length scale, the study of Durand et al. (2000) could be instructive.

- Figure 12: Even if the random error definition contains the correlation $\rho_{w\psi}$, is the figure really essential to the article?

Simulated aircraft measurements

- Lines 208-209: This sentence perfectly summarizes the main topic of the study. Isn't it a bit lateÂă? This message does not appear clearly enough throughout the article.

- Lines 246-247: As mentioned in the general comments, I have some concerns about the domain size with respect to the characteristic scales of fluxes that can be observed during airborne measurement campaigns. Consequently, the results that will arise from this study seem difficult to be transposed to measurement campaigns.

- Line 249: Another way to check Taylor's hypothesis, for airborne measurements, the true air speed (here $V = 85m/s$) can also be compared to the intensity of the turbulence $(\overline{u'^2})^{1/2}$. If $V \gg (\overline{u'^2})^{1/2}$ then the statistical properties of the turbulence field are assumed to be unchanged over the considered time interval.

Results

- Figures 14-16: These three figures could be concatenated into one. Moreover, even if these figures are at the core of the study, they are barely detailed and analyzed (Figure 15 is barely mentioned).

- Line 261: Including bibliographic references would be valuable.

- Figures 17-19: In order to facilitate the understanding of the figures, it can be useful to keep the empirical RMS error in red rather than changing the color. Are the parameters in blue necessary? If so, would it be better to include them in a table? As the minimum track length L10 for 10% relative accuracy is one of the main results, would it be a useful to group them together, for each flux and each altitude, in a table?

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