Reply to referees

26th June 2020

First referee - prof. dr hab. inz. Jacek Pozorski

1. **One would welcome that Sec. 4.2.1 (stratocumulus cloud-top simulation), important for the thesis, be more self-contained. In particular, I have not grasped where the reduction in Re (factor of 300) comes from? Is it due to rescaled L_0 ? Or viscosity? Is the size of dissipative eddies correctly reproduced in the DNS? What are the boundary conditions (BC) of the simulation? I guess, these are periodic BC in x and y (?). But what about z? Are the BC the same as in the LES case of Sec. 7.3.2? Probably not, as the velocity statistics at the extremes of z differ between Fig.4.3 and 7.7. Please explain.**

   The reduction in Re (factor of 300) is due to an increase in the viscosity. The viscosity value is 0.01 m^2/s, which is 3-order of magnitude larger than the atmospheric value. The ratio between the grid spacing and the Kolmogorov length η is approximately 1.5. It is assumed in Schulz and Mellado (2018) that the size of dissipative eddies is entirely reproduced and the use of this grid-spacing has no significant effect on the statistics of small scales. The boundary conditions imposed at the top and bottom are zero normal velocity and zero normal derivative of the horizontal velocities. Periodic boundary condition is applied in x- and y- directions. In fact, the boundaries of the computational domain are so far from the turbulent region that there is no difference between using either Dirichlet or Neumann boundary conditions [J. P. Mellado priv. comm.]. Sensitivity studies were carried out to check this. Fig. 4.2a shows only the turbulent regions, far from the vertical boundaries. For the stratocumulus simulation, the initial conditions that is imposed inside the cloud has stronger effects than the boundary conditions at the boundary of the computational domain. Hence, boundary conditions are not the same as in LES.

2. **Concerning Sec. 4.2.2: one would wish some quantitative results to be recalled here, as in Fig. 4.3. As amply discussed in the following, the flow anisotropy is an important factor that deteriorates the estimation of ϵ, right? And the profiles of rms(u_i) would provide some idea about at least the large-scale anisotropy. As the Reynolds number is rather low, the anisotropy may persist down to the smallest resolved scales. From this point of view, given the non-homogeneity of turbulence in the CTL case, the results reported for example in Tab. 5.2 are expected. On the other hand, the Author states (concluding on the estimates of ϵ based on different velocity components) that "this makes the isotropy assumption questionable" (p.44). Actually, already given the r.m.s. velocity profiles in Fig. 4.3b, one should not expect the turbulence to be isotropic. Please explain. Perhaps even, a better estimation of ϵ may be imagined, as supported by the data on anisotropy expressed by the respective levels of u', v' and w'? Not sure, but this might be a path worth some reflection and further exploration.**

   The statistics of velocity field in the convective boundary layer is shown in figure 2 of Mellado et al. (2016). From this figure, information about large-scale anisotropy can be deduced. I agree with the Referee that large scale anisotropy can persist to small scales especially in low Re DNS. The author investigated small scale anisotropy because it directly influences ϵ-retrieval. In fact, analyzing the relationship between large and small scale anisotropy is an interesting subject for future work.

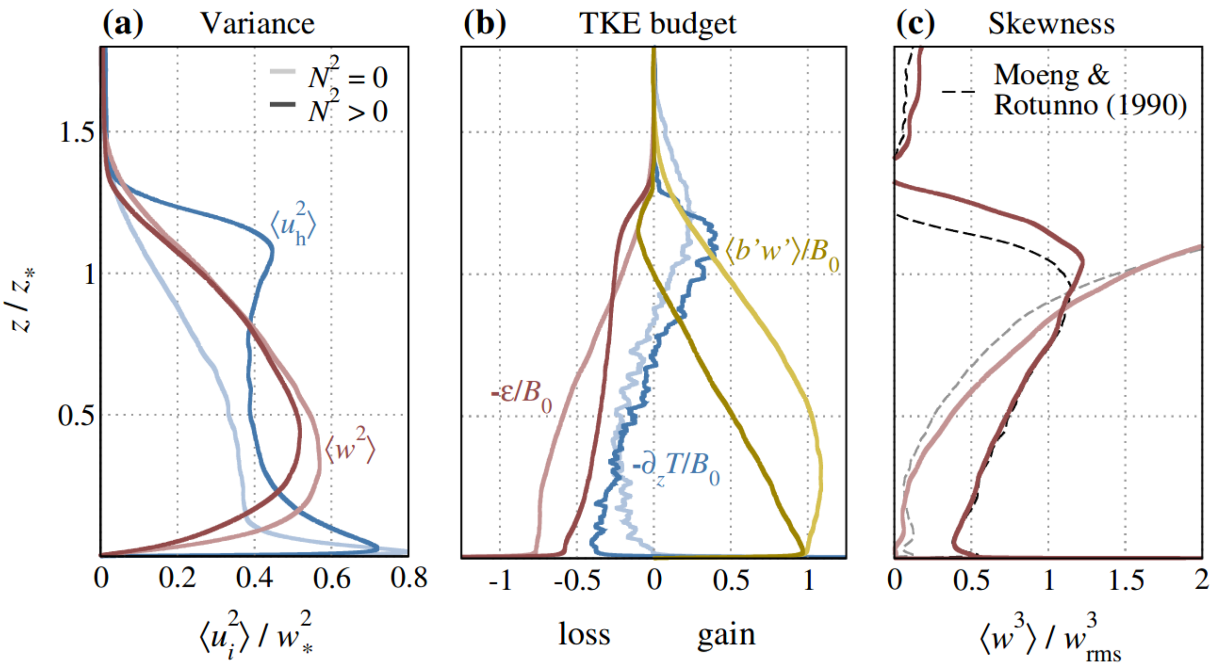

Fig. 2 Large-scale properties at $z_*/z_\kappa \approx 680$. $u_h = (u_1^2 + u_2^2)^{1/2}$ is the magnitude of the horizontal velocity. $T = \langle w'u_i'u_i'/2 + p'w' - u_i'\tau_{i3}' \rangle$ is the vertical turbulent flux of TKE and $\varepsilon = \langle \tau_{ij}' \partial_i u_j' \rangle$ is the viscous dissipation rate, $\tau_{ij} = \nu(\partial_i u_j + \partial_j u_i)$ being the components of the viscous stress tensor

3. **Fig. 5.15 seems to be quite rich in information (and perhaps usable in future work?). Any general comment on it?**

   Figure 5.15 was made to explore the bias and scattering of these ϵ estimates at different averaging windows. The figure showed that the power spectra estimate ϵ_{PS} are sensitive to aliasing, which can be due to finite frequency while number of crossing estimate ϵ_{NC} has a large scatter than ϵ_{PS}. Detailed analysis of both artificial time series and data from physics of stratocumulus cloud campaign (POST) has been performed recently by Wacławczyk et al. (2020). Errors due to finite averaging window, finite cut-off frequencies and different fitting ranges were discussed in this paper.

4. **The role of external intermittency in estimating the dissipation rate is well explained (the correction factor γ in Sec. 5.2.3, etc.). But then, the concept of internal intermittency, due to Kolmogorov (1963), is also invoked in the text. However, I have not found it in Chapter 3. What is in practice the role of internal intermittency when working with the estimates of ϵ?**

   As discussed in the thesis, external intermittency influences the number of crossings (related to the Liepmann microscale Λ). Internal intermittency can affect the statistics of velocity gradient (related to the Taylor's microscale λ_n). In the original work of Rice (1944) where the relation $\lambda_n/\Lambda \approx 1$ was derived, assumptions on the Gaussianity of the signal and its derivative were made. The latter assumption does not hold in the case of turbulence, where the internal intermittency manifests in strongly non-Gaussian tails of the probability density functions of velocity derivarives. Still, Sreenivasan et al. (1983) argue the relation $\lambda_n/\Lambda \approx 1$ is valid for a larger set of signals also with non-Gaussian statistics. In the airborne POST dataset the ratio of Liepmann to Taylor microscale λ_n/Λ is in fact close to 1, however, in the DNS data the ratio λ_n/Λ was approximately equal 0.7. In the thesis, I explained that the reason for this value (i.e. $\lambda_n/\Lambda = 0.7$) is possibly related to the statistics of small scales. Hence, I invoke the concept of internal intermittency.

   The influence of internal intermittency on the Kolmogorov (1941) scaling was investigated by Katul et al. (1994). Those authors compared the structure function of atmospheric measurement dataset with its conditional statistics where intermittency effects were suppressed. They found out that for second-order structure function, the internal intermittency effects were not very significant, thus they should not influence TKE dissipation rate retrieval considerably.

5. **Concerning the studies reported in Chapter 7, in particular the determination of statistical properties of d, using not-so-isotropic (neither homogeneous) data: perhaps, these properties, and the PDF($|d|$) in particular (and not just the autocorrelation computed in Sec. 7.5) could have been determined from the data on forced isotropic turbulence as well (unless this have been done in the literature already?).**

   Following the Referee's suggestion, the author performed also analysis of the statistical properties of d and determined its PDF based on dataset on forced isotropic turbulence (Yeung et al., 2012) downloaded from Johns Hopkins turbulence open Databases (JHTDB) (`http://turbulence.pha.jhu.edu/`). These results were not shown in the thesis. The PDF of d shows similar properties as in the case of stratocumulus boundary layer dataset (see figure 1). To the best of the author's knowledge, such results have not been presented before. Only the mean values of $|d|$ as a function of wall distance were presented by Salvetti et al. (2006). In an attempt to create synthetic turbulence, Ding et al. (2010) applied random values of d (assuming that the PDF of d has a Log-Poisson distribution) to a spatially randomized fractal interpolation technique. They performed this in order to reproduce the correct scaling of high-order structure functions but no study was performed on either experimental, LES or DNS datasets to investigate the PDF of d.

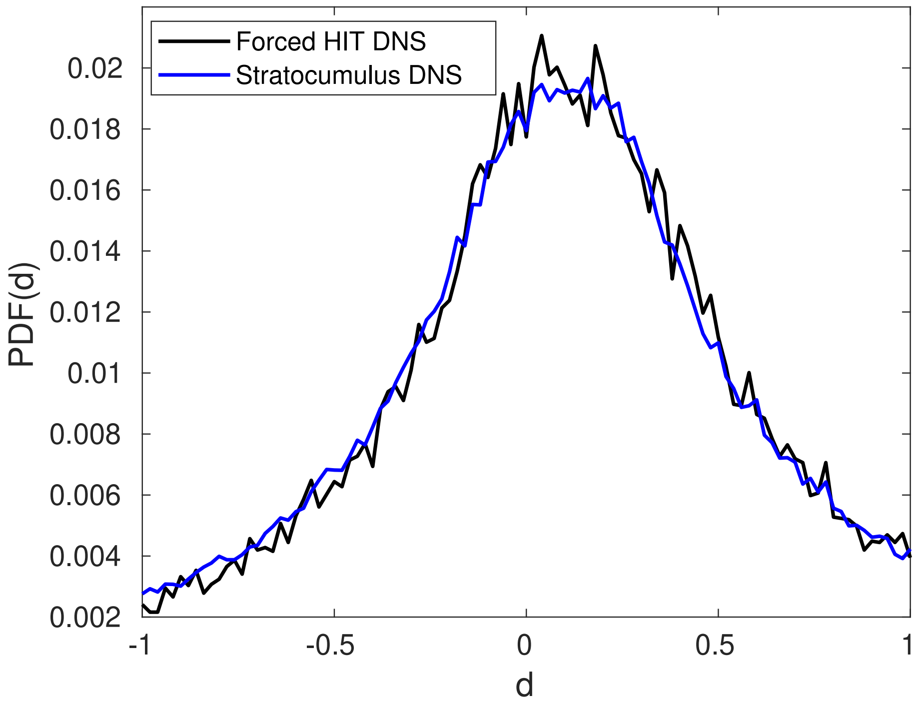

Figure 1: Probability distribution of the absolute value of the stretching parameter $|d|$ from filtered DNS of stratocumulus cloud-top and filtered DNS of forced homogeneous isotropic turbulence data. DNS velocity fields are filtered with wavenumber within the inertial range.

6. **Sec. 7.2: it is unclear how the 5-point algorithm (yielding d_1 and d_2 , Fig. 7.4) is applied to the whole velocity signal to infer d values from it (Fig. 7.5a)? Then, I have some concerns about the procedure: it seems from Fig.5 that $< d >$ (the mean value) is larger than 0, is it? If so, then the PDF is certainly not symmetric, so why then one should introduce PDF($|d|$)? Also, it is not clear why "the ensemble average $< |d| >$ should be comparable to the channel flow data of Ref.[118]" (p.91) which is a strongly non-homogeneous case? What are these mean values?**

   Mazel and Hayes' algorithm (see fig 7.4) is applied on 1-D velocity signal, by selecting every 5 grid points, moving from the first grid point to the last. The values of d_1 and d_2 generated for a part of the signal were shown in fig. 7.5a. In fact, as the Referee noted, the mean value of d is slightly positive and ≈ 0.0054. See figure 1 above. Moreover, the PDF is in fact non-symmetric and slightly negatively skewed with a skewness of -0.0384. In principle, it would be possible to apply this shape of

PDF to FIT procedure. However, I assumed that this asymmetry is small, at least in comparison with other simplifications which were employed in this study, such as restricting the range of the values of d. The author applied a symmetric PDF of d because it was easier to implement numerically. The author agrees with the Referee that this issue should be investigated further. In particular, it is interesting to study the relationship between the PDF of d and the PDF of velocity derivatives. The later is also negatively skewed (Burattini et al. 2008). The DNS of stratocumulus cloud-top (STBL) is strongly non-homogeneous like the channel flow data reported in Ref. [118]. The vertical mean profile of d is shown in fig. 7.9b in the thesis. The mean value of d in the z-direction ranges from 0.25 *in the outer-cloud region to approximately* 0.4 *in the core cloud region while in Ref. [118], a mean value of* $|d|$ *ranges from* 0.6 *at the wall and* 0.7 *away from the wall was reported.*

7. **Continuing on the proposed improvement of the fractal interpolation technique (p.86, p.91): judging from the plot of local stretching parameters (Fig.7.5a) and from the PDF (Fig.7.8a), when the values d such that $|d| < 0.5$ are discarded, then not much seems to be left (?). Actually, this is my most serious concern about the proposed improvement of the method. The argument (p.86) that "only then a fractal signal will dissipate energy" does not seem to be relevant as the reconstructed field does not evolve dynamically (as in the N-S eq.). Rather, as in the applications shown in Chapter 9, it is used to advect a passive scalar ("the attribute") or to provide an "enriched" local fluid velocity for particle motion. Please explain.**

   The author agrees that most of the values of d are less than 0.5*. In the thesis, it was first mentioned that the iterative procedure in the limit* $n \to \infty$ *creates a continuous function* $u_f(x)$ *provided that the stretching parameter d obeys* $0 \leq |d| < 1$ *(see Barnsley (1986) for details). Hence, the range of d was restricted. However, with such a choice the energy spectrum of the reconstructed signal was underpredicted (see figure 2 below). To improve the reconstructed energy spectra, the author looks for another possibly physically-based constraint. For this reason, the condition* $|d| > 0.5$ *was used. Both large values* $|d| > 1$ *and highly probable small values* $|d| < 0.5$ *are possibly connected with the phenomenon of internal intermittency. Neglecting large values of* $|d|$ *connected with high-amplitude, rare events led to energy underprediction. This underprediction was compensated by the second constrain, where the small scales* $|d| < 0.5$ *were also neglected. Although, the range of d is restricted, in the author's opinion, the proposed method provides an improvement in comparison to the formulations where constant values of d are assumed.*

8. **In the a priori LES test, the residual kinetic energy k_r is introduced through Eq. 9.4. When the DNS solution is filtered, the fluid kinetic energy decreases, so $k_r > 0$. Here, the residual energy may be negative (which is, NB, a priori unusual), as the filtering (or tilde) operator is not idempotent (unlike the ensemble average in RANS). As a consequence, the PDF of k_r is endowed with a quite large negative part. Please comment on these unusual features.**

   The notion "residual kinetic energy" in fact refers to the energy of all subgrid motions of a filtered DNS field. As the filtering removes certain range of scales, the "residual kinetic energy" is positive. In the thesis the term k_r *is called the "residual energy" but in fact it was obtained in the procedure of test filtering and could be interpreted as the contribution to residual stresses from the largest unresolved motions (Pope, 2000). Due to the lack of high-Reynolds number DNS data with long inertial range, LES velocity was treated as a reference field. The test filtering procedure was applied to LES field and in this case, the energy transfer may locally occur from small to large scales (backscatter), which results in negative values of* k_r*.*

9. **Concerning the autocorrelation $R(\tau)$ of the stretching parameter (Sec. 7.5): it is not clear why are we interested in this quantity? Is it meant for future applications of FIT? Which ones? Then, it would be perhaps worthwhile to also compute the decorrelation time out of the Lagrangian function R. Or even from the Eulerian one with the application of frozen turbulence hypothesis to see whether the decorrelation times substantially differ among these three cases. Also, this might be of future use for FIT applied to particle/droplet motion.**

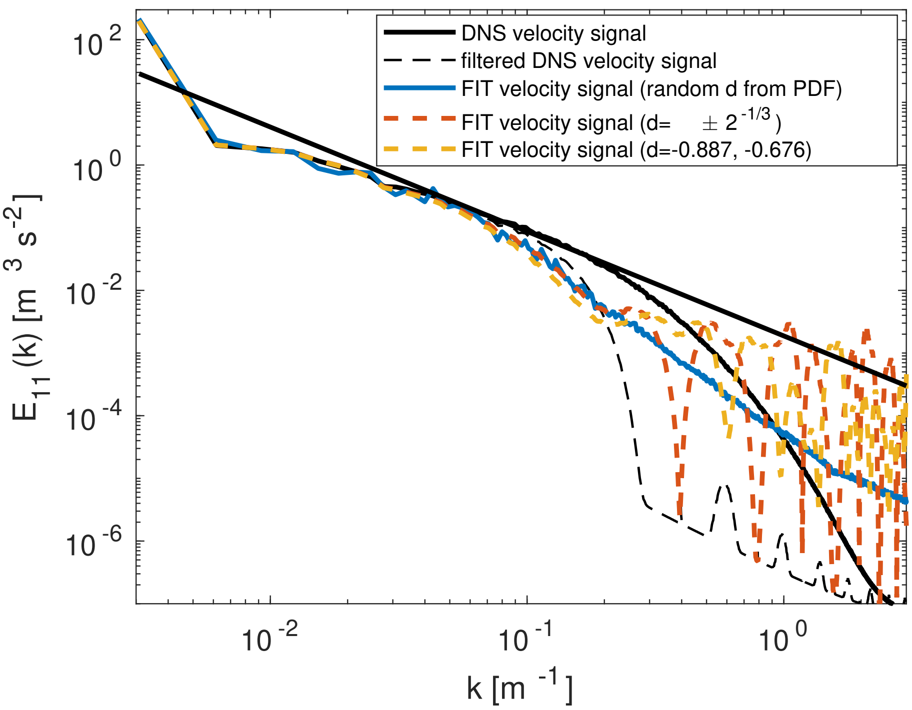

Figure 2: Longitudinal energy spectra of u velocity component for DNS and FIT with constant stretching parameter $d = \pm 2^{-1/3}$, with constant stretching parameters $d = -0.887$ and $d = -0.676$ and with random stretching parameters $|d| < 1$ from calculated PDF

The purpose for computing the autocorrelation $R(\tau)$ of the stretching parameter is for future application of FIT in LES. I observed that the stretching parameter decorrelates with the characteristic time scale of the order of the Kolmogorov time scale τ_η. Since the time steps used in LES are much larger than the Kolmogorov time scales, this implies that the stretching parameter can be chosen randomly after each consecutive time step in LES. In fact, as the Referee suggested, it will be an interesting work to check the autocorrelation of d along Lagrangian particle path.

Minor remarks

1. p.11: the statement "sound waves are completely neglected... since (they) propagate via density variation" seems inaccurate in the presented context (or even wrong – why?).

   This statement is changed to: This assumption makes the propagation of sound waves impossible. Sound waves are not neglected but the assumption that the density variations are negligible makes it impossible for sound waves to propagate.

2. rigorously, the integration limits in Eqs. 2.18 and 2.19 are incorrect;

   The author agrees with the Referee. This is a mistake on the part of the author. The integration limit is changed to:

$$\langle \boldsymbol{U}(\boldsymbol{x}_0, t_0)\rangle \approx \langle \boldsymbol{U}(\boldsymbol{x}_0, t_0)\rangle_{\Delta T} \equiv \frac{1}{\Delta T}\int_{t_0-\Delta T/2}^{t_0+\Delta T/2} \boldsymbol{U}(\boldsymbol{x}_0, \tau) d\tau.$$

   and

$$\langle \boldsymbol{U}(\boldsymbol{x}_0, t_0)\rangle \approx \langle \boldsymbol{U}(\boldsymbol{x}_0, t_0)\rangle_{\Delta \mathcal{V}} \equiv \frac{1}{\Delta \mathcal{V}}\int_{\Delta \mathcal{V}} \boldsymbol{U}(\boldsymbol{x}, t_0) d\boldsymbol{x}$$

3. Fig. 7.5b: to better appreciate the final effect of FIT reconstruction, it would be instructive to add a 2Δ-filtered velocity signal to the picture, even just in the zoom-in chunk (the inset plot);

   Fig. 7.5b is changed (see figure 3):

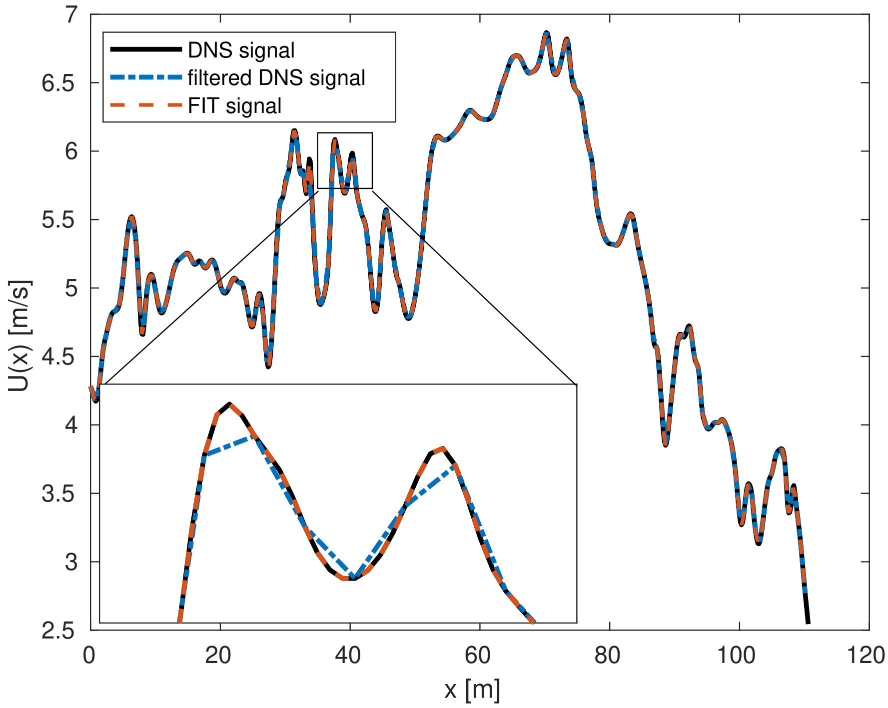

Figure 3: 1D DNS velocity signals showing the original, filtered and the FIT reconstructed signal using local values of d in (a)

Second referee - dr hab. Bogdan Rosa, prof. IMGW-PIB

1. **In the Abstract, the Doctorate states that the analysis of fine-scale structures can contribute to better parameterization of climate models. In general this is true, but the parameterization of dynamics in small scales is much greater for modelling weather forecasts.**

   The author agrees with the Referee. In the thesis, small scale dynamics was not modelled. The parameterization studies of dynamics of small scales are more significant in improving numerical models used for weather forcasting. However, analysis performed in the thesis can also contribute to our knowledge on small scale turbulence.

2. **Page 13 The statement "In general, the turbulence flows can be described as a random field" is a big simplification. In a typical turbulence flow there are structures that have a certain spatial organisation. This has a direct impact on the dynamics of cloud processes, e.g. the average speed of falling droplets in turbulent flow is higher than in air without turbulence. And in a random field, the speed of the drops does not change.**

   I assumed that a random field is a field which has both spatial and temporal correlations. Several authors such as in Fung and Vassilicos (1995) and Pinsky et al. (2007) developed models for such random fields. Also, Pope (2000) defines random fields as a field with spatial and temporal correlations in contrast to random variable.

3. **The doctoral student examines the differences in energy dissipation values calculated using different methods in quite a detailed way, while the analysis of statistical uncertainties of the obtained results is treated more cursory.**

   The datasets used for the analysis of energy dissipation rates are from DNS with sufficiently large averaging window. The statistical errors were very small and hence, not investigated. Preliminary error analysis was performed for artificial aircraft that flies through the cloud and measures velocities with different frequencies. The results were presented in the thesis in figures 5.14 - 5.16. Also, further statistical analysis was done by other authors in Wacławczyk et al. (2020) using both artificial and physics of stratocumulus cloud (POST) velocity time series. Errors due to finite averaging window, *finite cut-off frequencies and different fitting ranges were also discussed in this paper. They showed that the power spectra estimate ϵ_{PS} are sensitive to aliasing, which can be due to finite frequency while number of crossing estimate ϵ_{NC} has a large scatter that is comparable to power spectra estimate as k_{cut} decreases.*

4. **It is difficult to deduce from the text of the dissertation whether DNS data from one time step were used to determine energy dissipation or whether the data were averaged over a certain period of time. This may be important for the accuracy and generality of the results obtained.**

   DNS data from one time step, averaged over homogeneous x- and y- directions, is used for this analysis. The data was provided by Professor J. P. Mellado (Max Planck Institute for Meteorology, Hamburg, Germany). I assume with this data I can calculate the statistics of turbulence with a reasonable accuracy. Such approach is also used e.g. in Mellado et al. (2016), where only space averaging was used to calculate variance and skewness of velocity components and the turbulence kinetic energy budget, from the DNS data of the convective boundary layer. With the spatially-averaged statistics, Mellado et al. (2016) further studied evolution of the characteristic length scales in time.

References

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