prof. dr hab. inż. Jacek Pozorski
Group of Multiphase Flows
Institute of Fluid-Flow Machinery, Polish Academy of Sciences
Fiszera 14, 80-231 Gdańsk, Poland
Phone: +48 58 5225145, E-mail: jp@imp.gda.pl

Gdańsk, February 22, 2020

Review of the doctoral dissertation

“Analysis and modeling of small-scale turbulence”

by Mr. Emmanuel O. AKINLABI

The present opinion has been prepared according to the request of Prof. Zygmunt Lalak, President of the Scientific Board for Physical Sciences, Faculty of Physics, Warsaw University, from Jan. 15, 2020. The PhD thesis has been directed by Prof. Szymon Malinowski and advised by Dr. Marta Wacławczyk.

Actuality of the subject

Physically-complex flows at high Reynolds numbers, such as turbulence in the atmosphere, remain a challenge for theory and modelling. For the last fifty years or so, the progress in understanding such flows has been enormous, including the computational studies in terms of fully-resolved simulations, known as DNS, and large-eddy simulations, or LES.

The present PhD thesis focuses on theoretical and modelling issues involving smaller turbulent scales (from the inertial range down to the dissipative eddies). A huge body of evidence has been gathered over the years on the flow structure and dynamics at these scales in terms of various PDFs, statistics of velocity and vorticity, etc. One of the key variables in this respect is the dissipation rate of turbulence energy ε (called the dissipation rate for short). A better estimation of this quantity, out of limited, 1D flow velocity series available, is of importance in experimental fluid dynamics (also when validating turbulence models); it may also be useful for data assimilation in weather prediction. Then, LES increasingly often serve as predictive tools in meteorology, but one of the pacing items on their progress are subfilter processes that may, e.g., control the growth of cloud droplets by condensation or collisions and coalescence. The present PhD thesis is concerned with both: (i) a better estimation of ε and (ii) an improved modelling of the subfilter velocity field LES. Although these two issues may seem quite disparate at first sight, they have much in common, actually. Both heavily rely on the physics of small scales in turbulence; also, some of the recent DNS and experimental data sets serve as reference here and are explored in both cases.

In my opinion, the subject remains of actuality in terms of its practical as well as fundamental interest. The databases relevant to the atmospheric turbulence are available for analyses, development of new ideas and validation purposes. Therefore, a PhD thesis devoted to such studies is timely and well-justified. Moreover, the dissertation is well rooted in the expertise and achievements to date of the research group on atmospheric physics at the Institute of Geophysics, University of Warsaw.

Contents of the PhD thesis; assessment of the original contributions

The thesis starts with recalling some fundamental notions on turbulence, governing equations of atmospheric flows and the basics on LES (Chapter 2). The DNS of the flow cases used in the following are covered in Chapter 4. They refer to a two-phase atmospheric flow (stratocumulus cloud-top mixing layer, CTL), which is relevant for cloud microphysics, and a single-phase convective boundary layer (CBL). Fundamentals on various methods for the estimation of ε from velocity time series (Chapter 3) are followed by analysis of these methods in the selected cases (Chapter 5). To this aim, both DNS data sets are explored.

The second part of the PhD thesis covers the reconstruction of small (subfilter, subgrid) flow scales, otherwise not resolved in LES. A structural type model is used for the purpose and further developed by the Candidate. It is based on the fractal interpolation technique (FIT) of a turbulent velocity signal. The basics are recalled in Chapters 6 and 7, together with the determination of the key model quantity, which is the stretching parameter *d* and its probability density function, estimated out of the DNS and LES studies, as well as measurements, reported earlier. Once these statistics of *d* are gathered, they are used in Chapter 8 to demonstrate how the FIT works in retrieving the subgrid part of the turbulence kinetic energy spectrum. Some preliminary results on possible applications of FIT are shown in Chapter 9, including the reconstructed fluid velocity for use in the dynamics of particles or droplets in turbulence. Let me just note that the title of Chapter 7 (model description) is not quite adequate; mostly, the data sets are explored there to gain insight into the statistical properties of *d*, including its PDF and autocorrelation.

The structure of the thesis is fine in general but it might have been conceived a bit differently. In particular: (i) the number of chapters (some being really short) seems excessive; (ii) the known facts and the new material are quite mixed up in Part 1 of the document; also, why not move Chapter 4 to introductory part, after Chapter 2?; (iii) the results reported in Sec. 9.2 seem quite preliminary so they not necessarily belong here.

Concerning the novelty aspects in Part 1 (Chapters 2-5 on the estimation of the dissipation rate ε): very interesting are the results of Chapter 5, and in particular a comprehensive analysis of the CTL case reported in Sec. 5.1.1. They are original and well documented. Also, a modification of the assessment method (Sec. 3.4) has been proposed by the Author. The difficulties reported of Part 1 stem from the fact that the atmospheric flows are rarely isotropic (and the ones studied in the thesis are not, indeed). Therefore, estimation techniques using the one-dimensional velocity time series and based on the isotropy assumption are burdened with error from the outset. The Author looks at the anisotropy caused by buoyancy and external intermittency (next to a laminar-turbulent interface) to see how it affects the various methods applied to assess the dissipation rate.

The original contributions in Part 2 are several. First, the discovery that the PDFs of the stretching parameter *d* in FIT, obtained from the inertial range velocity series, are independent on Re. Then, an improvement of FIT with random values of *d* is proposed. It has been found to work better than the existing proposals, as proven on the data sets from DNS, LES and experiment (POST). In particular, the exponential (non-Gaussian) tails of the PDFs of velocity increments are retrieved using the new variant of FIT. At last, assessment of the velocity divergence caused by the fractal interpolation and of the computational overhead.

The Author gathered and documented a valuable, original material. Apart from interesting theoretical insights into small-scale turbulence, the PhD study has the flavor of “big data” exploration which becomes affordable and also increasingly popular nowadays.

Issues for discussion, remarks and questions

As a whole, the dissertation makes an interesting reading; the text is rather clear and written in good English. The state-of-the-art is adequately presented: the bibliography consists of nearly 160 entries, including a selection of very recent research papers. The way of reasoning, new developments and results are in general sufficiently detailed. However, it is natural to notice some points that are not obvious (at least for this referee), may call for a broader explanation, or trigger questions and comments. Also, there are some inaccuracies or incorrect statements. The major remarks are listed below (basically, in the order of appearance in the text). May the Author address them in a written reply to this opinion; also, some questions may serve as a support for discussion at the public defence. Some minor remarks are shifted to Appendix.

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.

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.

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

5) 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?

6) 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?).

7) 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?

8) 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.

9) 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.

10) 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.

Let me reiterate that, notwithstanding these remarks and some critics, I have no doubt that the Candidate produced a decent body of original material of good research quality, as reported in his PhD dissertation.

Final conclusion

The doctoral dissertation presented by Mr. Emmanuel AKINLABI provides a proof of his good knowledge of general fluid dynamics and both analytical and computational skills. He applied these skills to a problem (two, actually) of atmospheric turbulence. The candidate has demonstrated his capabilities to critically analyse the bibliography of the subject, he gathered and documented valuable findings, and summarized them in the doctoral dissertation. A considerable part of the original material of his thesis has already undergone an independent scrutiny and has been published as regular papers in *Flow, Turbulence and Combustion* (2019) and *Journal of Atmospheric Sciences* (2019). Both are known as quality archival journals. The candidate attended with presentation a few international conferences, including *17^{th} European Turbulence Conference* (Torino, September 2019) which is, to my knowledge, a leading European event in this field with ample participation from the overseas.

Given all the above, I recommend that Mr. Emmanuel AKINLABI be admitted to a public defence of his PhD dissertation.

Appendix: Minor remarks on the PhD dissertation

I refer here to the points where: (i) more explanation is suggested for the sake of a better clarity or rigorous writing, (ii) some notions/statements are incorrect; (iii) there are mistakes in the English text or (iv) other editorial improvements are in order. Only some imperfections of this kind, deemed more important or occurring a few times in the document, are listed below.

a) the title of the dissertation sounds quite general; perhaps, modifying it like "…of small-scale atmospheric turbulence" would better define the scope of the work;
b) the List of Figures is perhaps less useful in a PhD thesis than a List of Symbols (missing here) would be for a prospective reader;
c) throughout the document, punctuation is sometimes incorrectly applied in displayed math formulae (Eq. 2.1, no full stop after Eq. 2.3, for example), making the text less smooth to read.
d) Abstract (p.6) and Sec.1.1 (p.5): "access" → "assess";
e) A consistent notation for dimensional quantities is recommended throughout the document: preferably use 9.81 m/s^2 rather than 9.81m/s^2 or 9.81[m/s^2], etc. (all variants are used by the Author);
f) consistency is also recommended when referring to equations: either "Eq. x.y" or "Eq. (x.y)"; the same for "fig. x" and "Fig. x";
g) 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?).
h) apparently, the meaning of q_c in Eq.2.29 is not explained (?);
i) rigorously, the integration limits in Eqs. 2.18 and 2.19 are incorrect;
j) the results of ε reported in Tab. 5.1ff, and also in some figures, may have been normalised with ε_{DNS} instead and not with B_0. This way, the discrepancies w.r.t. the DNS level would be readily grasped;
k) Figures and tables should not appear in the text before the page of first reference to them, see for example p.44: Table 5.1 and Figure 5.7;
l) Fig. 5.16, vertical axis: the use of "std" is, NB, non-standard; why not r.m.s. or RMS?
m) p.69: I do not quite agree with the statement "SGS model errors (in LES) can lead to progressive divergence of particle trajectories w.r.t. those from the DNS". Actually, what matters in both DNS and LES are the statistics of particle motion, and not the individual trajectories (cf. also strong and weak solutions of stochastic differential equations);
n) Contrary to what is promised in the beginning of Chapters 6 and 7, the new FIT is actually "described" and "explained" in Sec. 8.1 only;
o) Fig. 7.5b: to better appreciate the final efect 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);
p) Eq.9.2: viscous term;
q) p.105: "energy of subgrid stresses" - ?
r) Bibliographic data of some References are incomplete [53, 73, 94, 122, 142, 148] or faulty [17, 133, 155]; also, the use of lowercase and uppercase letters in some titles and journal names needs to be revised.