Arkady Tsinober

The Essence of Turbulence as a Physical Phenomenon

With Emphasis on Issues of Paradigmatic Nature

Second Edition

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To my wife

Preface to the Second Edition

The main issue of concern and emphasis of the present edition remains the same as in the first edition and which is termed same way—the essence of turbulence. Likewise, the main premises for this version remain as for the first edition. These are the absence of theory based on first principles, the long-standing paradigmatic crisis and inadequate tools to handle both the problems, and the phenomenon of turbulence. Therefore, all the difficulties and the like were the same as described in the Preface to the First Edition. Moreover, the structure and the outlay remained basically the same mostly for the reader convenience.

However, the motivation and the reasons to go for a second edition although similar are considerably different. Apart from an updating, these are due to the need for the extensions/amendments and clarifications, and reiterations in specific contexts of key points and similar. The text of the first edition appeared to be too laconic as the book is not an introduction for the novice—the readers need a reasonable theoretical background in spite of the absence of theory and even because of this as there is no guiding theory based on first principles. Also judging by responses, the first edition appeared to be not an "easy reading"—but who can take the risk of claiming that turbulence is easy especially with continuing increase of the amount of publications into the existing ocean, though as before without any considerable progress as concerns fundamental aspects. Moreover, many new publications belong to the category of "interpretational abuse" and alike which are "aided" by the absence of theory based on first principles. Consequently, the revision was made throughout most of the text to one degree or another. The emphasis is given to what is related to the issue of "essence" including its meaning and issues of paradigmatic nature and related also with special attention to the Epilogue with a triple increase in its volume. All this along with fighting the most difficult temptation of considerably increasing the volume of the book as there are many remaining interesting issues worth to be addressed. The volume of the present edition is increased by 50% as compared to the previous edition without adding any "water." This along with the addition of some issues not touched in the first edition. The savings in the volume are also due to the increase of the emphasis on

constructive aspects and issues of general nature, i.e. in part due to qualitative reprocessing without quantitative changes due to added pages.

Finally, many of the existing texts tend to avoid most of the controversies, contradicting views, and unresolved questions (in turbulence, there are more of them than "solutions") attempting to "smooth the angles" as much as possible, in ways that are, to my view, both inappropriate and misleading. Consequently, some interpretations/views expressed in this book may appear to some as flagrant/egregious. My apologies.

As before, the emphasis on the fundamental issues is a consequence of the view that without corresponding progress in the fundamental aspects, there is little chance for progress in any applications. More generally, there is a desperate need for the physical fundamentals of a great variety of processes in nature and technology in which turbulence plays a central role. For this reason, the book is of interest for the widest audience as turbulence is omnipresent indeed throughout natural sciences and technology.

Tel Aviv, Israel Arkady Tsinober
July 2018

Preface to the First Edition

One of my hopes is that this small book will aid the readers, especially the young ones, to develop their own judgment on the huge literature in the field and to make some distinction between the "essence" and "the water".

It is also among the reasons that the main themes of this small book are about fundamental issues of paradigmatic nature. The reasons for using the term "issues of paradigmatic nature" instead of, say, just "paradigms in turbulence" is due to the question on the very existence of the latter (so far).

The main premises and the reasons/causes that it was possible to make this book short are the absence of theory based on first principles and inadequate tools to handle both the problem and the phenomenon of turbulence. Obviously, for this reason the "essence" appears implicatively rather than explicitly. This state of matters was and is continuously stressed in the community including many prominent scientists and among them by Batchelor (1962), Kolmogorov (1985), Liepmann (1979), Lumley and Yaglom (2001), von Neumann (1949), Ruelle (1990), Saffman (1978, 1991), Wiener (1938), see Appendix for essential quotations.

Indeed, the heaviest and the most ambitious armory from theoretical physics and mathematics was tried for more than fifty years, but without much success: genuine turbulence, the big T-problem, as a physical and mathematical problem remains unsolved.

It is not a trivial task to address things like paradigmatic issues (as, in fact, there are no real paradigms in fundamental research of turbulence) in a field which–in words of Liepmann is the graveyard of theories, so that one has to relay mainly on the empirical evidence. The task is not made easier in view of no consensus on what is (are) the problem(s) of turbulence, neither is there an agreement on what are/should be the aims/goals of turbulence research/theories and what would constitute its (their) solution.

The basic properties of turbulence of fundamental nature and the essential mathematical complications of the subject were only disclosed by actual experience with the physical (and numerical) counterparts of these equations (von Neumann 1949) and of the theoretical objects in question many of which still await to be found and properly defined. Today, as before, observations remain the major

exploratory tool in elucidating the properties of turbulence as a physical phenomenon. This is not to claim absence of theory(ies). On the contrary, there are plenty–many with qualitatively different and even contradictory premises–all agreeing well with some experimental data and even claiming rigor, but not necessarily for the right reasons and not based on first principles with few exceptions having no direct bearing on the Navier—Stokes equations and thereby being mostly out of scope of this book.

Paradoxically, due to the alarming overabundance and continuing major overproduction of publications of theoretical nature claiming "theory" or exploiting the term turbulence in one way or another, the above task is made easier. This is because most of such publications did not bring any real breakthrough in understanding and have not much to do with the essence of turbulence without contributing to alleviating the long-lasting and continuing paradigmatic crisis in the basic research of turbulence. There was still a need for the "Saffman's criterion" (Saffman 1978), in order to reduce the amount of references but complemented by typically giving some early and latest references. The book by the author (Tsinober 2009), contains more than one thousand references on basic issues, so that it may be of help also in the context of this small book as the former is devoted also exclusively to the basic issues.

Still in such a situation one has to make difficult choices on what one has to put on paper. It is obvious, however, that the choice has to be made out of issues of fundamental/basic nature which are based on fundamental observations/facts and closely related issues.

The book is limited by incompressible flows and consists of four parts. Part I is devoted to main aspects of what is both the nature of the phenomenon and the problem of turbulence, what equations describe turbulence adequately, and also inadequately, along with some closely related issues. Part II is about brief description of what are the origins of turbulence and in a bit more detail about the nature of both the phenomenon and the problem with the emphasis on discussion of various facets of the "undeniably statistical nature" contrasted to the "deterministic origin" of turbulence as described by purely deterministic non-integrable equations and related issues. This includes a discussion of the consequences of complex behavior of systems described by purely deterministic equations such as the necessity of change of the paradigmatic meaning of apparent randomness, stochasticity of turbulence which is roughly just the complexity due to a large number of strongly interacting degrees of freedom governed by the Navier Stokes equations. Part III is a continuation of the previous part, and covers more specific issues, but not less important. What follows comprises or are related to the second part of the major qualitative universal features of turbulent flows briefly described in Chap. 1. A distinction is also made between the issues of paradigmatic nature and those which are apparently/seemingly, i.e. pseudo-paradigmatic such as the most popular cascades. Part IV consists of an The Epilogue which is a brief reiteration of the main points

with different more general accents and reminding that all ideas/theories, etc. proposed so far did fall/failed belong to the category of misconceptions and/or ill defined concepts, see Chap. 9 in Tsinober (2009). These are followed by remarks on what next and an Appendix. Essential Quotations.

Tel Aviv, Israel Arkady Tsinober
March 2013

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Part I
Introduction—The Phenomenon and the Problem of Turbulence

Let us first understand the facts, and then we may seek for their causes. Aristotle 384–322 BC.

Remember, when discoursing about water, to induce first experience, then reason Leonardo da Vinci, 1452–1519.

We are certainly not to relinquish the evidence of experiments for the sake of dreams and vain fictions of our own devising. Isaac Newton, Preface to his Principia Mathematica, Motte translation as revised by F. Cajori, Berkeley, 1947, pp. xvii–xviii.

I soon understood that there was little hope of developing a pure, closed theory, and because of absence of such a theory the investigation must be based on hypotheses obtained on processing experimental data, Kolmogorov (1985), in notes preceding the papers on turbulence in the first volume of his selected papers; English translation, Selected works of A. N. Kolmogorov, I, ed. Tikhomirov, p. 487.

Studying nature, which is what natural sciences do, requires a flexible approach based on facts and not on dogmas. V. L. Ginzburg 2003 Autobiography.

The words of Koheleth son of David, king in Jerusalem.Vanity of vanities, said Koheleth; vanity of vanities, all is vanity. For in much wisdom is much vexation, and he who increases knowledge, increases pain. Making many books has no end, and studying much is a weariness of the flesh. Whatever your hand attains to do [as long as you are] with your strength, do; for there is neither deed nor reckoning, neither knowledge nor wisdom in the grave, where you are going. "Vanity of vanities," said Koheleth; "all is vanity". Ecclesiastes Judaica Press translation.

It is of importance to make at the outset a clear distinction between the phenomenon and the problem of turbulence—just like between the observations and the theories attempting to explain them, which are far from being synonymous. One has also to make a distinction between the nature of the phenomenon and of the problem; for example, one cannot qualify the phenomenon of turbulence just as a purely statistical object. The statistical/probabilistic nature is one among numerous attempts to put turbulence into the Procrustean bed of some specific field pretty arbitrarily and essentially just because it is more than very complex so that *we are still quite disoriented as to the relevant factors, and as to the proper analytical machinery to be used*, von Neumann (1949). Statistics is an approach in the first place, so one can hardly speak about statistical, probabilistic, etc., nature of turbulence as a phenomenon. In contrast, one can easily assign a statistical/random as contrasted to

deterministic "nature" to a theory—with the question to what extent such a theory is adequate to the phenomenon of turbulence. In other words, theory—if such exists—is the human representation of the phenomenon and it is important to make a clear distinction with the phenomenon itself. In particular, physical quantities, relations between them such as physical laws and alike are not synonymous to their statistical "images" and interpretations, especially, with all the regret of the absence of (almost) other approaches and theory based on first principles.

Indeed, unlike other complicated phenomena it is easy to observe at least some of the numerous manifestations of turbulence as a physical phenomenon, but it is extremely difficult to comprehend, interpret, understand, and explain—all this comprising the problem of turbulence at least as concerns the fundamental aspects.

The rest of this Part I is devoted to the issues concerning which equations describe turbulence adequately, and also inadequately, along with a number closely related issues.

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Chapter 1
The Phenomenon of Turbulence as Distinct from the Problem of Turbulence

Abstract The issue is about turbulence as a natural physical phenomenon as related to observations as distinct from the problem of turbulence. The dichotomic distinction between laminar and turbulent flows is problematic in several respects. First, most flows termed "turbulent" are in reality partly turbulent: some portions of the flow as turbulent and some as laminar—the coexistence of the two regimes, partly turbulent flows in one flow is a common feature with continuous transition of laminar into turbulent state via the entrainment process through the boundary between the two. Moreover, the reality is not that simple as the laminar/turbulent dichotomy as, e.g. the behavior of passive objects in flows with small Reynolds number looks as perfectly turbulent reflecting the qualitative difference between the chaotic flow properties in Eulerian and Lagrangian settings. These examples illustrate the enormous difficulties in defining what is both (i) turbulence and (ii) the turbulence problem. As concerns (i) one can only provide a description of major qualitative universal (sic) features of turbulent flows as obtained almost exclusively from observations (rather than by any theoretical deliberations) which form most important part of the "essence" of turbulence. This is because these mostly widely known **qualitative** features of all turbulent flows are essentially the same, i.e., it is meaningful to speak about **qualitative universality** of turbulent flows. It has to be stressed that the term "phenomenon of turbulence" as used above is mostly associated with the observational aspects, which in turbulence play far more important role due the unsatisfactory state of "theory": there seems to exist no such a thing based on first principles. Hence it is vital to put the emphasis on the physical aspects based in the first place on observations as a basis aiding to form the notion of what turbulence and its essince are.

The issue is about turbulence as a natural physical phenomenon as related to observations both passive and active.

A typical starting statement on turbulence is as follows:

It is known that all flows of liquids and gases may be divided into two sharply different types; the quiet smooth flows known as "laminar" flows, and their opposite, "turbulent" flows in which the velocity, pressure, temperature and other fluid mechanical quantities fluctuate in a disordered manner with extremely sharp and irregular space- and time-variations. Monin and Yaglom (1971, p. 1).

However, as common as it is, this division is problematic in several respects. First, looking at Fig. 1.1 one can easily see that the flow is only partly turbulent (PTF).

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Fig. 1.1 Side view of a turbulent boundary layer visualized by smoke traces, courtesy of Professor H. Nagib

Most flows termed "turbulent" are of this kind: boundary layers, all free shear turbulent flows (jets, plumes, wakes, mixing layers), penetrative convection in the atmosphere and in the ocean, gravity currents, avalanches and other phenomena at the boundary between single phase fluid and fluid loaded by a sediment (which includes resuspension), clear air turbulence, and many others (e.g., combustion). Transitional flows consisting as a rule of turbulent regions growing in a laminar environment (such as turbulent spots) are also partly-turbulent flows.

However, it is not conceptually correct to say that a turbulent flow (not a partly turbulent flow) is completely laminar locally, e.g. at small scales with locally small Reynolds number, as in the examples belonging to a span of time period of 65 years, Batchelor (1947), Southerland et al. (1994), Hamlington et al. (2012) and references therein. Another example is a problematic statement that "the large scale circulation of the atmosphere is non-turbulent" though "the smaller scale flow is certainly turbulent" Cullen (2006).

In many such flows one observes some portions of the flow as turbulent and some as laminar - the coexistence of the two regimes in one flow is a common feature with continuous transition of laminar into turbulent state via the entrainment process through the boundary between the two. All these flows are also neither homogeneous nor isotropic with some other "N's" , which has implications of using an interpretation the data for comparisons, "validation" of theoretical stuff based on homogeneous, isotropic and/or flows with periodic BC's.

Both PTF's and "purely" turbulent flows exhibiting a high degree of apparent randomness and disorder often reveal the presence of what is termed frequently as ordered embedded flow structures which are highly dimensional objects.

Another worthy mentioning is a class of very intense objects such as rotating storms in the atmosphere again existing in and coexisting with "ordinary"

Fig. 1.2 May 28, 2004 Highway 12 Nebraska Supercell. http://www.spc.ncep.noaa.gov/exper/archive/event.php?date=20040528

flows/regimes which, see Fig. 1.2. The intense objects belonging to the wide category of extreme events are generally strongly turbulent whereas the ordinary environment may be (or not) even close to a laminar state.

Moreover, the reality is not that simple as the laminar/turbulent dichotomy as illustrated by the example in Fig. 1.3 in which the flow *looks* perfectly turbulent. In fact, this is a flow at Reynolds number of order unity with an essentially linear dependence between the flow rate and the pressure drop and other dynamic attributes of laminar flows at low Reynolds numbers. There is a great variety of flows with Reynolds number effectively close to zero, i.e. laminar in Euler setting (E-laminar) but which are *chaotic* in Lagrangian setting (L-turbulent), see below Sect. 3.4 references in Tsinober (2009).

This is because what looks to be turbulent by employing visualizations such as via observing *passive* objects, e.g. using some dye, may have nothing to do with the behavior of dynamical variables (such as velocity and vorticity) and even with the turbulent nature of the flow in question, and reflecting the chaotic nature of the behavior of passive objects in a purely laminar flow in an Eulerian setting called also Lagrangian/kinematic chaos, or "Lagrangian turbulence" as distinct from the chaos in Eulerian setting. Another example is about the "streaks" in a turbulent flow in a plane channel, Baig and Chernyshenko (2005). Although the velocity structure of the flow is the same, the scalar streak spacing varies by an order of magnitude depending on the mean profile of the scalar concentration. Moreover, passive-scalar streaks were observed even in an artificial "structureless" flow field.

Fig. 1.3 Mixing in PPM – partitioned-pipe mixer at very low Reynolds number. **a** Schematic of the PPM, **b** is a close-up of the upper part of **c**. Reproduced from Kush and Ottino (1992). For other examples see references in Tsinober (2009). See the text above: "the behavior of passive objects in flows with small Reynolds number looks as perfectly turbulent reflecting the qualitative difference between the chaotic flow properties in Eulerian and Lagrangian settings"

The above examples illustrate the enormous difficulties (and impossibility, so far) in defining what is both *(i)* turbulence/turbulent motion[1] and *(ii)* the turbulence

[1]Though there are many attempts to do so. In contrast to mathematical *theories* in which the definition of the main object of the theory precedes the results, in turbulence (as in any field of physics) even if such a definition would be possible it is likely to come after the basic mechanisms of turbulence as a physical phenomenon are well understood.

In any case there is considerable 'turbulence' in the attempts to define what is turbulence indicating that such attempts at the present stage are futile, see the list of examples of such attempts in Appendix A. What is turbulence? in Tsinober (2009).

In a mathematical theory the definition of the main object of the theory precedes the results. In physics, especially in new fields, it is vice versa. Usually it happens when one studies a new phenomenon and only at a later stage, after understanding it sufficiently, classifies it, finds its proper place in the existing theories and eventually the most reasonable definition is chosen. Though

problem. As concerns (i) one can only provide so far a description of major qualitative universal (sic) features of turbulent flows as obtained almost exclusively from observations rather than by any theoretical deliberations, see the quotation by von Neumann (1949) in Chap. 11, Appendix. Essential quotations.

1.1 Major Qualitative Universal Features of Turbulent Flows

The list below contains a brief description of the major qualitative universal features of turbulent flow. Important quantitative and more specific features will be discussed in more detail in the sequel. These altogether form the most important part of the "essence" of turbulence.

• Intrinsic spatio-temporal apparent randomness. Turbulence is definitely chaos. However, the opposite, generally, is not true: many chaotic flow regimes are not necessarily turbulent, e.g. Lagrangian/kinematic chaos or 'Lagrangian turbulence' (L-turbulent), but laminar in Euler setting flows (E-laminar). One of the most important aspects is that the *apparent* stochastic/random nature of turbulent flows is its intrinsic property,[2] i.e. its apparent self-stochastization or self- randomization. We emphasize the term *apparent* randomness/stochasticity: after all turbulent flows are known (so far) to be governed by the NSE which are purely deterministic, so that there is quite a bit of a problem with the random/deterministic dichotomy either just like with the use of the term randomness and its synonyms.

• Extremely wide range of strongly and non-locally interacting degrees of freedom which are not necessarily synonymous to the most popular modes of some decomposition and alike. Turbulent flows are very large systems. In atmospheric flows, relevant scales range from hundreds of kms to parts of a mm, i.e., there exist $\sim 10^{29}$ excited degrees of freedom, many of which are strongly interacting. Hence the extreme complexity of turbulence enforcing the statistical description of turbulent flows. We emphasize again that statistical description is not synonymous to statistical theorization not to mention statistical "nature".

• Loss of predictability, which is one of the attributes of the chaotic nature of turbulence. Two initially nearly (but not precisely) identical turbulent flows become unrecognizably different on the time scale of dynamic interest. However, generally, different realizations of the same turbulent flow have the same statistical properties.

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turbulence in not a new field the above is so much true of turbulence – there is no theory so far. This time has not yet come, and it may not come soon if at all – to quote A.N. Kolmogorov (1985): *I soon understood that there was little hope of developing a pure, closed theory, and because of absence of such a theory the investigation must be based on hypotheses obtained on processing experimental data*, see Tikhomirov (1991, p. 487).

[2]Cf. with the case of random boundary/initial conditions and random noise on the RHS of NSE. Of special interest for this comparison are systems/equations which do not exhibit any quasi-random behavior without external stochastic excitation. For example, Burgers and Korteveg de Vries equations or just the NSE at small Reynolds numbers.

In this sense the statistical properties (not only some means, but almost all statistical properties) of turbulent flows are insensitive to disturbances – turbulent flows are statistically stable as possessing statistically stable properties.

• Turbulent flows are three-dimensional, highly dissipative and thereby time irreversible, and rotational, i.e., carry lots of strain and vorticity. Thus the field of velocity derivatives is of special importance, e.g. turbulence cannot be considered as just a purely inertial phenomenon. One of the key physical processes is the predominant *self-amplification* of the velocity derivatives (i.e. independence of "mediators" such as the mean flow, etc.), to stress both strain/dissipation and vorticity as almost equal partners. "Almost" is because it is the strain production which is responsible for production of both vorticity and strain contrary to common view about the role vorticity amplification,[3] These attributes are among the most specific and important as concerns fluid dynamic turbulence **.** On the paradigmatic level it is the *nonlinearity* that is responsible for these most basic key properties of turbulence as essentially rotational and strongly dissipative phenomenon. It is noteworthy that the predominant production of velocity derivatives is irreversible. It is the strain production that is mostly responsible for the dissipation "anomaly" of 3-D turbulence at large Reynolds numbers, the singular nature of the inviscid limit of NSE and irrelevance of Euler equations to turbulence. The property of self- amplification of velocity derivatives underlies the origin of small scales in 3-D turbulence rather than the fictitious "cascade".

There is no consensus whether two-dimensional chaotic flows even with many degrees of freedom should be qualified as turbulence. The main reason is that such flows lack the mechanism of vorticity and strain self amplification and absence of the dissipation "anomaly".

• Strongly diffusive (random waves are not). Turbulent flows exhibit strongly enhanced transport processes of momentum, energy, passive objects (scalars, e.g., heat, salt, moisture, particles; vectors, e.g., material lines, gradients of passive scalars, magnetic field). It should be emphasized that in respect with passive objects this property is true of a much broader class of systems. Namely, any random velocity field and even laminar flows in Euler setting, which are Lagrangian chaotic, exhibit an enhanced transport of passive objects.

• Strongly nonlinear, non-integrable, nonlocal, non-Gaussian with some more N's, see Chap. 7.

These mostly widely known *qualitative* features/processes of all turbulent flows are essentially the same, i.e., it is meaningful to speak about *qualitative universality* and the *essence* of turbulent flows prior to universality of numbers. We discuss this issue in the Chap. 8.

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[3]There are two aspects concerning the term of *self-amplification.* The first is about the self-amplification of the field of velocity derivatives (i.e. $A_{ij} = \partial u_i/\partial x_j \equiv s_{ij} + \frac{1}{2}\varepsilon_{ijk}\omega_k$) as largely independent on the field of velocity. The second concerns the self amplification of the field of strain as mostly produced via the term $-s_{ij}s_{jk}s_{ki}$ and the vorticity amplification via the interaction with strain $\omega_i\omega_j s_{ij}$. Hence here the term non-local amplification of vorticity as compared with the self amplification of strain.

It must be stressed that the term "phenomenon of turbulence" as used above is mostly associated with the observational aspects, which in turbulence play a far more important role due the unsatisfactory state of "theory": there seems to exist no such a thing based on first principles. It is vital to put the emphasis on the physical aspects based in the first place on observations with the distinction between "active" and "passive". Otherwise, such an endeavor becomes problematic due to the highly dimensional nature of the phenomenon: one can do a continuum of things leading nowhere or at best to very small useful output. In other words, we stress that the observational aspect is extremely not trivial in such a highly-dimensional system as turbulence. It is intimately related to the skill/art of asking the right and correctly-posed questions: one must have at least some idea about what to look at and for what reason, i.e. one must have some idea about what is (are) the problem(s) of turbulence.[4] The latter brings us to the issue of the problem of turbulence.

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[4]Thus the "experimentalists" should possess a serious theoretical background as the role of experiments in turbulence goes far beyond the view of those who think of experimentalists as just a superior kind of professional fixers knowing how to turn nuts and bolts into a confirmation of other people's 'theories'. The issue is more than serious in view of the general theoretical failure, i.e. absence of theory based on first principles. There is a number of issues of special concern about the relation of what is called theory in turbulence research and observations/experiments, in particular, regarding the proper use and interpretation of the factual information as concerns fundamental aspects. We return to these issues in different context in the sequel.

Chapter 2
The Problem of Turbulence as Distinct from the Phenomenon of Turbulence

Abstract The issue involves a set of questions concerning theory(ies) of turbulence and questions such as what is physics and what is mathematics of turbulence and what are the physical/mathematical problems of turbulence. Though there is no acceptable definition of (what is) turbulence as a physical phenomenon, the relative "easiness" of observing its diverse manifestations leaves less problems than in the above. There is no consensus on what is the problem(s) of turbulence are and what would constitute their solution. There are even doubts about the very existence of the problem or any essence in it, so that the failure so far of theoretical efforts may be blamed on the problem itself. Neither is there agreement on what constitutes understanding. There is also no consensus on what the main difficulties are and why turbulence is so impossibly difficult: almost every aspect of turbulence is controversial, which by itself is one of the greatest difficulties. As concerns the basic aspects of the problem there is far more to say about the difficulties rather than the achievements. The problem was recognized by Neumann and Kolmogorov among others. The most acute difficulty is of basic and conceptual nature and concerns the physics of turbulence and the lack of knowledge about the basic physical processes of turbulence, its generation and origin, and poor understanding of the very few processes which are already known. One of the key physical processes is the predominant production of the velocity derivatives, to stress both strain/dissipation and vorticity as almost equal partners. The "almost" is important because it is the strain production is responsible for production of both contrary to the common view about amplification of vorticity. Moreover these are not just production - they are *self-amplification*. These attributes are, probably, the most specific and important as concerns fluid dynamic turbulence. Another set of much neglected issues is about the nonlocal properties of turbulence and related questions such as the ill-posedness of the concepts/pseudo-paradigms of inertial range and cascade, and the role of large scales and viscosity/dissipation; scale invariance, symmetries and universality in turbulence; origins of intermittency and the so-called 'anomalous' scaling in the inertial range just to mention a few.

This state of matters to a large extent can be qualified as a long-lasting and continuing paradigmatic crisis.

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2.1 Is There a Consistent Theory of Turbulence Based on First Principles?

The issue involves a set of questions concerning theory(ies) of turbulence and questions such as what are the physics and mathematics of turbulence and what is the physical/mathematical problem of turbulence; what equations describe turbulence adequately. The very question is a problem - how one can decide whether, say, NSE are adequate or what are the equations of turbulent motion, see Chap. 3. An important aspect is that physical quantities, relationships between them such as the physical laws and the like are not synonymous to their statistical "images" and interpretations, especially in absense of (almost) other approaches.

Though there is no acceptable definition of (what is) turbulence as a physical phenomenon, the relative ease of observing its diverse manifestations leaves less problems than in the question what is/comprises (are) the scientific, mathematical and physical, problem(s) of turbulence. There is no consensus on what is (are) the problem(s) of turbulence and what would constitute its (their) solution.[1] Neither is there agreement on what constitutes understanding. Also just like no sophisticated experiment, laboratory or DNS,[2] by itself does not bring understanding, neither does modelling of whatever sophistication. This can be only brought about only by a genuine theory, which seems to be not in existence so far. Though there exists a set of deterministic differential equations, the Navier–Stokes equations (NSE), probably containing (almost) all of turbulence, most of our knowledge about turbulence comes from observations and experiments, laboratory, field and later numerical, which is

[1]This is definitely not, e.g. the proof of the existence of the smooth solution of the NSE on a three-dimensional domain for all time or anything similar.

There are even doubts about the very existence of the problem or any essence in it, so that the failure so far of theoretical efforts may be blamed on the problem itself:

I hope that my talk will help to clarify a number of the problems and I think that some of the models of chaotic phenomena that I will discuss are illuminating and suggestive, but it is far too early to claim that any of them gives the essence of the phenomenons of turbulence—if indeed it is a single phenomenon and has an ***essence*** Martin (1976).

…we should not altogether neglect the possibility that there is no such thing as 'turbulence'. That is to say, it is not meaningful to talk of the properties of a turbulent flow independently of the physical situation in which it arises. In searching for a theory of turbulence, perhaps we are looking for a ***chimera*** Saffman (1978).

However, today there is no doubt about the existence of a set of qualitatively universal properties comprising the essence of any three-dimensional turbulent flow. This is the result of dealing in the first place with the phenomenon itself, rather than with "theories", though excessively and quite ineffectively.

For more on the disparity in opinions on what comprises the scientific mathematical and physical problem(s) of turbulence, see Chap. 11 Appendix. Essential quotations.

[2]*Progress in numerical calculation brings not only great good but also awkward questions about the role of the human mind… The problem of formulating rules and extracting ideas from vast masses of computational or experimental results remains a matter for our brains, our minds.* Zeldovich (1979).

There are essential differences between physical and numerical experiments. For example, if underresolved the former still provide correct information which is a problem with the latter.

unfortunate as theory is supposed to guide and gives meaning to observations.[3] This was understood long ago by many outstanding people, Kolmogorov (1985), von Neumann (1949), Wiener (1938) and many other, see references and quotations for the preface in Chap. 11 Appendix. Essential quotations and Tsinober (2009).

Indeed, the basic properties of turbulence of fundamental nature such as the major features listed above among others *were only disclosed by actual experience with the physical* (and numerical) *counterparts* of the theoretical objects in question many of which still await to be found and properly defined. Today, as before, the experiment (physical and numerical) remains the major exploratory tool in elucidating the properties of turbulence as a physical phenomenon. This is not to claim absence of theory(ies). On the contrary, there are plenty – many with qualitatively different and even contradictory premises/assumptions – all agreeing well with *some* experimental data, but not based on first principles, i.e. with few exceptions have no direct bearing on the Navier–Stokes equations and thereby being mostly out of scope of this book as lacking the main attribute of a true theory:

The problem of turbulence is not just to find more accurate formulae for various physical quantities associated with a turbulent fluid, but also to obtain a conceptually satisfactory theory based on first principles…In spite of satisfaction which one may have in writing rigorous inequalities originating from nontrivial linear theory, it must be said that the great difficulty which remains is to understand the nonlinear objects of turbulence, Ruelle (1990).

But as mentioned 'understanding' is a vague concept, just like it is far from being clear what are 'the nonlinear objects of turbulence', though some would jump on 'coherent structures' which along with some other objects called "structures" still are pretty elusive and share quite a bit in common with the Emperors's New Clothes. Unfortunately, this is true of several other basic aspects of turbulent research.

In general terms and in absence of true theory the physical/mathematical problem is to identify, understand/interpret and explain the major basic/fundamental physical mechanisms that, e.g. result in the major qualitative universal properties of turbulent flows as described above along with some other specific fundamental issues. This includes the identification and study of the significant nonlinear objects of turbulence, i.e. what objects are right to look at and requires non-trivial efforts from theoretical physicists and mathematicians. Reiterating, so far, neither theoreticians nor the mathematicians were able to achieve any of these goals.

Indeed, though the heaviest and the most ambitious armory from theoretical physics and mathematics was tried for more than fifty years, as concerns the basic/fundamental aspects, the progress is far less than modest and the state of matters

[3]This is the main reason that this book is biased experimentally and this is why the importance of experimental research in turbulence goes far beyond the view of those who think of an experimentalist as a superior kind of professional fixer, knowing how to turn nuts and bolts into a confirmation of their theories. From the basic point of view there is almost nothing to be confirmed so far. In absense of true theory and in view of the high dimensional nature of the problem the issue of confirmation/validation of 'theories' in turbulence is far more serious than just checking the 'validity' of some formulae or any other theoretical stuff via comparison with limited experimental evidence, especially because turbulence is a highly dimensional problem.

is not much different from that described by Batchelor (1962), Liepmann (1979), von Neumann (1949), Saffmann (1968) Saffman (1991) along with many other famous scientists, see Chap. 11 Appendix. Essential quotations and Tsinober (2009).

Along with the wide recognition that turbulence is formidably difficult,[4] just like there is no consensus on what the problem(s) of turbulence are and what would constitute their solution,[5] there is no consensus on what are the main difficulties and why turbulence is so impossibly difficult: almost every aspect of turbulence is controversial, which by itself is one of the greatest difficulties. As concerns the basic aspects of the problem there is far more to say about the difficulties rather than achievements. The difficulties mentioned by Neumann and Kolmogorov among others comprise only a part of the whole set of difficulties. Naturally, this is one of the themes throughout this small book along with the emphasis on the key issues of turbulence as a physical phenomenon.

2.2 On the Physics and Mathematics of Turbulence

One of the key words is - as expected - difficulties, so that a considerable place is taken here discussing this issue as it is throughout the sequel.

The most acute difficulty is of basic and conceptual nature and concerns the physics of turbulence and lack of knowledge about the basic physical processes of turbulence, its generation and origin, and poor understanding of the very few processes which are already known. For example, the underlying mechanisms of predominant vortex stretching, which is why in turbulent flows vorticity is stretched more than compressed, are poorly understood and essentially not known. Until recently a not less important concomitant process of strain production was mostly neglected by the community. It is precisely this process, rather than vortex stretching, that is directly

[4]The mood was not always that sceptical. Here are two examples from outstanding people.

Hopf (1952): *My attempts at finding the "relevant" solutions of the α-equation have been unsuccessful even in the simplest case of boundary-free flow, but I believe that the mathematical difficulties of the problem arise from the fact that it is unprecedented and not from any intrinsic complexity.*

Kraichnan (1961): *In order to keep the formalism as simple as possible, we shall, work here with the one-dimensional scalar analog to the Navier–Stokes equation proposed by Burgers. In the method to be presented here, the true problem is replaced by models that lead, without approximation, to closed equations for correlation functions and averaged Green's functions . . . The treatment of Navier Stokes equation for an incompressible fluid, which we shall discuss briefly, does not differ in essentials.*

The temptations with such and similar analogies are still going on, e.g. Eyink and Frisch (2011) and references therein, and Chap. 9 Analogies, Misconceptions and ill defined Concepts in Tsinober (2009).

[5]In spite that there is no definition of the problem there are proposals as to how solve it, e.g. *the solution to the turbulence problem(s) must eventually be in the form of some reduced order modelling valid either universally or in different ways in different universality classes so that industry can use it to explore new unchartered flow concepts for the invention of new flow technology solutions.* This hardly can be considered as belonging to the realm of fundamental research.

responsible for the enhanced dissipation of 3-D turbulent flows and even feeds the process of predominant vortex stretching as well. There are qualitative differences between the two. The enstrophy production is a nonlocal process as interaction with strain $\omega_i\omega_j s_{ij}$, whereas the strain production is mostly a local process, i.e. *self-amplification* $-s_{ij}s_{jk}s_{ki}$. It is noteworthy that this "self" is conditional in the sense that the field of strain is efficient in the above two missions only with the aid of vorticity, i.e. only if the flow is rotational. One of the most important aspects is that the strain self amplification process - being an inherent property of turbulence as a dissipative phenomenon at any whatever large Reynoldes number - is responsible for the dissipation "anomaly" at large Re, the singular nature of the inviscid limit of NSE and irrelevance of Euler equations to turbulence. Along with vorticity production the strain self amplification process underlies the qualitative universality of the small scales of turbulent flows. We would like to emphasize that there is a broader meaning and another use of the term "self-amplification" for the production process of the whole the field of velocity derivatives, i.e. both strain and vorticity self amplification as mostly independent on the field of velocity. A noteworthy mathematical aspect is that the quantities $-s_{ij}s_{jk}s_{ki}$, $\omega_i\omega_j s_{ij}$ are just the sort of quantities that are crucially involved when one is trying to find rigorous upper bounds to various norms when studying the well-posedness (in the large) of the Euler and Navier–Stokes problems. importance, see Chap. 7.

To reiterate: the quantities $-s_{ij}s_{jk}s_{ki}$, $\omega_i\omega_j s_{ij}$ are indeed outstanding as reflecting the processes responsible for the dissipative and rotational nature of turbulence along with its non-Gaussianty and time irreversibility.[6]

Another set of much neglected issues concerns the nonlocal properties of turbulence and related questions such as the ill posedness of the concepts/psuedo-paradigms of inertial range and cascade, and the role of large scales and viscosity/dissipation; scale invariance, symmetries and universality in turbulence; origins of intermittency and the so-called 'anomalous scaling in the inertial range just to mention some. An important issue is that - with all the regret of absense of (almost) other approaches - a clear distinction shoud be made between physical quantities, relations between them such as physical laws and alike with their statistical "images" and interpretations, especially, the latter being far from synonymous to the former.

This state of matters to a large extent can be qualified as a long-lasting and continuing paradigmatic crisis. Indeed, the basic properties of turbulence of fundamental nature, such as the major features listed above among others, were only disclosed by actual experience with the physical and numerical counterparts of the theoretical objects in question, von Neumann (1949) many of which still await to be found and properly defined. Today, as before, the observations and experiments, physical and numerical, remain the major exploratory tool in elucidating the properties of turbulence as a physical phenomenon.

[6]The story, of course does not end with the above as the problem of turbulence is likely to be endless (see e.g. Kolmogorov 1985). For example, one will need to address the evolution equations for $s_{ij}s_{jk}s_{ki}$ and $\omega_i\omega_j s_{ij}$ containing important nonlocal terms $\omega_i\omega_j \frac{\partial^2 p}{\partial x_i \partial x_j}$ and $s_{ik}s_{kj}\frac{\partial^2 p}{\partial x_i \partial x_j}$, where $\frac{\partial^2 p}{\partial x_i \partial x_j}$ is the pressure hessian.

Another kind/set of difficulties is mostly of a formal/technical nature and concerns the lack of adequate tools to handle the problem(s) - both theoretical/mathematical and experimental, especially at large Reynolds numbers not accessible via DNS.

Whatever the tools applied the emphasis in the sequel is amongst other things on the difficulties of general nature in the contexts of conceptual and fundamental nature.

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Chapter 3
What Equations Describe Turbulence Adequately?

Abstract As for today the standpoint of continuum mechanics reflected by the Navier–Stokes equations as a coarse graining over the molecular effects is considered as adequate and *Perhaps the biggest fallacy about turbulence is that it can be reliably described (statistically) by a system of equations which is far easier to solve than the full time-dependent three-dimensional Navier–Stokes equations* (Bradshaw, Exp Fluids 16:203–216, 1994).

One of the hot issues is on large Re, zero-viscosity limit and relevance of Euler equations with the natural but futile tendency to "circumvent" it in a variety of ways (as briefly described in this chapter) the singular nature of the zero-viscosity limit.

A variety of empirical observations show clearly that turbulence is not a slightly viscous/dissipative phenomenon and that inertial and dissipative effects are of the same order at whatever Reynolds number and, moreover, are not limited each by its own range of "scales" due to inherent nonlocal nature of turbulent flows with a multitude of the problems/ambiguities defining the term "scales" and alike. So one would prefer in the sequel to use the term "degrees of freedom" - as we do - wherever possible as a better-defined concept, though it involves all the problems in using decompositions as an essentially linear procedure. This is manifested not only by the finite energy dissipation rate presumably at any however large Reynolds numbers (the dissipation anomaly), - but among other evidence as below in Chaps. 7 and 8 and Tsinober (2009). For example, the so called Tennekes and Lumley balance between the enstrophy production (and a similar relation for strain production) and its viscous destruction/'dissipation', so that vorticity is not frozen in the flow field – again at any, however, large Reynolds numbers.

Special mentioning deserves the issue of utility of various decompositions. Being useful in many respects in the analysis of both flow states and processes decompositions as borrowed from methods used in *linear* problems have a rich potential (i) to be misused leading to misconceptions and ambiguities and introducing non-trivial artifacts and spurious effects due to properties of the decompositions not characteristic of genuine turbulence and not found in physical space and (ii) become an object of study by themselves threby keeping many people busy to a large extent with the properties of decompositions themselves rather than with genuine physics of turbulence.

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The above is among the reasons why decompositions became to some extent less productive/useful and even obscuring the physics of turbulence.

An important issue with serious consequences concerns the formal equivalence of Eulerian and Lagrangian representations as describing the same physical object. However, the latter appear to be 'more chaotic' with the multitude of E-laminar flows having no counterpart to the corresponding L-turbulent statistics of the same fluid flow.

One more problem is with use of functional analysis as "suspect" to belong to excessive generalizations or 'overgeneralizations' dealing with generally singular objects of unknown relevance to the physical phenomenon of turbulence. This tendency for overgeneralization in science has already been criticized already by Poincare at the beginning of the 20th century 1902, see Poincare (1952a, b).

The title above contains already a statement that some equations do describe turbulence adequately which is definitely a luck. The existing evidence and the consensus is that these are the Navier–Stokes equations. This short statement has quite a bit behind it. At this stage the most important is that the observed properties of turbulence statistical and/or not, for a given flow geometry and a given Reynolds number, are the same in air as in water and many other Newtonian fluids very different at a molecular level and share very little except the Navier-Stokes equations.

3.1 Navier–Stokes Equations

> *Perhaps the biggest fallacy about turbulence is that it can be reliably described (statistically) by a system of equations which is far easier to solve than the full time-dependent three-dimensional Navier–Stokes equations,* Bradshaw (1994).

Though the NSE have a limited kinetic foundation, they are commonly believed to be adequate in the sense that their solutions correspond to real fluid flows and, indeed, there exists large empirical evidence that NSE are adequate, at least, at all accessible Reynolds numbers. Theoretically this is not obvious, since the NSE are a gradient expansion. So in principle, higher order terms may become dominant in regions with large velocity gradients, but so far there no evidence for this.[1]

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[1] But see Goldstein (1972) concerning the success of NSE equations for the laminar flows of viscous fluids, but even in this case, *it is, in fact, surprising that the assumption of linearity in the relation between* τ_{ij} *and* s_{ij} *as usually employed in continuum theory,...works as well, and over as large a range, as it does. Unless we are prepared simply to accept this gratefully, without further curiosity, it seems clear that a deeper explanation must be sought.*

Also, Ladyzhenskaya (1975) and McComb (1990), Friedlander and Pavlović (2004) on alternatives to NSE, and Tsinober (1993, 2009) and references therein. In any case it is safe to keep in mind that no equations are Nature.

Note aslo the statement by Ladyzhenskaya (1969): *... it is hardly possible to explain the transition from laminar to turbulent flows within the framework of the classical Navier-Stokes theory.*
Finally, since Leray (1934) one was not sure about the (theoretical, but not observational) possibility

Thus as for today the standpoint of continuum mechanics reflected by the NSE as a coarse graining over the molecular effects is considered as adequate. One of the basic reasons for the success of the NSE is the existence of a gap between the scales of molecular motions and the scales of the smallest relevant scales in fluid flows including turbulence.

Thus at least formally, as concerns 'theory' the first step is to solve the Navier–Stokes equations subject to initial and boundary conditions and forcing. At present, it is possible to obtain fully resolved solutions at modest Reynolds numbers via direct numerical simulations of the Navier–Stokes equations. Such solutions are in some sense of the same nature as the data available from observations, but both are only necessary for the beginning of the process called understanding. However, strong arguments were given that looking at the behavior of a particular solution may not necessarily contribute much to the understanding of the basic physics of turbulent flows, so that *nothing less than a thorough understanding of the* [global behavior of the] *system of all their* [NSE] *solutions would seem to be adequate to elucidate the phenomenon of turbulence... There is probably no such thing as a most favored or most relevant, turbulent solution. Instead, the turbulent solutions represent an ensemble of statistical properties, which they share, and which alone constitute the essential and physically reproducible traits of turbulence*, von Neumann (1949).[2] That is in order to understand the dynamics, or the main characteristics of the dynamics, it is necessary to understand a significant portion of the phase flow. However, at present (if ever) it is impossible due to very high dimension and complex behavior of turbulent flows and structure of the underlying attractors (assumed to exist): *one may never be able to realistically determine the fine-scale structure and dynamic details of attractors of even moderate dimension... The theoretical tools that characterize attractors of moderate or large dimensions in terms of the modest amounts of information gleaned from trajectories* [i.e., particular solutions]... *do not exist...they are more likely to be probabilistic than geometric in nature,* Guckenheimer (1986). Note the accent on "probabilistic tools" rather than "nature" in contrast to, e.g. Foiaş et al. (2001).

In other words, one has so far to resort to statistical methods of data processing and analysis (not synonymous to statistical theorization) as the necessity imposed by extremely intricate, complex, effectively/apparently and seemingly random behavior along with a huge number of strongly and nonlocally interacting degrees of freedom.

With all this we will take the position that the basic block of the operational truth is provided by a solution of a "master" problem such as an initial, boundary conditions problem with deterministic (rather than stochastic/random) forcing for the NSE without stratification, rotation, combustion, additives, etc. if not stated otherwise, assuming that such a problem is well posed, which seems to be the case for the

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that turbulence is a manifestation of breakdown of the Navier–Stokes equations. Today it is clear that these doubts are definitely not correct.

[2]However, a far less-trivial issue is ergodicity, i.e. if the flow is statistically stationary, the common practice is to use one long enough realization, i.e. it may suffice to have such a realization at least for those who believe that statistics is enough. The basis of this is the ergodicity *hypothesis*, see Chap. 6.

NSE. Though the latter is still not rigorously proven the evidence and consensus is that NSE most probably contain all of incompressible turbulence. We return to the issue of the dichotomy deterministic - statistical and related matters in Chap. 5.

It is noteworthy that the conclusion that NSE are useful as only an experimental tool would be incorrect. It is true that there is little substantial theoretical use of NSE in turbulence, since there is almost no way so far to use them explicitly in theoretical approaches, e.g., by solving them by 'hand'. However, there are several ways to do this implicitly, i.e. by indirect use of NSE and their consequences. For example, "simply looking" at the NSE and their consequences themselves enables us to recognize the dynamically important quantities and physical processes involved, i.e. NSE and their consequences aid us to recognize what quantities and relations should be studied especially together with some empirical knowledge/evidence and reasonable theoretical (!) background. So far this can be done mostly using experiments (physical and numerical), but this kind of 'guiding' should also be useful theoretically. The most elegant example is a set of theoretical results on the a priori upper bounds of long time averages of dissipation and global transport of mass, momentum and heat, see Doering (2009) and references therein. In this sense the NSE tell us more than any estimates of the dimensions of attractors and similar things. Two simple examples illustrate the possible usefulness of "direct looking" at the NSE. The first example concerns the field of velocity derivatives: just looking at the equations for strain and vorticity one can recognize almost immediately the leading role of strain in both self-amplification (i.e. being responsible for the enhanced dissipation in turbulent flows) and in production of vorticity, in contrast to the common view on the role of vorticity, see Sect. 7.2.2. The second example is about "simply looking" at the NSE in the Lagrangian setting (LNSE). With all the intractability of the LNSE it is straightforward to recognize the qualitative difference of the statistical properties between the velocity fields in the Eulerian (ENSE) and the Lagrangian (LNSE) settings. For example, almost all laminar flows in the Eulerian setting are not chalotic, whereas these same flows in the Lagrangian setting are chaotic, see below Sect. 3.4.

There are serious/problematic issues about adequate boundary and initial conditions, the relevance of the Euler equation and other equations "replacing" NSE such as employing hyperviscosity, Galerkin truncations and decimated approaches among others, see Sect. 10.3 Turbulence versus mathematics and vice versa in Tsinober (2009).

3.1.1 On Boundary Conditions, Forcing, etc.

Using the NSE for simulating real turbulent flows is quite a bit more complicated due to problems with the boundary conditions and other factors. Amongst others these include inflow and outflow conditions, and problems with the so called and popular periodic boundary conditions believed to be appropriate in DNS computations to mimic (but seems that not more than that!) some artificial/idealistic flows such as statistically homogeneous turbulent flows or flows with homogeneous direction(s).

An example of a problem of conceptual nature is represented by good agreement between (some) results of typical DNS computations of NSE of turbulent flows (e.g., in a circular pipe and a plane channel, in a cubic box, etc.) with periodic boundary conditions with the results obtained for real flows in laboratory, in which the boundary conditions have nothing to do with periodicity and pretty frequently even not well known, and in which the inlow/outflow conditions were totally different from those used in DNS. There are problems with hyperviscosity and other "replacements" of NSE.

Other examples concern the role "remaining" for boundary conditions at zero viscosity limit, relevance of the Euler equation, etc. and especially the nonlocal effects, see e.g. below Sect. 7.3. All of them seem to spoil considerably the hunt after some fantoms as universality in a variety of contexts such as criticisms of Kolmogorov, "nonequilibrium turbulence", etc.

3.2 Large Re, Zero Viscosity Limit and Relevance of Euler Equations

One of the 'natural' conjectures in the mathematical community was that turbulent flows may be described asymptotically correctly by some sort of specially selected weak, i.e. distributional solutions of the Euler equations, though these singular solutions have little to do with real physics at any however large Reynolds number. This was prompted mainly by two inputs. The first is the hypothesis on the existence at high Reynolds numbers of the so-called inertial range in which viscosity/dissipation play no role, Kolmogorov (1941a), the second - is the conjecture by Onsager (1949) that not smooth enough solutions of the Euler equations do not conserve energy, i.e. in some sense are "dissipative" and thereby time irreversible but physically not meaningful as employing not smooth solutions. The issue goes back to Taylor (1917). For later references see Shnirelman (2003) and Sect. 10.3 Turbulence versus mathematics and vice versa in Tsinober (2009).

It is the right place to remind that dissipation (energy input) or drag only are not sufficient to define the properties of a turbulent flow. For example, Bevilaqua and Lykoudis (1978) performed experiments on flows past a sphere and a porous disc with the same drag. However, other properties of these flows even on the level of velocity fluctuations were quite different; see also Wygnanski et al. (1986) who performed similar experiments with a larger variety of bodies with the same drag, and also references in George (2012). It should be kept in mind that in both cases the flow was partly turbulent and most probably had different large scale stability properties for different bodies not directly related to the turbulent nature of the flow within the wake. This may also contribute to the differences in the observations. Similarly, many properties of turbulent flows with rough boundaries are not defined uniquely by their friction factor either, Krogstad and Antonia (1999).

The existence of such weak solutions of the Euler equations has not been proven. Even though a nontrivial and important question is about what happens in the zero-viscosity singular limit to quantities described by smooth physically meaningful solutions of the Navier Stokes equations. This concerns not only the conserved quantities in Euler such as energy, but also key quantities such as strain, vorticity, their *self-amplification*, and acceleration, especially its *solenoidal part*, among others. More problematical are geometrical issues even the most elementary as the alignments of vorticity with the eigenframe of strain. In problems with long time behavior, i.e. infinite time limit there is a problem that the result, generally, appears not the same depending on the order which limit is taken first. This contrasts with the absence of such a problem for any finite however large Reynolds number.

A variety of empirical observations show clearly that turbulence is not a slightly viscous/dissipative phenomenon and that inertial and dissipative effects are of the same order at whatever Reynolds number and are not limited each by its own range of scales. This is manifested not only by the finite energy dissipation rate, but among other evidence, for example, by the so called Tennekes and Lumley balance between the enstrophy production (and similarly squared strain, i.e. $s_{ij}s_{ij}$) and its viscous destruction/'dissipation', so that vorticity is not frozen in the flow field - again at any, however, large Reynolds numbers. This is not surprising as the zero-viscosity limit is singular.[3] Nevertheless, there are numerous attempts to construct "theories" based solely on Euler equations with justifications such as that *a theorem which is valid for any finite, but very large, Reynolds number is expected to be compatible with results concerning infinite Reynolds number,* Bardos and Titi (2007), Migdal (1995); for more see Constantin (2016), and Chap. 8 below. Such expectations, however, are not convincing since as mentioned the vanishing viscosity limit of solutions of Navier-Stokes equations is singular. Moreover, the existing experimental evidence does not provide any indication whatsoever that Euler equations are relevant to turbulence which is a strongly dissipative phenomenon (with its dissipative "anomaly") of non-local nature at any however large Reynolds number. Again the evidence is that most real fluids exhibit purely Newtonian behavior at large Reynolds numbers with the exception of a variety of rheological fluids and the like.

Another theme is about replacements of NSE by equations, e.g. with hyperviscosity, the so-called truncated Euler equations (and the like). It is claimed that in the latter *the thermalized modes* (smaller scales) *act as a fictitious microworld on modes with smaller wave numbers in such a way that the usual dissipative Navier–Stokes dynamics is recovered at large scales* because *artificial microscopic systems can act just like the real ones as far as the emergence of hydrodynamics is concerned* and because *dissipation in real fluids is just the transfer of macroscopically organized*

[3]To quote Constantin (2016): *An asymptotic description of the Navier-Stokes equation, based on a given "nearby" smooth Euler flow is difficult because the connection between the imposed Euler flow and the Navier-Stokes equation is illusory near the boundary.*

This is absolutely correct but attempting to imagine both flows (NSE and Euler) in some "similar conditions" one is tempted to doubt that this is the only "location" where these flows are not "nearby". They seem to be drastically far from being such in most turbulent flows and at most of locations and times.

(hydrodynamic) energy to molecular thermal energy, Frisch et al. (2008). One of the problems here is that in real fluids[4] the role of strain field is not limited just by dissipation with a number of other important functions including nonlocal effects, see Sect. 6.2.2. Why strain too? in Tsinober (2009) and Chaps. 7 and 8 below. In the above context the consequence is that the nature of dissipation makes an essential qualitative difference including nonlocal effects, so that different dissipative mechanisms result in different outcomes as, e.g. in the artificial case with hyperviscosity.

3.3 Averaged Equations, Filtering, Decompositions and Similar Approaches/Issues

Based on detailed assessment of an attractor dimension it is shown that a low dimensional quantitative model is very likely fools' quest, Sirovich (1997).

The intricacy of turbulent flows as high dimensional phenomena with many degrees of freedom and nonlinear not weak interactions prompted the question whether a kind of low-dimensional description is possible, Kraichnan and Chen (1989), for more see Chap. 3 in Tsinober (2009). It was made meaningful by the fortunate empirically observed property of turbulent flows exhibiting reproducible statistical properties including averages, although it had nothing more to justify the assumption of such a possibility. This resulted in Reynolds averaged Navier-Stokes equations (RANS) and large eddy simulations (LES) and similar modeling approaches such as methods of stochastic modeling. The key word is "modeling" as all of these approaches produce "equations of turbulent motion", using the language of Monin and Yaglom (1971, p. 20), which are always unclosed. The consequence is that one needs additional information to close these equations. This is the so-called closure problem, in which one treats explicitly only a small fraction of the whole flow field, e.g. modes in some decomposition and represent the *dynamic* effects of the "rest" of the flow, typically small scales, by some additional information based on dimensional analysis, variety of scaling arguments, symmetries, invariant properties and various assumptions, many of which are of unknown validity and obscured physical and mathematical justification (if any) along with using non-trivial "surgeries" with removal of large fractions of the flow field. The really acute question is whether all these "equations of turbulent motion" are adequate in describing the genuine/true physics of turbulent flows, i.e. whether all they do really deserve to be termed as *equations of turbulent motion* or do they just mimic some the properties of turbulent flows not necessarily for the right reasons. It is only formally that the origin of the closure problem looks

[4]Massive evidence supports the view that most real fluids are Newtonian i.e, obeying the NSE with the viscous term $\nu\nabla^2 u_i$, resulting from the product of the deviatoric stress and the strain tensors $\tau_{ij}s_{ij}$ with linear relation between them $\tau_{ij} = 2\rho\nu s_{ij}$. It is for this reason the rate dissipation rate ϵ in newtonian fluids is proportional to s^2, $\epsilon = 2\nu s_{ij}s_{ij}$.

In the hyperviscous case the viscous term $(-1)^{h+1}\upsilon_h\nabla^{2h}u_i$ does not seem to correspond to any realistic relation between τ_{ij} and s_{ij} even in rheology.

as essentially technical due to the nonlinearity, but formidably difficult and so far remains unsolved, if ever. The above questions are of conceptual nature and utmost importance as they are about the nature, properties and role of the "rest", i.e. of the part of the flow not treated explicitly, i.e. modeled.[5] In other words, the questions are about whether or not an adequate reduced description of the highly dimensional problem is possible from the fundamental point of view. The crucial - explicit and/or implicit - assumption here is that the "rest" is somehow 'slaved' to the explicitly treated part of the flow (ETPF) without any reaction back and serving at best as a passive sink of energy so that the 'rest' can be "parameterized" in a simple way reminiscent of a less strong statement: *dissipation again is clearly seen as a passive process in the sense that it proceeds at a rate dictated by the inviscid inertial behavior of the large eddies* Tennekes and Lumley (1972). In other words, there is a *belief* that one can *represent the unresolved scales in (climate) models by imagining an ensemble of sub-grid processes in approximate secular equilibrium with the resolved flow. The ensemble-mean (or "bulk") effect of these sub-grid processes is then given by a set of relatively simple (e.g. diffusive-like) deterministic formulae* which are called *"parametrizations" of the sub-grid processes*, Palmer (2005). As concerns fundamental aspects this is a major misconception: the rich direct and *bidirectional* coupling between large and small scales comprises an essential part of the complex interaction between the multitude of the degrees of freedom in turbulent flows. Indeed, there is a variety of manifestations of direct and bidirectional impact/coupling of large and small scales which is essentially a manifestation of nonlocality: turbulent flows appear to be far more nonlocal than a theoretician would like to encounter. All this along with the trivial fact that the solutions of the models are not the solutions of the NSE. One of the issues of utmost importance is that turbulence - along with being an inertial - is an essentially dissipative and rotational phenomenon and that these properties reside mostly in small scales. The big 'howevers' are that due to nonlocality the latter are not passive and their impact on the resolved scales is essential

[5] Saffman wrote in (1968): *A property of turbulent motion is that the boundary conditions do not suffice to determine the detailed flow field but only average or mean properties. For example, pipe flow or the flow behind a grid in a wind tunnel at large Reynolds number is such that it is impossible to determine from the equations of motion the detailed flow at any instant. The true aim of turbulence theory is to predict the mean properties and their dependence on the boundary conditions.*

The latter view is still very popular in the community. Such an aim may be interpreted as giving up important aspects of understanding the physics of basic processes of turbulent flows. Indeed, there exist a multitude of various "theories" predicting at least some of the mean properties of some flows, but none seem to claim penetration into the physics of turbulence.

It may be said that most of the theoretical work on the dynamics of turbulence has been devoted (and still is devoted) to ways of overcoming the difficulties associated with the closure problem, Monin and Yaglom (1971, p. 9).

These difficulties have not been overcome and it does not seem that that this will happen in the near future if at all. There are several reasons for this. One of the hardest is the nonlocality property which is discussed in the Chap. 7.

We mention that formally there exist two closed formulations which in reality are suspect to be just a formal restatement of the Navier Stokes equations, at least, as concerns the results obtained to date. One is due to Keller and Friedmann (1925) infinite chain of equations for the moments and the equivalent to this chain is an equation in term's of functional integrals by Hopf (1952).

which can hardly be represented by simple formulae like eddy viscosity and the like. In other words, the small scales interact *bidirectionally* with the resolved scales.

Indeed, even just looking at the equations for the small/unresolved scales it is straightforward to realize that the small/unresolved scales depend on the large/resolved scales via nonlinear space and history-dependent functionals, i.e. essentially non-local both spatially and temporally. So it is unlikely - and there is accumulating evidence for this - that relations between them would be approximately local in contradiction to K41a hypotheses and surprisingly numerous efforts to support their validity.

One of the severe consequences of low dimensional approaches, i.e. closure, is that the essential dynamics and physics of the rotational and dissipative effects, which are mostly "small scale", are treated in terms of transport coefficients like an eddy viscosity.

Another related misconception is that the large/resolved, etc. scales obey in some sense just the Euler equation, e.g. Landau and Lifshits (1987), Migdal (1995), e.g. *in the inertial range we can neglect the viscosity and forcing and study the Eulerian dynamics of an ideal incompressible fluid.* Even if the inertial range does exist (which is does not) after excluding some range of scales such a statement by itself does not seem to make any sense - after all Euler is a differential equation.

As it stands now all the enormous effort in this direction - being useful in a great variety of applications - inherently lacks the basis to be used in the issues of fundamental nature, though some people believe that *successful approximation methods would almost certainly illuminate the physics of turbulent flow*, Salmon (1998). The key words are 'successful approximations' and the remaining big question is how successful the "equations of turbulent motion" are (in the sense just mentioned above) in the context of basic physics of turbulence. So far, any modeling remains *modelling only* and in principle cannot contribute to the basic aspects of the turbulence problem and our understanding of the (nature) of turbulence. Modeling still resembles what von Karman termed the "science of variable constants". The history of science shows clearly that the importance and non-trivial consequences of the fundamental research of practical importance is far broader than for (validation of) LES, subgrid modelling, etc. This importance cannot and should not be underestimated.

Modeling involves typically a kind of decomposition: means versus fluctuations as in Reynolds decomposition, resolved-unresolved in LES and a variety of other decompositions both formal and heuristic. But the issue of using decompositions in turbulent flows is far broader. It is the introduction of some decomposition which along with the nonlinearity results in a "cascade" with its properties depending on the properties of the decomposition. This among other problems makes the popular concept of cascade ill posed which along with other important issues calls for reminding the acute and generic problems arising from employing decompositions, see below.

A final note is about RDT approximation and similar quasi-linear approaches which pretend (in both meanings) to be able to address issues of genuine turbulence - both the problem and the phenomenon. However, the latter is a strongly non-linear

phenomenon and its fundamental issues do not seem to be amenable to quasi-linear methods and the like.

3.3.1 On the Utility (and Futility) of Various Decompositions. Exceptional Role of Physical Space

> *There is a fundamental dilemma concerning honesty of representation. If structures are guessed at the start (whether the guess is shear layers, horseshoe vortices, smoke rings, or whatever), there is danger that the subsequent theoretical construction may have its most important predictions built in. If a neutral underlying representation is adopted, like uncorrelated Fourier modes, the danger is that the effects of characteristic structures of the real flow may be totally lost* (Kraichnan 1987).

It is still as true today as it was long before that there exists no *proper analytical machinery to be used*, von Neumann (1949). In other words, there are no adequate tools to treat turbulence either analytically or via other theoretical approaches such as attempts to construct statistical and/or other theories, see Chap. 11, Appendix. Essential quotations in Tsinober (2009). Consequently, among the most common approaches are those borrowed from methods used in *linear* problems both in theory and data analysis in turbulence which are mostly reductionist ones. A typical example is represented by various decompositions of the flow field which suffer from all the troubles due to the aforementioned linearity. Hence the addition of the term "futility" above. While being useful in many respects in the analysis of both flow states and processes decompositions have a rich potential (i) to be misused leading to misconceptions and ambiguities and introducing non-trivial artefacts and spurious effects due to properties of the decompositions not characteristic of genuine turbulence and not found in physical space thereby obscuring the physics and (ii) become an object of study by themselves keeping many people busy to a large extent with the properties of decompositions themselves rather than with genuine physics of turbulence. The above is among the reasons why decompositions became to some extent less productive/useful and even obscuring the physics of turbulence. A typical example of rather popular engagement is the study, sometimes pretty sophisticated, of the interaction of the components of some decomposition not necessarily reflecting any physics, especially as concerns physical space, though frequently claiming confirmation of the 'classical energy cascade picture'[6] of turbulence which in reality is a phenomenological assumption on the hierarchical nature of turbulent flows and reflecting the hierarchic structure of various models of turbulent flows rather than reality. Otherwise it is "simply" assumed and seems to serve a kind of mostly decorative function or "pedagogical" imagery, Falkovich and Sreenivasan (2006).

[6]As a fictitious "mechanism" of delivery of energy from large to the small scales to be dissipated in the latter there - hence the frequently used term "Richardson Kolmogorov cascade", for more on this and related issues, see below Sect. 7.3.

As mentioned cascade is an ill-defined concept of phenomenological nature, see Sect. 5.4 in Tsinober (2009) and Chap. 7 below.

There is a multitude of decompositions from formal to heuristic ones and there are several difficulties with all/any decompositions as an essentially linear procedure mainly due to the nonlinear and nonlocal nature of turbulence. These difficulties are not trivial and are 'generic'.

By nonlocality is meant (among other things) the direct an bidirectional coupling between large (resolved) and small (unresolved) scales, see Chap. 6 in Tsinober (2009), and below Chap. 7. Indeed, the most accepted division of turbulent flows on large and small scales is to a large extent artificial and in some sense even unphysical. This is due to strong coupling between the two and multitude of problems/ambiguities defining the term 'scale' and related, along with the problematic nature of the decomposition approach to the phenomenon of turbulence and the necessity to handle turbulence as a whole in one sense or another.

More specific issues involve the phenomenon of cancellations with the total being much smaller than contributions from '+' and '−' modes, e.g. Meneveau (1991a, b), Chen et al. (2003), and misinterpretations of the meaning of this "smallness" of the total; employing wavepacket filters and/or narrow local wavepacket filters with some results very similar in the NSE turbulence and the corresponding random Gaussian field, e.g. that "pancake eddies" in turbulent flows possess strong alignments between velocity and vorticity, the so-called local Beltramization of purely kinematic nature, Yakhot and Orszag (1987), Shtilman (1987); the property of the nonlocal interactions with local transfer character of triadic interactions as not a property of turbulence physics, but rather a general feature of the Fourier representation, e.g. Waleffe (1992); and the general kinematic effect of "narrow banding" leading to Gaussian statistics in the narrow bands of modes whatever the departure from Gaussian statistics in physical space.

Moreover, *with a large number of degrees of freedom*, *a large class of plausible phase-space distributions have low-order moments that are indistinguishable from the low-order moments of the Gaussian distribution. This result is just a special case of the central limit theorem*, Orszag (1977). See also Lumley (1970 pp. ix, 50 and 91; 1972), Leonov and Shiryaev (1960).

These are examples of warning against *overprocessing* of the data, see below Sect. 5.2 Limitations of statistical methods.

Closely related is the hunt (mostly futile so far) for "simple basic structures" and other 'simple' objects usually embedded in a "structureless random" background, such as the so-called "coherent structures" that "govern the dynamics of the flow" and similar strong claims. There seems to exist no simple dynamically relevant structure(s) of the kind that beena while (e.g. worms, sheets, vortex structures/filaments, vortons, 'eigensolutions', significant shear layers, etc.). Those which are observed are very far from being simple, but frequently misinterpreted as such. This is among the reasons and one of the acute problems that the structure(s) is (are) not represented by the modes of any known formal decomposition, and heuristic attempts at representation of turbulence as a collection of simple objects. For example, a Fourier-decomposition of a flow in a box with periodic boundary conditions. The

emergence of structures in such a flow, such as the slender vortex filaments, in a random fashion (at random times with random orientation, and to a large extent random shapes) points to the limitation of Fourier decomposition or similar, which does not 'see' these or any other real physical structure(s) which is generally true of any decompositions. Another example is the chaotic regime of a system with only a few degrees of freedom, e.g. three as in the Lorenz system, Lorenz (1963), or four in the forced spherical pendulum, Miles (1984), but with a *continuous* spectrum. This points to a serious problem since components of a decomposition are not necessarily synonymous and cannot be automatically identified as degrees of freedom. Hence the problems with the ambiguity of decompositions, in general, and of Fourier decomposition, in particular, see Liepmann (1962), Tennekes (1976) and Lohse and Müller-Groeling (1996) for other aspects of Fourier-transform ambiguities. So far all the attempts to find a 'good' decomposition are related to what Betchov (1993) called the 'dream of linearized physicists', i.e., a superposition of some, desirably simple elements, e.g. Germano (1999). The dream is, of course, to find sets consisting of small numbers of weakly interacting elements/objects adequately representing the turbulent field. Those known so far are interacting strongly[7] and most of them nonlocally, see Sect. 7.3 below. This is a reflection of one of the central difficulties in 'solving the turbulence problem' as a whole, in general, and the 'closure problem' such as LES and other reduced descriptions of turbulence (Kraichnan 1988), in particular, as well as in construction of a kind of statistical mechanics of turbulence generally (Kraichnan and Chen 1989).

A noteworthy problem is that there is an inherent problem of definition of scales generally and in terms of some decomposition. A serious problem is that the objects claimed to represent "simple" elements of turbulence structure are "educed" from time snapshots of the flow field do not possess any (e.g. Lagrangian) identity allowing to follow their time evolution unambiguously in a reasonable manner except of "cartoon-like" approaches and the like. The problem is more general, e.g. vortex lines having no Lagrangian identity as vorticity is not frozen in the flow field at however large Reynolds numbers and consequently problems with the phenomenon of reconnection.

A recent (among many) example concerns employing band pass filters decomposition. On one hand Eyink and coauthors, see references in Eyink and Drivas (2018), insist on localness of energy cascade, i.e. *that the interscale energy transfer is dominated by local triadic interactions.* On the other hand (Leung et al. 2012) find quite a bit of non-locality in the relation between filtered vorticity and strain (*Most of the enstrophy generation occurs during stretching by the largest strain... due to non-local straining*) but at the same time claiming that they *have found that the classical view of the energy cascade is qualitatively correct, with energy passing down the cascade as large vortices straining the smaller ones.* Perhaps, it is the right place to remind

[7] From time to time there appear claims to decompositions with weakly interacting elements/objects. This appears to be true only if some parameter is small as in RDT like theories, but not for genuinely nonlinear/strong turbulent flows whith no hope that small parameter does exist.

that the problematic concept of "cascade" whatever this means, is a local process by definition, see Sect. 7.2 below.

A most popular paradigmatic heuristic example is the decomposition on energy-containing (ECR), inertial (IR) and dissipative ranges (DR). It is massively accepted/believed that the statistical properties of IR at large Reynolds numbers are universal (in some sense) and independent of viscosity/nature of dissipation and consequently of the properties of DR. In particular, it is widely believed that *Kolmogorov's basic assumption* (Kolmogorov 1941a) *is essentially that the internal dynamics of the sufficiently fine-scale structure (in x-space) at high Reynolds numbers should be independent of the large-scale motion. The latter should, in effect, merely convect, bodily, regions small compared to the macro scale,* Kraichnan (1959). This is what is called*sweeping* which is claimed to have purely kinematic nature. Though these hypotheses indeed appear to be approximately correct due to the kinematical effects, the subtle point is that they are erroneous conceptually as missing the essential effects responsible for all the dynamics in the Eulerian representation, see Sects. 5.2 and 7.3.

So the question is whether studying turbulence via (some) decompositions is aiding understanding its fundamental physics. More than one hundred years of experience seems to be a clear indication that it is hardly the case. Turbulence seems to be a kind of nondecomposable system and it must be studied as such, i.e. as a whole and nondecomposable as it is meaningful as a whole: separate "components" have at best very limited meaning if at all with all the problems as dependence of "physics" on the decomposition. Hence the special role of physical space as enhanced by nonlocality.

And just because different decompositions produce different outcomes sometimes with many subtle and opaque effects (due to all kinds of, e.g. cancellations) which are just artefacts one must beware of overprocessing the "raw" data and stay in physical space as much as possible. Any decomposition results in a nontrivial bidirectional relation between the small and the large scales (whatever this means) which is different for different decompositions, non-local (functional) both in space and time (i.e. history- dependent). Hence there is little chance that this dependence can be local (in several meanings) as has been insisted upon for quite some time. Today – also in view of accumulating evidence – it is becoming clear that locality is at best an extremely crude approximation which in many cases is good for empirical purposes, but not as a basis for studying the physics of turbulence.

3.4 Eulerian Versus Lagrangian Representations

Though these are formally equivalent, i.e. describing the same physical phenomenon, the majority of the issues are considered in an Eulerian setting in which the observation of the system is made in a fixed frame as the fluid goes by. In this case the motion is characterized by the velocity field $\mathbf{u}(\mathbf{x}, t)$ as a function of position vector,

$\mathbf{x}$, and time, t. In the Lagrangian[8] setting the observation is made following the fluid particles wherever they move. Here the dependent variable is the position of a fluid particle, $\mathbf{X}(\mathbf{a}, t)$, as a function of the particle label, $\mathbf{a}$ (usually it's initial position, i.e., $\mathbf{a} \equiv \mathbf{X}(0)$) and time, t. The two ways of description are formally equivalent with the relation between the two. The Lagrangian velocity field, $\mathbf{v}(\mathbf{a}, t) = \partial \mathbf{X}(\mathbf{a}, t)/\partial t$, and the Eulerian velocity field, $\mathbf{u}(\mathbf{x}, t)$, with $\mathbf{v}(\mathbf{a}, t) \equiv \mathbf{u}[\mathbf{X}(\mathbf{a}, t); t]$ are related by the equation

$$\partial \mathbf{X}(\mathbf{a}, t)/\partial t = \mathbf{u}[\mathbf{X}(\mathbf{a}, t); t], \tag{3.1}$$

which being a purely kinematic relation still plays an important role in whole issue.

On the technical side, the NSE equations in a pure Lagrangian (LNSE) setting are intractable so far for viscous flows, see, e.g. Sect. 9 The Lagrangian Description of Turbulence in Monin and Yaglom (1971). For this reason most of Lagrangian studies in turbulence are based on flow properties obtained from tracking fluid particles using data from Eulerian fields in DNS and directly in physical experiments, i.e. in reality using both approaches, for a recent example see Johnson et al. (2017). This is relatively reliable for velocities and accelerations along particle trajectories, more difficult for velocity derivatives (strain and vorticity) and so far impossible for pressure and its gradient. The latter is due to the essentially non-Lagrangian and nonlocal nature of pressure as defined by the whole field of velocity derivatives as inverse of the Laplacian

$$p = -\rho \nabla^{-2}(\omega^2 - 2s_{ij}s_{ij}), \tag{3.2}$$

The latter along with the "mixing" of the kinematical with the dynamic aspects in the Lagrangian setting due participation of the relation (3.1), see below, is among the main reasons why the study of Lagrangian turbulence is far more difficult and far beyond those in the Eulerian setting.

However, there are essential differences of conceptual nature rather than just technical ones. Along with the technical issues the very form of LNSE as compared to ENSE provokes nontrivial and important questions and considerations of conceptual nature as briefly outlined below.

The first issue of this kind is that in the Lagrangian setting the fluid particle acceleration is linear in the fluid particle displacement so that the 'inertial' nonlinear effects are manifested only by the term containing pressure, i.e. there are no terms like the advective terms $(\mathbf{u} \cdot \nabla)\mathbf{u}$ in the pure Eulerian setting and consequently no sweeping, which is hunted for a while to be eliminated from theories of hydrodynamic turbulence in Eulerian setting, Gkioulekas (2007). Moreover, one can hardly speak about things like Reynolds decomposition and Reynolds stresses, turbulent kinetic energy production in shear flows in a pure Lagrangian setting and the like. It is noteworthy that in the purely Lagrangian setting the importance of pressure is clearly emphasized as the only term representing the inviscid nonlinearity. In particular, the non-viscous

[8] In fact it is also due to Euler, see Lamb (1932). A detailed account on the 'misnomer' by which the 'Lagrangian' equations are ascribed to Lagrange is found in Truesdell (1954), pp. 30–31.

nonlinearity in the Lagrangian representation cannot be interpreted in terms of some local cascade as it cannot be maintained solely by the pressure term. This is due to the essentially nonlocal relation between fields of pressure and velocity (3.2). The latter is also the reason that the non-viscous nonlinearity represented by pressure cannot be purely inertial as defined nonlocally only by the field of velocity derivatives (sic!) as in (3.2). It is also far less clear how one can employ decompositions even at the problematic level as done in the pure Eulerian setting. An extreme example is the absence of a "cascade" in Lagrangian chaotic, but Eulerian laminar flows. Finally, there is a difficulty of handling the rotational part of particle acceleration due to the terrible nonlinearity of the viscous term in the Lagrangian setting.

A most important issue of a conceptual nature comes from the seemingly simple kinematic equation (3.1) relating the Eulerian and Lagrangian settings. This equation is nonlinear for almost all (even for very simple) fluid flows and is generically non-integrable for all flows with the exception of very simple flows such as unidirectional ones. Thus for a wide class of almost all laminar flows in the Eulerian setting, i.e. with the Eulerian velocity field, $\mathbf{u}(\mathbf{x}; t)$ not chaotic, regular and laminar, the Lagrangian velocity field $\mathbf{v}(\mathbf{a}, t) \equiv \mathbf{u}[\mathbf{X}(\mathbf{a}, t); t]$ as any other property of fluid particle is chaotic because $\mathbf{X}(\mathbf{a}, t)$ is chaotic! This fact is of the utmost importance for issues like the relation between the Eulerian and Lagrangian (statistical) characteristics of the same flow field. It must be emphasized that this chaotic behavior is of purely kinematic nature resulting solely from the Eq. (3.1) relating the Eulerian and Lagrangian settings and has nothing to do with dynamics, i.e. genuine (as NSE) turbulence, for a recent example see Lukassen and Wilczek (2017), which exhibit "lagrangian" intermittency, e.g. "intermittency" of a purely Eulerian Gaussian field in its Lagrangian representation. This also concerns all problems with prescribed velocity fields in Eulerian setting – synthetic, Gaussian, etc. Similarly, all randomly forced E-laminar flows with low Reynolds number including multiscale ones, flows in porous media, microdevices, to name a few, belong to this category, being thus not easily acceptable for modelling real turbulence (i.e governed by NSE) using the Lagrangian properties of afore mentioned prescribed (i.e. non-NSE) E-velocity fields. The obvious examples are issues on interaction between inertial and diffusive effects at the level of vorticity and strain (dissipation) in real turbulent flows.

Indeed, whereas the E-turbulence is a dynamic phenomenon this is not necessarily the case with the L-turbulence which may be purely kinematic one as in Fig. 1.3 above of a flow at small Reynolds numbers. In other words the Eulerian setting reveals the pure dynamic chaotic aspects of genuine turbulence as contrasted to "mixing" of the kinematic with the dynamic ones in the Lagrangian setting, i.e., in genuine turbulence the latter contains both kinematic and dynamic ones, which seem to be essentially inseparable. Consequently, studying Lagrangian statistics only may not provide adequate information on the L-statistics of genuine turbulence as not necessarily containing its pure dynamic "stochasticity".

In other words the flow can be purely L-turbulent (Lagrangian chaotic), but E-laminar as mentioned above as almost any E-laminar flow at small Reynolds numbers, i.e. without any E-turbulent counterpart, simple laminar Eulerian velocity field (E-laminar) may create a complicated Lagrangian field (L-turbulent). However, if the

flow is E-turbulent, i.e. $Re \gg 1$ it is also L-turbulent. In other words, one encounters a dilemma concerning how meaningful is the question on the "relation" between the E- and L-statistics of the same flow and not only as in the latter case. This shows that, in general, there may not exist a unique relation between Lagrangian and Eulerian statistical properties in genuine turbulent flows as was foreseen by Corrsin (1959): *in general, there is no reason to expect that* L_{ik} (the Lagrangian two-point velocity correlation tensor) *and* E_{ik} (the Eulerian two-point velocity correlation tensor) *will be uniquely related*. This adds an issue concerning the use of Eulerian, Lagrangian and mixed approaches especially in the *same flow* and also in the context of the equivalence of ENSE and LNSE.

One more consequence is that the structure and evolution of passive objects including fluid particles in genuine turbulent flows arises from two contributions: one due to the Lagrangian chaos and the other due to the complex nature of the Eulerian velocity field itself. Hence the behavior of passive objects in flows with small Reynolds number looks as perfectly turbulent (see Fig. 1.3) reflecting the qualitative difference between the chaotic flow properties in Eulerian and Lagrangian settings.

Thus the ENSE and LNSE are not equivalent as the latter appear to be 'more chaotic' with the multitude of E-laminar flows having no counterpart to the corresponding L-turbulent statistics in the same fluid flow. The equivalence is between the LNSE with the ENSE plus the Eq. (3.1) relating the Eulerian and Lagrangian settings. An interesting aspect concerning the differences between the mathematical properties of Lagrangian and Eulerian settings of Euler equations is found in Constantin et al. (2016).

Among additional issues is one on ergodicity as most of it refers to the Eulerian setting. The problem with the Lagrangian setting is that, generally, Lagrangian statistical properties are not (and cannot be) stationary for stationary fields in the Euler setting with the exceptional cases such as both homogeneous and stationary Eulerian field implying stationarity of Lagrangian statistical functions dependent on a single space point, Lumley (1962), see also Sect. 9 in Monin and Yaglom (1971). This along with the 'more chaotic nature' of the Lagrangian setting is the likely reason that non-ergodicity is encountered in the Lagrangian setting for flows which are ergodic in the Euler setting, see e.g., references for Sect. 3.7 Ergodicity in Tsinober (2009).

The Lagrangian description of fluid flows is physically more natural than the Eulerian one, since it is related most directly to the motion of fluid elements. Though a great variety of dynamical issues in turbulence *per se* can be treated satisfactorily in the Eulerian setting, further insight into the basic physics of turbulent flows requires information on time evolution and associated Lagrangian statistics of such quantities as vorticity, strain, accelerations, etc., as relating the spatial structure (the most popular time snapshots) and the time dimension. There are important problems/questions of dynamical nature for which Lagrangian information is of utmost importance (as well), i.e., one must employ both settings with all the problems mentioned above. Examples include the class of flows where turbulence memory and/or sensitivity to the inflow conditions plays an essential role, partly-turbulent flows involving the entrainment phenomenon and flows with polymer solutions and other additives.

With all the problems in using the Lagrangian setting it is hard to agree with the 'natural expectation' that nothing new can be expected in a pure Lagrangian setting. One question concerning the limit deserves to be mentioned $\nu \to 0$. It is about so-called Cauchy invariant $\mathbf{\Omega}$ expressed in terms of vorticity and the Jacobi deformation matrix $\partial X_k(\mathbf{a}, t)/\partial a_j$. The peculiar feature is that the equation for the Cauchy invariant, see Tsinober (2009) p. 343 and Sect. 13.5, is a parabolic equation which is linear in $\mathbf{\Omega}$ having a complicated viscous term with coefficients defined by the metrics of the Lagrangian trajectories $\mathbf{X}(a, t)$. Setting $\nu = 0$ results in $\mathbf{\Omega} = const$ along fluid particle trajectories, i.e., it is an inviscid pointwise invariant. It is unlikely that the same result is arrived in the limit $\nu \to 0$?

For more, see Sect. 3.6. Eulerian versus Lagrangian descriptions, Sect. 4.2. Kinematic/Lagrangian chaos/advection Sect. 4.3. On the relation between Eulerian and Lagrangian fields, 6.7 Acceleration and related matters, 9.3.4 Issues associated with the E-L relations. Analogy between genuine turbulence and Lagrangian chaos, and 13.5 Pure Lagrangian description in Tsinober (2009).

3.5 Final Remarks

Returning to the NSE, the conclusion that NSE are useful only as an experimental tool would be incorrect. It is true that there is little substantial theoretical use of NSE in turbulence and that the road to high Re does not look as short even in the simplest geometries. However, as mentioned above there are several ways to do this implicitly, i.e., by indirect use of NSE and their consequences. For example, looking at the NSE and their consequences themselves enables one to recognize at least some of the dynamically important quantities and physical processes involved including some important nonlinear objects many of which still await to be identified and properly defined. In other words, NSE and their consequences tell us what quantities and relations should be studied to aid the difficult task of identifying the significant nonlinear objects of turbulence. So far this can be done mostly experimentally,[9] but this kind of 'guiding' is hoped to be also useful theoretically.

It seems that in order to solve even the standard NSE problems, there is a need for progress in the understanding of turbulence as a physical phenomenon and not the other way around. There is therefore every reason to follow the advice of Kolmogorov (1985) to go on with non-trivial observations. The role of mathematics in physical sciences cannot be underestimated. Concerning turbulence mathematics it is supposed to aid in achieving the goals of basic physical problems. However, in the case of turbulence mathematics did not became what von Neuman and von Karman expected and played a pretty modest role especially as concerns the qualitative physics of turbulence. These were mainly limited by proofs of well known facts and related by employing methods of functional analysis with Sobolev, Besov

[9] Victor Youdovich used to say that one of the best solutions of NSE is experiment, 1971, private communication.

and other more exotic spaces with not less exotic objects. These are "suspect" to belong to excessive generalizations or 'overgeneralizations' dealing with objects of unknown relevance to the physical phenomenon of turbulence and unjustified claims that sometimes it makes physical sense to admit solutions with singularities in spite that, generally, singularities are not admissible from the physical point of view and, in fact, nobody can tell so far what is the relationship between the two. The big problem is that it is not at all clear how the results obtained with these singular objects, e.g. distributions are relevant/related or even have anything to do with turbulence.[10] The most elegant exception is a set of theoretical results on the a priory upper bounds of long time averages of dissipation and global transport of mass, momentum and heat, see Doering (2009) and references therein, though these exploited the methods of functional analysis. The issue is broader and extends well into theoretical physics.

This tendency for overgeneralization in science was criticized by Poincare at the beginning of the 20th century 1902:

And in demonstration itself logic is not all. The true mathematical reasoning is a real induction, differing in many respects from physical induction, but, like it, proceeding from the particular to the universal. All the efforts that have been made to upset this order, and to reduce mathematical induction to the rules of logic, have ended in failure, but poorly disguised by the use of a language inaccessible to the uninitiated. H. Poincare, Science and method, p. 286, Dover, 1952.

Some have set no limits to their generalizations, and at the same time they have forgotten that there is a difference between liberty and the purely arbitrary. So that they are compelled to end in what is called nominalism; they have asked if the savant is not the dupe of his own definitions, and if the world he thinks he has discovered is not simply the creation of his own caprice. H. Poincare, Science and hypothesis, pp. xxiii–iv, Dover, 1952.

For example, Foiaş et al. (2001) have shown that there are measures on a function space that are time-invariant. However, invariance under time evolution is not enough to specify a unique measure which possibly would describe turbulence. An attempt to select the right one was made by Ruelle (1979): *The effect of thermal fluctuations on a turbulent flow is estimated, and it is argued that these fluctuatioins are important in selecting the stationary measure on phase space which describes turbulence*.

Another problem is that it is not clear how the objects that the authors have constructed and used in their proofs are relevant/related or even have anything to do with turbulence.

All these and similar are of interest for their own sakes, but from the point of view of theoretical study of turbulence and its understanding it is definitely not the first priority to reproduce in a (more or less) rigorous manner simple known results by complicated theory. *It may be original to produce a simple known result by a complicated theory, but mathematical study of turbulence is not supposed to have its main aim as providing full employment for abstract mathematicians*, Saffman

[10]There are "smaller" problems such as uniqueness as shown in a recent example by Buckmaster and Vicol (2017) that *weak solutions of the 3D Navier-Stokes equations are not unique in the class of weak solutions with finite kinetic energy*.

(1978). Quoting von Karman (1943), *in order to get the solution of engineering problems..., you need some kind of tool designers. These are the real applied mathematicians. Their original backgrounds may differ..., but their common aim is to "tool up" mathematics for engineering* and physics. It is of interest to contrast the statement by Friedrichs*: Applied mathematics is those areas in which physicists are no longer interested.* In turbulence there seem to be still no such tool(s) at all. Mathematicians mostly live their own life in a variety of functional spaces and even have their own Journal of Mathematical Fluid Dynamics. However, this is only a part of the problem. *Sometimes experiments provide us with so beautiful and clear results that it is a shame on theorists that they cannot interpret them,* Yudovich (2003), not to say to guide the observations. Unfortunately, not many care about the observational evidence. The approach is in reality in some sense the reverse. The widespread view of both mathematicians and theoretical physicists is that the main function of all experiments/observations both physical and numerical is to "validate theories"- paradoxically so far nonexistent. This is beautifully reflected by Monin and Yaglom (1971), p. 21 especially in the last sentence of the following quotation: *The combination of theoretical and experimental approaches, which is extremely fruitful in all investigations of natural phenomena, is especially necessary in statistical fluid mechanics where the theory is often still of a preliminary nature and is almost always based on a number of hypotheses which require experimental verification. However, we have avoided introducing experimental results which have no theoretical explanation and which do not serve as a basis for some definite theoretical deductions, even if these data are in themselves very interesting or practically important.* It is an approach which to a large degree (not unjustly) was called by Saffmann "postdiction" and is not useful especially in a situation in which from the basic point of view there is almost nothing to be confirmed. *Correlations after experiments done is bloody bad. Only prediction is science,* Hoyle (1957). In other words, the role of experiments in turbulence goes far beyond the view of those who think of experimentalists as a superior kind of professional fixers knowing how to turn nuts and bolts into a confirmation of other people's 'theories'. The issue is more than serious in view of the general theoretical failure, i.e. absence of theory based on first principles. There are a number of issues of special concern about the relation of what is called theory in turbulence research and observations/experiments, in particular, regarding the use of the factual information as concerns fundamental aspects. We return to these and similar issues in the Epilogue, see also Sect. 10.3. Turbulence versus mathematics and vice versa in Tsinober (2009).

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Part II
Issues of Paradigmatic Nature I—Origins and Nature of Turbulence

The true problem of turbulence dynamics is the problem of its origin(s) and successive development from some initial and boundary conditions and forcing to an ultimate state assuming "for simplicity" that this state is statistically stationary, i.e., with statistically stationary forcing. However, since this route is extremely complicated and involved, a second approach is used quite frequently using statistical methods at the price of loosing important aspects of dynamics among other. Namely, turbulent flows are studied "as they are" regardless of their origin. This was prompted also by the beliefs in universality and lack of "memory" as concerns the initial and boundary conditions and the nature of the mechanisms of turbulence production and sustaining. It appears that there is far less universality and quite a bit of memory than expected. In both approaches, at least some aspects of the time evolution are of central importance, since turbulence dynamics is a process and has memory along with other properties such as nonlocality with its diverse manifestations. These are the reasons that before getting to the main theme—the fully developed turbulent flows—an overview is given of the origins of turbulence. This is followed by the discussion of the various facets of the "undeniably statistical nature," e.g., Foiaş et al. (2001), contrasted to the "deterministic origin" of turbulence as described by purely deterministic non-integrable equations and related issues, such as the necessity of replacing the paradigm of the meaning of apparent randomness, stochasticity of turbulence which is roughly just the complexity due to a large number of strongly interacting degrees of freedom governed by purely deterministic equations. These comprise the first part of the major qualitative universal features of turbulent flows briefly described/listed in the Introduction. Considerable attention and emphasis is given to generic limitations of statistical methods generally including statistical theorizing and as concerns validation of "theories," interpretation of observational evidence and interpretational abuse.

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Chapter 4
Origins of Turbulence

Abstract It is a common (but not the only) view that the origin of turbulence is in the instabilities of some basic laminar flow. As the Reynolds number increases, some instability sets in, which is followed by further instabilities/bifurcations, transition and a fully developed turbulent state and the processes by which flows become turbulent are quite diverse. One process deserves special attention as a specific universal phenomenon. It is the continuous transition of laminar flow into turbulent flow via the entrainment process through the boundary between laminar/turbulent regions in the partly turbulent flows. In all these there is a distinct Lagrangian aspect: the abrupt transition of fluid particles (i.e. Lagrangian objects) from the laminar to turbulent state when passing across the laminar/turbulent 'interface'. Abrupt is a key word here, i.e. without any cascade whatsoever. Another issue of special interest is that there is a conceptual difference between the two kinds of flows: those arising 'naturally', e.g. by a simple time independent and smooth in space deterministic forcing, i.e. governed by a purely deterministic system and those produced by some external random source/excitation. The point is illustrated by the examples of stochastic forcing of integrable nonlinear equations (Burgers, Korteveg de Vries, etc. or just NSE at low Reynolds numbers) which without such forcing do not exhibit any "stochastic" behavior whatsoever. In other words, the properties of a turbulent flow are not neutral to the nature of excitation. Likewise boundary, initial and inflow conditions may cause qualitative differences especially due to essential role of nonlocal properties of the system. There is a qualitative difference between transition to turbulence as a phenomenon characterized by a large number of strongly interacting degrees of freedom and transition to chaotic behavior, in general, and between such notions as degrees of freedom and the dimension of attractor (assumed to exist), in particular. Any fluid flow which is adequately represented by a low-dimensional system is not turbulent—a kind of definition of 'non-turbulence'. The immediate examples are laminar in Euler setting (E-laminar) which are low-dimensional chaotic fluid flows (L-turbulent).

The state of some particular flow configuration depends on the magnitude of the Reynolds number, Re, but is not solely defined by Re. At small enough Re the flow is laminar for any initial conditions and is the same for stationary conditions. At larger Re the flow state, generally, depends strongly on initial conditions, at large Re is turbulent and is believed to be smooth enough and unique in some sense at

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any Re. For example, for statistically stationary conditions the asymptotic state is an attractor - a complex non-trivial object, Doering and Gibbon (2004), Foiaş et al. (2001), Robinson (2001, 2007).

4.1 Instability

As mentioned it is a common (but not the only) view that the origin of turbulence is in the instabilities of some basic laminar flow. This is understood in the sense that any flow is started at some moment in time from rest, and as long as the Reynolds number or a similar parameter is small, the flow remains laminar. As the Reynolds number increases, some instability sets in, which is followed by further instabilities/bifurcations, transition and a fully developed turbulent state. Such sequences of events occur, generally, not throughout the whole flow field, but at successive downstream locations in spatially developing flows, at the laminar/turbulent 'interfaces' in turbulent spots and in all partly turbulent flows which comprise the majority of all observed ones. The main special features of these flows are the coexistence of regions with laminar and turbulent states of flow and continuous transition of laminar flow into turbulent via the entrainment process through the boundary between the two. In all these there is a distinct Lagrangian aspect: the abrupt transition of fluid particles (i.e. Lagrangian objects) from the state when passing across the laminar/turbulent 'interface'. Abrupt is a key word, i.e. without any cascade whatsoever.

From the mathematical point of view the transitions from one flow regime to another with increasing Reynolds number – as we observe them in physical space – are believed to be a manifestation of generic structural changes of the mathematical objects called phase flow and attractors in the phase space through bifurcations in a given flow geometry, Hopf (1948), though partly turbulent flows with the special feature of these flows being the coexistence of regions with laminar and turbulent states of flow are not easily 'fit' into this picture.

Whereas the processes by which flows become turbulent are quite diverse,[1] most known qualitative and some quantitative properties of many (but not all) turbulent

[1]The diversity of the processes by which flows become turbulent is in part due to the sensitivity of the instability and transition phenomena to various details characterizing the basic flow and its environment. For example, the Orr-Sommerfeld equation governing the linear(ized) (in)stability contains the second derivative of the basic velocity profile. Many flows (some of the so-called open flows) are very sensitive to external noise and excitation. There are essential differences in the instability features of turbulent shear flows of different kinds (wall bounded - pipes/channels, boundary layers, and free - jets, wakes and mixing layers), thermal, multidiffusive and compositional convection, vortex breakdown, breaking of surface and internal waves and many others. It is important that such differences occur also for the *same* flow geometry, which display in words of Morkovin *bewildering variety of transitional behavior* see Morkovin and Reshotko (1990). The specific route may depend on initial conditions, level of external disturbances (receptivity), forcing, time history and other details in most of the flows mentioned above. This diversity is especially distinct for the very initial stage - the (quasi) linear(ized) instability. Later nonlinear stages are less sensitive to such details. Hence there is some tendency to universality in strongly nonlinear regimes, such as developed turbulence. But even in this case there are examples of multiple bifurcations, etc.

flows were considered as not depending either on the initial conditions or on the history and particular way of their creation, e.g. whether the flows were started from rest or from some other flow and/or how fast the Reynolds number was changed. However, relatively recent evidence shows that there exist differences deserving special attention; some flow properties, generally, do depend on the nature of excitation, i.e. forcing and inflow, initial and boundary conditions. This calls for a discussion and reexamination of such issues as universality, symmetries and nonlocal properties of turbulent flows among others.

One of the basic features of utmost importance of processes resulting in turbulence is that all of them tend to enhance the field of velocity derivatives, i.e. the rotational and dissipative properties of the flow in the process of transition to turbulence. The first property is associated with the production of vorticity (i.e. its rotational nature), whereas the second property is due to the production of strain, i.e. its dissipative nature. Both processes are known empirically to be irreversible, i.e. with prevalent positive production. These two key properties of all turbulent flows are among our main concerns below.

4.2 Transition to Turbulence Versus Routes to Chaos

One of the main achievements of modern developments in deterministic chaos is the recognition that chaotic behavior is an intrinsic fundamental property of a wide class of nonlinear physical systems (including turbulence) described by purely deterministic equations/laws, and not a result of external random forcing or errors in the input of the numerical simulation on the computer or the physical realization in the laboratory. The nonlinear systems and the equations describing them produce an apparently random output 'on their own', 'out of nothing' – it is their very nature. However, there is a variety of qualitatively different systems exhibiting such a behavior just like there is a large diversity of such behaviors.

The qualification of turbulence as a phenomenon characterized by a large number of strongly interacting degrees of freedom enables us to make a clear distinction between transition to turbulence and transition to chaotic behavior, in general, and between such notions as degrees of freedom and the dimension of attractor (assumed to exist), in particular.

The main points of distinction are as follows.

Low dimensional chaotic systems like the famous Lorenz (1963) system or the spherical pendulum studied by Miles (1984) change their behavior from simple regular (as periodic) to distinctly chaotic as some parameter of the system changes. However, obviously the number of degrees of freedom of all such systems remains the same, only the character of the interaction of these degrees of freedom changes. In contrast, the idea that the essential feature of transition to turbulence[2] is an increase of the number of excited degrees of freedom dates back to Landau and Lifshits (1944)

[2]Not to be confused with transition to low dimensional (usually) temporal chaos.

and Hopf (1948) and is correct, though the details of their scenario appeared to be not precise, see Monin (1986). However, *Kolmogorov's ideas on the experimentalist's difficulties in distinguishing between quasi-periodic systems with many basic frequencies and genuinely chaotic systems have not yet been formalized*, Arnold (1991). In other words it is very difficult if not impossible to make such a distinction in practice.

It is now recognized that *despite the considerable successes of the present studies of the application of modern ideas on chaos to well-controlled fluid flows, they appear to have little relevance when applied to the more general problem of fluid turbulence*, Mullin (1993, p. 93) see also Tritton (1988, p. 410) and that *considerations of the properties of fully-developed turbulence require rather different ideas,* Batchelor (1989).

So it is quite plausible that any fluid flow which is adequately represented by a low-dimensional system is not turbulent – a kind of definition of 'non-turbulence'. The immediate examples are laminar in Euler setting (E-laminar) low-dimensional chaotic fluid flows (L-turbulent) and other examples mentioned in the introduction above and Chaps. 3 and 9 in Tsinober (2009).

Here is the right place to note that there is an important difference between the number of degrees of freedom roughly proportional to the number of ordinary differential equations necessary to adequately represent a system described by partial differential equations (NSE) and the dimension of the attractor of the system, though sometimes it is considered *natural to identify the dimension of the attractor as an effective measure of the number of degrees of freedom in the system*, Doering and Gibbon (2004). However, in a particular dynamic system described by ODE, the former is obviously fixed and is independent of the parameters of the system, whereas the latter is changing with the parameters but is bounded. In turbulence both are essentially increasing with the Reynolds number and become very large at large Reynolds number.

4.3 Many Ways of Creating (Arising/Emerging) Turbulent Flows

Any turbulent flow is maintained by an external source of energy produced by one or more mechanisms. The mechanisms maintaining/sustaining and influencing turbulence, at least some of them, are believed to be closely related (but are not the same) to those by which laminar and transitional flows become turbulent. Apart from a great variety of turbulent flows in nature/technology and 'natural' ways resulting from instabilities, turbulent flows can be produced by 'brute force', i.e. by applying external forcing of various kinds both in real physical systems and in computations by adding some forcing in the right hand side of the Navier–Stokes equations. For example, one of the "simplest" kinds of turbulent flow - quasi-homogeneous and quasi-isotropic - can be established by moving a grid through a quiescent fluid or placing such a

grid in a wind tunnel, or oscillating such grids in a water tank. Turbulence is produced by forcing in the interior of the fluid flow (by electromagnetic forces, e.g. in electrolytes, liquid metals or plasma; or other body forces) or at flow boundaries, which can be still or moving/flexible, smooth or rough, simple or complex. Similarly turbulent flow can be produced numerically with an infinite versatility by adding a force (random or deterministic) to the right hand side of the Navier–Stokes equations and/or forcing the flow at its boundaries.

An important point is that the nature of forcing (deterministic, random, temporally modulated or whatever) is secondary in establishing and sustaining a turbulent flow, provided that the Reynolds number is large enough and the forcing is mostly in the large scales, but, in contrast to the common beliefs, not necessarily unimportant to the particular properties of the resulting turbulent flow. Moreover, there is a conceptual difference between the two kinds of flows: those arising 'naturally', e.g. by a simple time independent and smooth in space deterministic forcing, i.e. governed by a purely deterministic system and those produced by some external random source.[3] The point is illustrated by the examples of stochastic forcing of integrable nonlinear equations (Burgers, Korteveg deVries, etc.) which without such forcing do not exhibit any "stochastic" behavior whatsoever. In other words, one can wonder about the differences between the turbulent flows produced by some stochastic forcing of NSE (or a real physical system) and those arising intrinsically, though it is a common practice to consider both as similar, at least qualitatively, so that the stochastically forced ones are even seen as representative of real turbulent flows. However, the properties of a turbulent flow are not neutral to the nature of excitation, e.g. if it is not large scale may result in qualitative differences such as in case of broadband forcing.[4] Similarly boundary, initial and inflow conditions may cause qualitative differences either especially due to essential role of nonlocal properties of the system.

At small enough Reynolds numbers, the flow produced by deterministic forcing is not random, it is laminar, but the flow produced by random forcing, though random, is in many respects trivial as any randomly forced linear system, e.g. there is no interaction between its degrees of freedom. Strictly speaking this is true of pure dynamic flow properties described in Eulerian setting because some of its 'kinematic' properties as described in pure Lagrangian setting can be and usually are pretty complex and not trivial (L-turbulent), see, e.g. Fig. 1.3.

Thus a turbulent flow originates not necessarily out of a laminar flow with the same geometry. It can arise from any initial state including a 'turbulent' one, such as random initial conditions in direct numerical simulations of the Navier–Stokes equations. That is, the transition from laminar to turbulent regime is not the only causal relation. This problem is related to a somewhat 'philosophical' question on whether flows become or whether they just are turbulent, and to the unknown properties of the

[3]The random force method in turbulence theory is due to Novikov (1963).

[4]In this case if the forcing is strong enough not only in the large scales (broadband forcing) or being prevalent in smaller scales it can balance the viscous effects directly thereby bypassing the nonlinearity.

phase flow, attractors and related matters, which are mostly beyond the scope of this book.

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Chapter 5
Nature of Turbulence

Abstract The main dispute about the origins and nature of turbulence involves a number of aspects and issues in the frame of the dichotomy of deterministic versus random. In science this dispute covers an enormous spectrum of themes such as philosophy of science, mathematics, physics and the other natural sciences. Fortunately, we do not have to venture into this ocean of debate and opposing and intermediate opinions. This is mainly because (as it now stands) turbulence is described by the NSE which are purely deterministic equations with extremely complex behavior enforcing use of statistical methods, but this does not mean that the nature of such systems is statistical in any/some sense as frequently claimed. The bottom line is that turbulence is only apparently random: the apparently random behavior of turbulence is a manifestation of properties of a purely deterministic law of nature in our case adequately described by NSE. An important point is that this complex behavior does not make this law either probabilistic or indeterminate.

One of the problems of turbulent research is that we are forced to use statistical methods in one way or another. All statistical methods have inherent limitations the most acute reflected in the inability of all theoretical attempts (both physical and mathematical) to create a rigorous theory along with the other inherent limitations of handling data such as the description and interpretation of observations without having a theory upon which to rely. However, the technical necessity of using statistical methods is commonly stated as the only possibility in the theory of turbulence. The consequence of this leads to the necessity of low-dimensional description with the removal/replacement in one way or another (modeling) of small scale and high frequency components of the dynamics of a flow including quantities containing a great deal of fundamental physics of the whole flow field such as the rotational and dissipative nature of turbulence among others. Thus, relying soley on statistical methods (again with all respect) one is inevitably losing/missing essential aspects of basic physics of turbulence which are vital to creating a rigorous theory non-existent so far, see below Sect. 11.1 On the absence of genuine theory. So one stays with the troublesome question of whether it is possible to penetrate into the fundamental physics of turbulence via statistics alone. In other words, there is an essential difference between the enforced necessity to employ statistical methods in view of the absence of other methods so far and the impossibility in principle to study turbulence via other approaches. This is especially discouraging all attempts to get into

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more than just the "en masse" properties of turbulent flows. Also such a standpoint implies that there is not much to be expected concerning the *essence* of turbulence using exclusively statistical methods.

Typical questions are as follows. Is turbulence just probabilistic or just deterministic? or both or whatever? Can it be just statistical? But perhaps, as it stands today the first question is whether these and similar questions are as meaningful as thought previously

The main dispute about the origins and nature of turbulence involves a number of aspects and issues in the frame of the dichotomy of deterministic versus random. The latter term is frequently replaced by its synonyms such as statistical, stochastic, probabilistic: to stress this point we will deliberately use all of these terms as having the same meaning. In science this dispute covers an enormous spectrum of themes such as philosophy of science, mathematics, physics and the other natural sciences.

Fortunately, we do not have to dwell in this ocean of debate and opposing and intermediate opinions.[1] This is mainly because (as it now stands) turbulence is described by the NSE which are purely deterministic equations.[2] It is due to this fact along with developments in what is called deterministic chaos that the quite common contraposing of the 'traditional' statistical and deterministic (and so-called structural) approaches in turbulence research has lost most (but not all) of its meaning. As mentioned it is well established that even simple systems governed by purely deterministic nonlinear sets of equations, such as those described by only three nonlinear ordinary differential equations, as a rule exhibit irregular apparently random/stochastic behavior. In fact, in respect to turbulence this was known long before the 'discovery of chaos': this deterministic system was (and is) studied by exclusively statistical methods, which are used today to study the statistical properties of various chaotic dynamical systems described by purely deterministic equations, e.g. Ornstein and Weiss (1991), Loskutov (2010).

Even the simplest nonlinear systems exhibiting chaotic behaviour are analyzed via various statistical means. Also, the so called 'coherent structures' in turbulent flows are sought using essentially statistical methods, such as conditional statistics though with limited success. Finally, methods of dimensional analysis, similarity and symmetries including group theoretical methods and phenomenological arguments

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[1] *Quantum mechanics is certainly imposing. But an inner voice tells me that it is not yet the real thing. The theory says a lot, but does not really bring us any closer to the secret of the "old one." I, at any rate, am convinced that He does not throw dice* (Einstein 1926).

Not only does God definitely play dice, but He sometimes confuses us by throwing them where they can't be seen (Hawking and Penrose 1996).

Today "chaotic" and "deterministic" are not considered as counterparts of a false dichotomy. This takes its origin to Poincare (1952), *H. Poincare, Science and hypothesis, pp. xxiii–iv, Dover, 1952.*

[2] However, we repeat that since Leray (1934) until recently one was not sure about the theoretical, but not observational, possibility that turbulence is a manifestation of breakdown of the Navier–Stokes equations. Also note the statement by Ladyzhenskaya (1969): *... it is hardly possible to explain the transition from laminar to turbulent flows within the framework of the classical Navier-Stokes theory.*

are applied exclusively to quantities expressing the statistical properties of turbulent flows, see, e.g. Bonnet (1996), Holmes et al. (1996).

One can study the properties of deterministic systems by statistical methods, but this does not mean that the nature of such systems is statistical in any/some sense as frequently claimed.

5.1 Turbulence is Only Apparently Random

There is no "intrinsic noise" - all the noise, e.g. in grid flows is under the NSE control.

One of the most important aspects is that the apparently stochastic/random behavior of turbulent flows is their intrinsic property - as in intrinsic chaos - though in many other respects the NSE turbulence is qualitatively different. This is in contrast to stochastic NSE with the external random forcing or excitation. There is no necessity for external random forcing either in the interior of the fluid flow or at its boundaries, nor does one need to start the turbulent flow with some random initial conditions provided that the Reynolds number is large enough. The fascinating popular question is how such a behavior emerges from purely deterministic equations as the Navier–Stokes equations are, deterministic forcing along with smooth nonrandom initial and boundary conditions. The common answer is the extreme sensitivity to disturbances however small (initial conditions, boundary conditions, external noise). However, this has little to do with the complete randomness associated with the "absence of laws" as turbulence is governed by purely deterministic equations. The essential point and the bottom line is that *to say that turbulent flow is 'completely random' would define turbulence out of existence,* Tritton (1988).

The term "apparently" is stressed again for several reasons. The first - already mentioned - is that turbulence is described by purely deterministic equations. Second, turbulence only looks and exhibits seemingly/effectively random behavior. This extremely intricate and complex behavior stems from the non-trivial strong nonlinear and nonlocal interaction of an enormously large number of degrees of freedom (DOF) with the number of DOF increasing from unity for a stationary laminar flow at subcritical Reynolds number and growing as $\sim \mathrm{Re}^{9/4}$ at large Reynolds numbers, Landau and Lifshits (1959), not later editions, since this estimate was removed from the subsequent editions. Another and a different way to put this is that what is called intrinsic stochasticity (apparent) is just the *complexity of the dynamics along the attractor rather than its stability*, Arnold (1991), with highly non-trivial structure of the attractor governed by the NSE. The third concerns the very meaning of the term "random". Mathematicians and physicists alike have found it advantageous to introduce axiomatically the concept of complete randomness associated with the "absence of laws", which is not the case here.

Most problems in classical stochastic processes are reduced to solubility by statistical independence, or the assumption of a normal distribution (which is equivalent) or some other stochastic model; because of the governing differential equations, the turbulent velocity at

> *two space-time points is, in principle, never independent – in fact, the entire dynamical behaviour is involved in the departure from statistical independence. The equations, in fact, preclude the assumption of any ad hoc model, although this is often done in the absence of a better idea*, Lumley (1970).

It is the intricacy and complexity which is taken frequently as identical/synonymous to the random/statistical nature of turbulence and which enforces the employment of statistical methods which is quite a different matter. The necessity and justification of using statistical methods comes both from our ignorance/lack of information about some aspects of the phenomenon in question – or, in the words of Kolmogorov (1956) – *a large number of random factors* and/or from excessively large amounts of information and consequent inability or lack of desire to handle this huge information otherwise ideally in full. The two situations are essentially the same as in both cases only partial information is used. In other words the probabilistic approach *is related, in part to this ignorance, in part to our knowledge,* Laplace (1951). The fascinating empirical outcome in both cases is exhibited by statistically stable properties, but the problematic aspect is that the statistical methods both in theory and in description and interpretation of the data from the laboratory, the field and numerical experiments are inherently limited or deficient, see next subsection.

On the other hand, the apparently random behavior of turbulence is a manifestation of properties of a purely deterministic law of nature in our case adequately described by NSE. An important point is that this complex behavior does make this law neither probabilistic nor indeterminate. Frequently assumed unpredictability of individual realizations of turbulent motion - which has made the application of stochastic theory attractive - is incompatible with the fact that this as any other realization is governed by NSE. The lack of predictability is related to what we humans can observe, analyze and compute, (i.e. in a sense subjective), independently whether this lack of predictability is due to sensitive dependence on initial conditions or not. The real issue here is that the law of nature we deal with is deterministic rather than probabilistic. After all it is well known that 'chaotic' (not synonymous with random!) behavior is fully compatible with deterministic laws. Thus the above issue is not identical to what is called "uncertainty", e.g. *Within classical physics, with their deterministic and precisely known laws, the evolution of many systems is nevertheless uncertain because these laws are chaotic,* Palmer and Hardaker (2011).

In discussing the nature of turbulence an important distinction must be made between the nature of the phenomenon and of the problem itself. Indeed, one can assign statistical nature to a theory which is then qualified as statistical hydrodynamics claimed by many as synonymous to a theory of turbulence or even mathematical turbulence theory, which is not, - we repeat again - some aspects of turbulence can be treated via statistical methods, but it is a physical phenomenon which cannot be qualified as having a purely statistical nature. In other words, physical quantities, relations between them such as physical laws and alike are not synonymous to their statistical "images" and human interpretations.

In fact, the "statistical nature" is introduced into the problem of turbulence at the outset "by hand" as the NSE are not stochastic PDE, they are purely deterministic. This is done in different ways and different motivations. For example, just by claiming

the necessity due to observations and treating the variables as random, e.g. *It is natural to assume that in a turbulent flow...the...fluid dynamic variables, will be random fields,* Monin and Yaglom (1971, p. 214), and studying from the outset, for example, probabilistic measures in some function space that are supported on Navier-Stokes solution, statistical solutions of the Navier–Stokes equations, etc. (Foiaş et al. (2001), Monin and Yaglom (1971), Vishik and Fursikov (1988). Another approach is replacing NSE by stochastic NSE, i.e. introducing a stochastic random force in the RHS of NSE both in theoretical physics, mathematics and computations with the belief that the SNSE with some "properly chosen" (!) stochastic forcing are equivalent to some problems of pure NSE with numerous attempts to mimic the behavior of real turbulence in such a way. However, there is hard evidence that different forcing of NSE - even in large scales only - results in different outcomes unless with the random force is properly chosen as in, e.g. RNG theories, see Biferale et al. (2004) and references therein, there is an issue of choice of not only random forcing in DNS. An obvious counter-example is the above mentioned stochastic forcing/excitation of integrable equations, (e.g. Burgers, Korteveg de Vries, restricted Euler, etc.,) which without such forcing do not exhibit any "stochastic" behavior whatsoever. More generally random behavior of nonlinear systems as a response to random forcing is not necessarily (if at all) turbulence and is an example among a multitude of qualitatively different phenomena – from Hamiltonian chaos to dissipative systems – all of which are qualified as "stochastic behavior".

We stress again that the statistical descriptions of the phenomenon of turbulence are not synonymous with the "statistical nature" of both the phenomenon and of the problem. Both are different from each other and by no means are not exhausted by the term "statistical".

Another important distinction is between statistical theories and the statistical methods of description and interpretation of the data from laboratory, the field and numerical experiments. For example, in the former ad hoc assumptions are made (in one way or another) that a kind of low-dimensional description is possible with corresponding hypotheses mostly of unknown validity and obscured physical and mathematical justification about modeling/parametrization of the unresolved scales in terms of the resolved ones. As concerns the latter having fully resolved data from experiments and/or DNS one can get access to such key small-scale quantities as the velocity derivatives (strain, vorticity, their production, etc.) providing the possibility of studying the rotational and dissipative nature of turbulence, along with other important quantities (e.g. fluid particles accelerations) for the fundamental physics of the whole flow field rather than large or small scales only. This flow decomposition is to a large extent artificial (and in some sense even unphysical) due to strong coupling between the two, the ambiguity of the very term 'scale' and the problematic nature of the decomposition approach to the phenomenon of turbulence and the necessity to handle turbulence as a whole.

Among the bottom lines to be remembered is that physical quantities, relations between them such as physical laws and alike are not synonymous to their statistical "images" and our interpretations, especially, with all the regret of absense of (almost) other approaches.

5.2 Limitations of Statistical Methods

> *To sum up: statistical approaches are not wrong, or a dead end; they simply have their limitations, like all other approaches, and must be used judiciously...If statistical approaches are to be used, they must to be used creatively.* Lumley (1990) p. 56.

One of the fundamental problems of turbulence research is that we are forced to use statistical methods in one sense/way or another. As mentioned all of them have inherent limitations the most acute reflected in the inability of all theoretical attempts to create a rigorous theory along with other inherent limitations of statistical methods of handling data such as those concerning validation of "theories", interpretation of observational evidence and interpretational abuse.

5.2.1 Statistical Theories

As mentioned the problem was recognized by many outstanding people, see Chap. 11 Appendix Sect. 11.1 On absence of genuine theory. We must remember the two great probabilists Wiener (1938) and Kolmogorov (1985) who admitted that it is not clear how one must rigorously treat turbulence using probabilistic approaches, for more see Sect. 3.2 in Tsinober (2009):

> *It has been realized since the beginning that the problem of turbulence is a statistical problem; that is a problem in which we study instead of the motion of a given system, the distribution of motions in a family of systems . . . It has not, however, been adequately realized just what has to be assumed in a statistical theory of turbulence*, Wiener (1938).
>
> *From the very beginning it was clear that the theory of random functions of many variables (random fields), whose development only started at that time, must be the underlying mathematical technique... I soon understood that there was little hope of developing a pure, closed theory, and because of absence of such a theory the investigation must be based on hypotheses obtained on processing experimental data*, Kolmogorov (1985), see Tikhomirov (1991 p. 487).
>
> We add one more
>
> *The statistical-mechanical treatment of turbulence is made questionable by strong nonlinearity and strong disequilibrium that result in the creation of ordered structures imbedded in disorder* (Kraichnan and Chen 1989).

Note that all three claim that *the problem of turbulence is a statistical problem* and state that there is no such a theory in existence so that *the investigation must be based on hypotheses obtained on processing experimental data.*

The reason for the not optimistic second part of all three of the above statements is most probably, that as in other theoretical approaches the beautiful probabilistic instruments (probability as a measure, etc.) produced by Kolmogorov (1933) following the proposal by Borel (1909) appeared to be not instrumental for realization concerning turbulence. Nevertheless, the mathematical community insist that, e.g. such a theory should be based on the Navier-Stokes equations and their invariant

measures and ergodic properties. Note the plural, which by itself is a problem as there exist infinitely many and the choice is not a trivial matter as no one knows which one describes the real turbulence, Ruelle (1979) and references therein. Along with this problem there are several others. The general one is that it is not certain that a purely statistical rigorous approach if possible will be effective in solving the key physical problems - on the contrary, it seems that progress with the latter will aid in progress with the former.

The statements by Wiener and Kolmogorov were followed by the community in recognition that whatever the nature of the phenomenon and of the problem the intricacy and complex behavior of turbulence enforces the use of statistical methods. This became a common view throughout the community and also of necessity also concerning the theory of turbulence whatever this means, e.g. *The theory of turbulence by its very nature cannot be other than statistical, i.e., an individual description of the fields of velocity, pressure, temperature and other characteristics of turbulent flow is in principle impossible* and unavoidable *study of specific statistical laws, inherent in phenomena en masse,*[3] *i.e., in large ensembles of similar objects,* Monin and Yaglom (1971) p. 3.

Just as in statistical physics, the technical reason that the statistical approach should be adopted from the outset in turbulence not only in 'theories', but also in handling the data from physical and numerical experiments. In both cases certain statistical hypotheses are made. But the former was quite successful in making a number of important predictions. Whereas the latter, with few exceptions, such as the Kolmogorov four-fifths law, Kolmogorov (1991b), was unable to produce genuine predictions based on the first principles and did not result in any substantial progress of a fundamental nature. All the rest - with all respect to the statistics - are at best postdictions and mimicking *after the experiments done*, Hoyle (1957). Apart from the above-mentioned reasons for such failure it should be mentioned that, unlike in statistical physics, in turbulence neither 'simple objects', (such that a collection of these objects would adequately represent turbulent flows - perhaps the basic nonlinear objects of turbulence, if such exist and can be identified - 'to do statistical mechanics' with them), nor 'right' statistical hypotheses have so far been found enabling. e.g. the "solution of the closure problem". The question about the very existence of both remains open. Nevertheless, a major theoretical effort in statistical theories was made using various ad hoc assumptions of unknown validity and obscured physical and mathematical justification mainly on the relation of the small-scale structure with the rest of the flow. All this efforts were motivated by the natural tendency to simplify the problem which is manifested in numerous searches for a reduced/low dimensional description of turbulent flows. As mentioned the whole issue is closely related to the problem of decomposition/representation of turbulent flows. From the fundamental point of view it is not obvious that such a reduced adequate description does exist

[3]The *en masse* comes from the analogy with statistical physics. But there one has literally many similar objects - molecules. So one realization there may well suffice either, see below.

and/or is possible at all[4] because, e.g. it is missing the essential physics contained in small scales and associated with such fundamental properties of turbulence as being a rotational and dissipative phenomenon, see Chaps. 7 and 8 and Chap. 3 in Tsinober (2009).

Among the essential differences (apart from those mentioned in Monin and Yaglom (1971), pp. 4–5) it should be mentioned that the small scales in turbulence (i) are very far from being simple as objects used in statistical mechanics and (ii) and interact non-trivially, bidirectionally and non-locally, with the rest of the flow, i.e. contrary to the common view, the small scales cannot be seen as a kind of passive sink of energy and/or as 'slaved' to the large scales – the small scales react back in a nonlocal manner (iii) and carry lots of the essential basic physics of turbulence such as its inherently rotational and dissipative nature. For these reasons the following rather popular expectation appears to be conceptually incorrect: *It is both reasonable and realistic to expect that the removal of small scale and high-frequency components of the dynamics of a flow be described in terms of modified molecular transport coefficients like an eddy viscosity*, Orszag et al. (1993).

The eddy viscosity is one of the oldest and greatest analogies/misconceptions in the sense that it 'explains the enhanced transfer rates', etc. whereas it is just an empirical way of accounting for such rates, but not at all an explanation in any sense. Moreover, it is doubtful that any model except (hopefully) the NSE can be used to adequately study the physics of turbulent flows which in the first place means its basic/fundamental and conceptual aspects. Remembering that *Perhaps the biggest fallacy about turbulence is that it can be reliably described (statistically) by a system of equations which is far easier to solve than the full time-dependent three-dimensional Navier–Stokes equations*, Bradshaw (1994).

In other words, the consequence of insisting on statistical theory as the only possibility in the theory of turbulence leads to the necessity of a low-dimensional description with *the removal of the small-scale and high-frequency components of the dynamics of a flow* including quantities containing a great deal of the fundamental physics of the whole flow field such as the rotational and dissipative nature of turbulence among others. Thus, relying on statistical methods only (again with all respect) one is inevitably in danger of losing/missing essential aspects of the basic physics of turbulence, see Chaps. 7 and 8.

The problem seems to be even more complicated since turbulence – being studied by all kinds of statistical methods of description – cannot be considered as just a problem of statistical physics/mechanics. There is no effective/satisfactory theoretical framework to handle turbulence – nothing new: this was stated by von Neumann (1949), though there are claims that turbulence can be seen also (but only in part!) as a problem of nonequilibrium statistical physics or whatever. For example, most

[4] *The basic question* (which usually is not asked) *concerning statistical description is whether such complex behavior permits a closed representation that is simple enough to be tractable and insightful but powerful enough to be faithful to the essential dynamics.* Kraichnan and Chen (1989).

The problem is that in such an approach the rotational and dissipative aspects are **not** considered as belonging to *the essential dynamics.*

turbulent flows are only partly turbulent with the coexistence of regions with laminar and turbulent states in the same flow.

There are inherent limitations in any theory employing statistical methods as handling quantities like probabilities because probabilistic approaches avoid the details of time evolution (i.e. dynamics) and structure. Indeed, the fluid flow is described by a trajectory in phase space. It is obviously not described adequately by only the corresponding probability distributions and similar objects. Having the latter only, (which is what one gets using statistical methods) reflects, at least in part, our ignorance of the trajectories and the information associated with it such as the dynamic and evolutionary aspects/details and structure of turbulent flows, from relatively simple to the fine-scale structure of attractors, Guckenheimer (1986). In other words, statistical methods aim from the outset and allow to obtain only partial information, e.g. Monin and Yaglom (1971) pp. 8 and 217. This is of special concern due to the troublesome question of whether it is possible to penetrate the fundamental physics of turbulence via statistics only. All this even if the aim as formulated by Orszag (1977) would be fully fulfilled: *Ideally, the objective of analytical turbulence theory is the exact calculation of all statistical properties of turbulence.*

There is an essential difference between the enforced need to employ statistical methods in view of the absence of other methods and the impossibility **in principle** to study turbulence via other approaches. This especially discourages all attempts to get into **more than just an "en masse" approach**. There are many "however" such as the issue of dynamical significance versus statistical predominance: there are important aspects where the 'majority' is less relevant than the dynamically significant 'minority', see below. Also such a standpoint means that there is not much to be expected concerning revealing the essence of turbulence using statistical methods.

Another important aspect is that the use of statistical methods - together with excessive (over)generalizations such as functional analysis introducing objects with unknown relevance to real physical objects and unclear physical meaning - brings inherent problems in the interpretation of experimental data. The issue of difficulties of interpretation of statistical objects introduced by humans is not new, see e.g. Orszag (1977). In particular, data from statistics only can be misused and misleading, so that there is the difficult issue of (mis)interpretation, especially if the information is not in physical space, e.g. Fourier or any other decomposition. Here the question is also about what kind of statistics one has to use. It is directly related to the most difficult question on what the problems are, i.e., to the skill/art to ask the right and correctly posed questions. This is quite a problem in turbulence research.

It is for the above reasons that (meanwhile) the concern here is not with statistical theories of whatever nature as these so far have not been useful in handling the fundamental issues: our concern is mostly much less ambitious – the focus is on statistical methods of description and interpretation of the data from the laboratory, the field and from numerical experiments on turbulent flows via appropriate processing of the data. The latter is likely to be a prerequisite for any worthy 'theory' of turbulence.

5.2.2 *Statistical Methods of Description and Interpretation of the Data from Laboratory, Field and Numerical Experiments*

There are also limitations on the statistical methods of description and interpretation of the data from laboratory, field and numerical experiments with each particular statistical tool having its own limitations. Well known examples are represented by relatively simple quantities like means, correlations, spectra, and probability density functions of various quantities. These tools like means and correlations smooth out some important qualitative features of (typical) individual realizations. The 'mean fields', e.g., large-scale averages of velocity or concentration of some species or particles, are smooth whereas the individual realizations are not. They are corrugated, highly intermittent and contain clusters/regions of high levels of some quantities (enstrophy, dissipation, passive tracer, reacting species, particles, etc.) surrounded by low level 'voids' of this quantity. In other words, 'standard' 'traditional' statistical methods to a large extent ignore the structure of turbulent flows, which was the main reason for the numerous objections against statistical methods often understood as averaging only. More subtle statistical properties of turbulent flows associated with their structure both in small and large scales are important in many applications.

For instance, special information on small-scale structure is needed in problems concerning, e.g., combustion, disperse multiphase flow, mixing, cavitation, turbulent flows with chemical reactions, some environmental problems, generation and propagation of sound and light in turbulent environments, and some special problems in blood flow related to such phenomena as hemolysis and thrombosis. In such problems, not only special statistical properties are of importance like those describing the behavior of the smallest scales of turbulence, but also actual 'nonstatistical' features like maximal concentrations in such systems as an explosive gas which should be held below the ignition threshold, some species in chemical reactions, concentrations of a gas with strong dependence of its molecular weight on concentration (such as hydrogen fluoride used in various industries, e.g., in production of unleaded petrol) and toxic gases. Similarly, problems such as the manipulation (and possibly control) of turbulence and turbulence induced noise require information on large-scale structure of turbulent flows far beyond such simple statistical characteristics as averages, correlations, spectra and PDFs.

One of the general claims is that *the only possibility in the theory of turbulence is statistical description, based on the study of specific statistical laws, inherent in phenomena en masse, i.e., in large ensembles of similar objects* and that *an individual description of the fields of velocity, pressure, temperature and other characteristics of turbulent flow is in principle impossible*, Monin and Yaglom (1971) p. 3. If so it is by itself a severe and pretty problematic limitation since in many cases one has to look not only at the properties of turbulent flows *en masse*, but also of special interest is prediction of the behavior of particular fields and specific properties of individual realizations like those involved in weather forecasting.

> *Investigators who prefer to look upon turbulence as a stochastic process may be interested in predicting the future statistical properties of developing or decaying turbulence, or simply in determining the statistical properties of stationary turbulence. At the same time they may have little interest in predicting future states of particular realizations. Indeed, it is likely to be some assumed basic unpredictability of individual fields of turbulent motion which has made the application of stochastic theory attractive to these investigators. There are nevertheless some (look for other examples with a single realization!!!) instances where prediction of the behavior of particular fields of turbulent motion is of considerable interest and importance. This is notably true in the case of weather forecasting. The atmosphere is, after all, a turbulent fluid; the migratory cyclones and anticyclones which bring us much of our weather are among the more conspicuous turbulent elements.* Lorenz (1972) p. 195.

The above is an extreme example of the dynamical significance of 'statistical minority.' After all one does not need ensemble averaging to be sure that the coffee will be well mixed via only one and pretty short realization. Likewise, not much can be done statistical-wise to cope with a destructive hurricane or a tropical cyclone.

> *It may also be that such (i.e. very rare and exceptionally strong) events are rather sensitive to details of the physics that do not appreciably affect the character of the majority of events. This does not mean that one should not keep trying, by insight and discernment, to discover useful statistical measures, but rather that statistics will have to be used with that humility and appreciation of the combination of admission of ignorance and decision to ignore detail so successfully used by workers in the past,* Mollo-Christensen (1973).

The problematic aspects of the claim that *the only possibility in the theory of turbulence* is the *study of specific statistical laws, inherent in phenomena en masse, i.e., in large ensembles of similar objects* are seen from the emergence of apparently random behavior in a system described by a purely deterministic system such as NSE in **one** realization**.** In particular, some essential aspects of important physics of all these *similar objects* may be lost in handling them *en masse.* The issue is well familiar to experimentalists who routinely use long enough single realizations to study various statistically stationary turbulent flows, or single snapshots of homogeneous flows in DNS on the basis of the ergodicity hypothesis, see below, which in a way eliminates the necessity to study the problem *en masse:* one "good" realization/or snapshot may be (and in many cases is) good enough. Figure 6.1 below contains a clear indication how much can be lost as concerns time evolution looking just at the statistics.

5.2.3 On Particular Tools - Examples

Each particular statistical tool has its own limitations; being useful in one context/respect, it may say nothing in many others or may be even misleading. A typical example is correlation, widely used in many aspects of turbulence research. Usually if a correlation between two quantities is not small, it reflects some important relation. This kind of behavior is mostly adequate, e.g. in reflecting the correspondence between statistical predominance and the dynamical relevance. However, there is a multitude of examples and phenomena in which *statistical predominance does not*

correspond to the dynamic relevance/importance of the relation in question leading to the misinterpretation of observations. This issue is among most problematic.

A bit more general interpretational aspect concerns the use of the energy balance equation. The nonlinear term in NSE is $(\mathbf{u} \cdot \nabla)\mathbf{u} + \nabla p \equiv \boldsymbol{\omega} \times \boldsymbol{u} + \nabla(p + u^2/2)$. An important detail is that only the term $\mathbf{u} \cdot \nabla(p + u^2/2)$ remains in the equation of energy, since the term $\mathbf{u} \cdot (\boldsymbol{\omega} \times \boldsymbol{u}) \equiv 0$ vanishes locally! That is there is no contribution to the energy balance from the Lamb vector $\boldsymbol{\omega} \times \boldsymbol{u}$ which is in the heart of any turbulent motion. This is true of the energy balance equation in any turbulent flow **locally**, in contrast to the NSE in which only $\langle \boldsymbol{\omega} \times \boldsymbol{u} \rangle = 0$ and only for homogeneous flows. Among other possible consequences the above calls for cauton in making excessive/hasty conclusions from the statistics **based solely on the energy balance equation**! So one is tempted to ask, e.g. whether the Lamb vector has anything to do with many of the conclusions drawn from the statistical processing and interpretations **based solely** on the energy balance equation.

Purely kinematic/statistical effects A general feature of the Fourier representation Orszag (1977), Waleffe (1992), and the general kinematic effect of "narrow banding" leading to Gaussian statistics in the narrow bands of modes whatever the departure from Gaussian statistics in physical space. Moreover, *With a large number of degrees of freedom, a large class of plausible phase-space distributions have low-order moments that are indistinguishable from the low-order moments of the Gaussian distribution. This result is just a special case of the central limit theorem* (Orszag 1977). See also Lumley (1970), pp. 50 and 91 and Lumley (1972), Leonov and Shiryaev (1960).

One-point statistics - simple examples Indeed, if the correlation is small, it is not necessarily insignificant. For instance, let us have a look at the famous Reynolds stress $\langle u_1 u_2 \rangle$ – the correlation between the velocity fluctuations in the direction of the mean flow (x_1) and those normal to the wall (x_2) in a wall-bounded turbulent flow. The typical value of the corresponding correlation coefficient is $\langle u_1 u_2 \rangle / u_1^{'} u_2^{'} \sim 0.4$. However, the real quantity entering the equation for the mean flow is the derivative $d \langle u_1 u_2 \rangle / dx_2$. In a developed turbulent flow with its mean properties independent of the streamwise coordinate, x_1, (flat channel, pipe), $d \langle u_1 u_2 \rangle / dx_2 = \langle (\boldsymbol{\omega} \times \boldsymbol{u})_1 \rangle \equiv \langle \omega_2 u_3 \rangle - \langle \omega_3 u_2 \rangle$. That is the 'turbulent force' is due to the coupling between large and small scales, i.e. nonlocality, see Chap. 7. The corresponding correlation coefficients between velocity and vorticity are small: both $\langle \omega_2 u_3 \rangle$ and $\langle \omega_3 u_2 \rangle$ are of order 10^{-2} or smaller. However, this does not mean that the coupling between $\boldsymbol{\omega}$ and $\mathbf{u}$ is insignificant. Indeed, without such a coupling $d \langle uv \rangle / dx_2 = 0$, so that the mean flow would not 'know' anything about turbulent fluctuations at all and therefore would remain as the laminar one. In this context a flow model with a mean homogeneous shear of infinite extent is pretty problematic. This is an example of more general problems with theoretical idealizations.

Moreover, even if a correlation between two quantities is very small or even precisely vanish, this still does not necessarily mean that the interrelation/coupling between these two quantities is not existing or is unimportant. For example, in homogeneous turbulent flows, velocity and vorticity, and vorticity and the rate of strain

tensor are precisely uncorrelated, $\langle \boldsymbol{\omega} \times \boldsymbol{u} \rangle \equiv 0$, $\langle \omega_i s_{ij} \rangle \equiv 0$, but their interaction is in the heart of the physics of any turbulent flow! Similarly, the correlation coefficient between $\mathbf{u}$ and $\nabla^2 \mathbf{u}$ is very small ($\sim Re^{-1/4}$) in high Reynolds number flows, but is very significant as directly related to the rate of dissipation of energy in turbulent flows. It is also noteworthy that the above correlations are ≈ 0 in quasi (or locally)-homogeneous flows.

One more example of the limited value of quantities like correlations and correlation coefficients is provided by a helically forced turbulent flow, Galanti Tsinober (2006). In such a flow correlations between $\mathbf{u}$ and $\boldsymbol{\omega}$, and also $\boldsymbol{\omega}$ and $curl\boldsymbol{\omega}$ do not vanish due to lack of reflectional symmetry. Nevertheless these correlations are an order of magnitude smaller than those between $\mathbf{u}$ and $curl\boldsymbol{\omega}$, and $\boldsymbol{\omega}$ and $curlcurl\boldsymbol{\omega}$. This is in spite of the fact that the scales of $\mathbf{u}$ and $\boldsymbol{\omega}$ are 'closer' than those of $\mathbf{u}$ and $curl\boldsymbol{\omega}$ in the sense that the characteristic scales of $\mathbf{u}$ and $\boldsymbol{\omega}$ differ less that those of $\mathbf{u}$ and $curl\boldsymbol{\omega}$. Moreover, in flows with reflectional symmetry the correlation coefficients between $\mathbf{u}$ and $\boldsymbol{\omega}$, and $\boldsymbol{\omega}$ and $curl\boldsymbol{\omega}$ vanish, whereas correlations between $\mathbf{u}$ and $curl\boldsymbol{\omega}$, and $\boldsymbol{\omega}$ and $curlcurl\boldsymbol{\omega}$ remain practically unchanged. The latter is directly related to the rate of dissipation of energy in turbulent flows as, e.g. in homogeneous flows $\langle \mathbf{u} \cdot curl\boldsymbol{\omega} \rangle = -2\langle s_{ij} s_{ij} \rangle$.

Single point statistics in many cases may be and usually are insufficient and may even be misleading. For example, single point PDFs of velocity fluctuations are known to be quite close to the Gaussian distribution. In particular, the third moment of velocity fluctuations is close to zero, more precisely its skeweness, $\langle u_1^3 \rangle / \langle u_1^2 \rangle^{3/2} \approx 0$, and the flatness, $\langle u_1^4 \rangle / \langle u_1^2 \rangle^2 \approx 3$, as in a Gaussian field. Similarly other higher order odd moments are small, and even moments assume values close to those of a Gaussian field, e.g. $\langle u_1^6 \rangle / \langle u_1^2 \rangle^3 \approx 15$. However, the conclusion that velocity fluctuations are really almost Gaussian is a misconception, not to mention the field of velocity derivatives. This is already seen when one looks at two-point statistics. For instance, in such a case the odd moments are significantly different from zero, e.g. Frenkiel et al. (1979). This is one of the simplest among numerous examples when multipoint in space and time statistics is useful, which in turn can be problematic at a more sophisticated level, as will be seen below.

One-Point Statistics - Less Simple Examples. The Issue of Statistical Predominance Versus the Dynamical Relevance/Importance

I. Sweeping decorrelation hypothesis. An example of special interest concerns the so called sweeping decorrelation hypothesis called also the random Taylor hypothesis:

> *Kolmogorov's basic assumption (Kolmogorov 1941) is essentially that the internal dynamics of the sufficiently fine-scale structure (in x-space) at high Reynolds numbers should be independent of the large-scale motion. The latter should, in effect, merely convect, bodily regions small compared to the macro scale* (Kraichnan 1959, *p. 536). This is what is called sweeping which is claimed to have purely kinematic nature, and the claims that it preserves the shapes of the advected small scale eddies - this is why Lagrangian aspects are of importance - and thus has no effect on the turbulence energy spectrum in the Eulerian frame); An underlying assumption of Kolmogorov theory is that very large spatial scales of motion*

convect very small scales without directly causing significant internal distortion of the small scales. The assumption usually is considered to be consistent with, and to imply, statistical independence of small and large scales, Kraichnan (1964) p. 1723.

This hypothesis is a reflection of a simplest heuristic decomposition of the flow field on large and small scales. The consequence is that, following Tennekes (1975), at large Reynolds numbers one can assume that *Taylor's "frozen-turbulence"*, random Taylor hypothesis - RTH, *approximation should be valid for the analysis of the consequences of large-scale advection of the turbulent microstructure and that the microstructure is statistically independent of the energy containing eddies*[5] so that the fluid particle acceleration as assumed to vanish $\mathbf{a} = 0$ which is true if the sweeping is purely kinematic. On the other hand, the equality $\mathbf{a} = 0$ cannot and should not be understood literally because otherwise one is led to absurd consequences. First, $\mathbf{a} = 0$ brings turbulence out of existence. Second, $\mathbf{a} = 0$ would mean that both $-\rho^{-1}\nabla p + \nu\nabla^2 u = 0$ and $\partial\mathbf{u}/\partial t + (\mathbf{u}\cdot\nabla)\mathbf{u} = 0$ are valid, which is wrong even as an "approximation". However, it is true that in some sense $\mathbf{a} \approx 0$, i.e. $\mathbf{a}$ is small compared both to the local acceleration, $\mathbf{a}_l = \partial\mathbf{u}/\partial t$ and $\mathbf{a}_c = (\mathbf{u}\cdot\nabla)\mathbf{u}$, e.g., $\langle a^2\rangle/\langle a_l^2\rangle \ll 1$ and $\langle a^2\rangle/\langle a_c^2\rangle \ll 1$. This in turn is possible if there is mutual (statistical) cancellation between $\mathbf{a}_l$ and $\mathbf{a}_c$. Since these quantities are vectors, the degree of this mutual cancellation should be studied both in terms of their magnitude and the geometry of vector alignments. Indeed, both in terms of magnitude, the total acceleration $\mathbf{a}$ is much smaller than its local and convective components $\mathbf{a}_l$ and $\mathbf{a}_c$, and that there is a strong anti-alignment between $\mathbf{a}_l$ and $\mathbf{a}_c$, see Fig. 5.1

An important observation here is very large in magnitude values of correlation between $\mathbf{a}_l = \partial\mathbf{u}/\partial t$ and $\mathbf{a}_c = (\mathbf{u}\cdot\nabla)\mathbf{u}$, see Fig. 5.1b.

In other words the approximation $\mathbf{a} \approx 0$ is very good and becomes better with increasing Reynolds numbers. This is true also of the validity of the Random Taylor Hypothesis (RTH) or sweeping decorrelation hypothesis (SDH). The important point is that though both are *approximately* kinematic, this - as mentioned above - does not mean that the non-kinematic "small difference" justifies the validity of the equations $\partial\mathbf{u}/\partial t + (\mathbf{u}\cdot\nabla)\mathbf{u} = 0$ and $-\rho^{-1}\nabla p + \nu\nabla^2 u = 0$. *It is this "small non-kinematic difference" which is mostly responsible for all the dynamics in the Eulerian representation and that sweeping cannot be considered as just a kinematic effect. The dynamics involved is of utmost importance as there is no turbulence without the above mentioned "small difference"!* This is because in the Kraichnan/Tennekes 'decomposition' there are two ingredients in the Eulerian decorrelation: (i) - the sweeping of microstructure by the large scale motions and associated kinematic nonlocality, (ii)- and the local straining, which is roughly pure Lagrangian. It appears that this kind of 'decomposition' is insufficient as it is missing an essential dynamical aspect - the interaction between the two. The random Taylor hypothesis, and, of course, the conventional Taylor hypothesis, lack/discard this aspect at the outset, which does not mean that these hypotheses are useless, but both are 'too kinematic'. It is

[5] The experimental evidence points to the opposite: the microstructure is not statistically independent of and even not decorrelated from the energy containing eddies, see below, Chaps. 7 and 8 below and Chap. 6 in Tsinober (2009) and references therein.

Fig. 5.1 Left - PDFs of the cosine of the angle between a_l and a_c. The insets show this dependence with the vertical in log and in the proximity of $cos(\mathbf{a}_l, \mathbf{a}_c) \approx -1$. This alignment was observed first in DNS shown at the left, Tsinober et al. (2001), and also in laboratory experiments, Lüthi et al. (2005) and in the atmospheric surface layer Gulitskii et al. (2007a), (2007b), (2007c). Right - Correlation coefficients between $\mathbf{a}_l$ and $\mathbf{a}_c$, and $\mathbf{a}_l$ and $\mathbf{a}_{cs}$, DNS, Tsinober (2001), Tsinober et al. (2001). The latter is the solenoidal part of $\mathbf{a}_c$. Similar results for $\mathbf{a}_l$ and $\mathbf{a}_c$ obtained in laboratory experiments, Lüthi et al. (2005) and in the atmospheric surface layer Gulitskii et al. (2007b)

noteworthy that the efforts on *elimination of the sweeping interactions from theories of hydrodynamic turbulence* are continuing, Gkioulekas (2007).

The bottom line: in the context of the discussion on correlations the above results show that very large correlations may be of little dynamic significance as contrasted to small counterparts of utmost importance: the decomposition on large and small scales appears "too kinematic" as it is missing an essential dynamical aspect – the direct and bidirectional interaction between the two.

II. Geometrical statistics - alignments of vorticity ! and the eigenframe $\breve{}_i$ of the rate of strain s_{ij} and related The example below exemplifies in a different context the issue of statistical predominance versus the dynamical relevance. Namely, it shows that the former not necessarily corresponds to the dynamical relevance of the observed relations. This is seen from recent examples which concern the use of averaging of turbulent flow fields represented in the local coordinate system defined by the eigenvectors of the strain rate tensor at each point, Elsinga and Marusic (2010), also Wei et al. (2014) and references therein. Calculating the average fluctuating velocity field on a grid aligned with eigenframe of the strain revealed a shear layer at 45^0 with the most stretching and compressing eigendirections, which separates two larger-scale flow with regions of opposing velocity directions and vorticity associated with this shear layer preferentially oriented in the intermediate eigen-direction. The claim is that such *organization of the small-scale motions is not only found in the*

average patterns, but is also frequently observed in the instantaneous velocity fields of the different turbulent flows and that *the presented average pattern in the strain rate eigenframe is representative of a general frequently occurring flow pattern*. However, the problem is that all the evidence as presented by the authors (and some others) on the instantaneous fields is different from the above mentioned average, though with preferential alignment of vorticity and the intermediate eigen-direction. Moreover, though this alignment is statistically predominant it is not most dynamically relevant as, e.g. *the flow patterns associated mostly with the alignment of vorticity and the largest positive eigen-direction contribute most to the enstrophy production!* That is at best it is not clear in what sense the above mentioned average is *representative of a general frequently occurring* instantaneous *flow pattern* if at all. Thus the approach of the authors is suspicious to what can be called overprocessing of the data by "exotic" averaging.

The above is a part of a broader issue on analogies and the role of kinematics, e.g. *when Re is large this vorticity is virtually frozen into the fluid, p. 154. However, since material-line stretching seems to be a norm for the broader class of kinematically-admissible fields, it should also be the norm for the narrower class of dynamically-admissible velocity fields, and so one should not be surprised that vortex-line stretching, like material-line stretching, is seen in practice.* Davidson (2004) p. 259. We add that the predominant alignment for material lines is, indeed, with the largest positive eigen-direction of strain whereas vorticity aligns predominantly with the intermediate eigen-direction, but the most of the enstrophy production is associated with alignment of vorticity and the *largest* positive eigen-direction, for more on this and related see Chap. 9 on analogies and misconceptions in Tsinober (2009) and Chap. 7 below.

Finally, apart from the two examples above the issue on statistical predominance versus the dynamic relevance/importance is far broader and include various statistical balances for production of enstrophy and strain, balances in approximately stationary flows, Tenekes and Lumley balances, to mention a few. We will encounter some of these below.

Two-Point Statistics and Related

> *The question whether fluctuations of ε should be reflected in the form of the correlation functions in the inertial range with certainty until we have a consistent theory of turbulence.* Landau and Lifshits (1987).

> *Most problems in classical stochastic processes are reduced to solubility by statistical independence, or the assumption of a normal distribution (which is equivalent) or some other stochastic model; because of the governing differential equations, the turbulent velocity at two space-time points is, in principle, never independent – in fact, the entire dynamical behaviour is involved in the departure from statistical independence. The equations, in fact, preclude the assumption of any ad hoc model, although this is often done in the absence of a better idea.* (Lumley 1970).

One of the reasons for considering two-point statistics to be important in turbulence research is that, e.g. for some separation r and/or time the two-point correlations are related to the flow structure(s) (whatever this means) larger than $\sim r/t$ with the

belief that the contribution/impact of smaller structures is not essential (if not negligible). In passing we mention the oversimplification occurring in the approach when ascribing one scale to such creature as structure in turbulence.

This is precisely the view in an outstanding example with the two-point statistics as, e.g in K41 concerning the statistics of inertial range (IR) at large Reynolds numbers. In particular, by definition the statistical properties of IR are (hypothesized to be) independent of viscosity, which among other things means that it is of no importance how large the dissipation is at the 'points' $\mathbf{u}(\mathbf{x})$ and/or $\mathbf{u}(\mathbf{x}+\mathbf{r})$ with scale of, say, of order of Kolmogorov scale η as long as r is within the inertial range, $\eta \ll r \ll L$. This raises the questions as to whether indeed such events $\Delta\mathbf{u} = \mathbf{u}(\mathbf{x}+\mathbf{r}) - \mathbf{u}(\mathbf{x})$ (we call them strong dissipative events SDE) have no impact on the statistics of IR. The questions are prompted by a simple consideration/doubt: computing $\Delta\mathbf{u}$ one encounters also large and even very large instantaneous dissipation at the ends $\mathbf{u}(\mathbf{x}+\mathbf{r})$, $\mathbf{u}(\mathbf{x})$. In other words, the hypothesis (!) concerning the properties of IR involves a strong assumption that the SDE's do not matter for the statistics of velocity increments $\Delta\mathbf{u} = \mathbf{u}(\mathbf{x}+\mathbf{r}) - \mathbf{u}(\mathbf{x})$. In particular, one wonders about:

(i) the so called "anomalous scaling" (i.e is it LS or SS or vice versa or both?) and
(ii) whether the 4/5 law in the inertial range at large Re is a purely inertial relation at large Re, i.e. does not care about the SDE's.

We will see below in Chap. 8 that the SDE's do have an essential impact on the IR, which among other things is the consequence of the nonlocal effects on the IR. For this reason the IR is out of existence as a concept and, in particular, the 4/5 law in the "inertial range" at large Re is not a purely inertial relation either.

More generally, the two- or more- point statistics is capable of introducing nonlocal effects of "kinematical" nature. An important aspect in using two-point statistics is that it "destroys" the very basis of the cascade ideology as based on its locality.

Similar behavior is expected in other two point statistical objects with participation of strong events of various nature, e.g. just correlations as appearing in the equation by von Karman and Howarth (1938).

In other words, two-point statistics has the potential of possessing and displaying nonlocal effects (such as between the "inertial" and dissipative ranges, but not only) which must be taken into account in the process of interpretation of statistical quantities and relations. It is the right place to mention that velocity increments (let alone structure functions and their scaling if such exists) are not the only objects of interest and *do not constitute a representation basis for a flow* (Goto and Kraichnan 2004) even with the information on the statistics of all structure functions of all orders.[6] Such information even on the "simplest" structure function of the second order or the so called scale-by-scale energy balance involves nontrivial issues and difficulties of interpretational nature arising from the limited information due to using an "energy balance" equation only resulting in missing a variety of nonlocal

[6]We mention that formally there exist two closed formulations which in reality are suspect to be just a formal restatement of the Navier Stokes equations, at least, as concerns the results obtained to date. One is due to Keller and Fridman (1925) infinite chain of equations for the moments and the equivalent to this chain the equation by Hopf (1952) in term's of functional integrals.

effects, see Portela et al. (2017), Cimarelli et al. (2016), Jimenez (2018), Yasuda and Vassilicos (2018) and references therein.

A related concern arises in issues handling turbulence structure(s) as being not low-dimensional objects, which nevertheless are mostly hunted by isosurfacing and thresholding and are defined at some time moment only; moreover, they cannot be followed in time without non-trivial identification. Therefore producing statistics is performed from collections of "similar objects" obtained from snapshots at the same and different time moments. This leads to the painful question on how really "similar" are all these if they are typically defined by one parameter? It is almost obvious that such a kind of "statistical" processing (i.e. another kind of "exotic" averaging) is killing most of essential features of the real "structure" and leaves the question of, say the dynamical relevance of these "structures" at best open.

The bottom line is that with all respect, relying on statistics only such as, e.g. very strong and very weak correlations, 'exotic averaging', etc., may bring one nowhere by missing essential dynamic effects. Judicious use of statistics is more than vital in turbulence research. Otherwise it leaves too much dangerous room open for misinterpretations and "right results" for the wrong reasons. Two quotations are in place here concerning the important point that probability criteria are insufficient

> *...one can find in statistical data irrelevant structures with high probability,* Lumley (1981).

> *...you can find structures, essentially arbitrary, which have equal probability to the ones we have latched onto over the years: bursts, streaks, etc....If structures are defined as those objects which can be extracted by conditional sampling criteria, then they are everywhere one looks in turbulence,* Keefe (1990).

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Chapter 6
Additional Issues of Importance Related to the Use of Statistical Methods

Abstract One of the most serious concerns is that statistics alone can be misused and misleading due the absence of theory based on first principles and inadequate tools for handling both the problem and the phenomenon and difficult issues of (mis)interpretation, validation, oversimplification and related, especially if the information is not in physical space, e.g. Fourier or any other decomposition. Among the concerns is the issue of statistical (pre)dominance versus dynamical relevance. The statistical predominance does not necessarily corresponds to the dynamical relevance as, e.g. in the case of sweeping decorrelation hypothesis or "exotic" averaging of turbulent flow fields such as represented in the local coordinate system defined by the eigenvectors of the strain rate tensor at each point.

Similar concern arises in issues dealing with turbulence structure(s). These are finite objects which nevertheless are mostly hunted by isosurfacing and thresholding and are defined at some time moment only, moreover, they cannot be followed in time. Therefore, the producing of statistics is performed from collections of "similar objects" obtained from snapshots at the same and different time moments. But the painful question is how really "similar" are all these if they are typically defined by only one parameter? It is almost obvious that such a type of "statistical" processing, (i.e. another kind of "exotic" averaging) is killing most of the essential features of the real "structure" and also leaves the question of relevance, say the dynamical one, of these "structures" at best open. The bottom line is that with all due respect, relying on statistics alone such as, e.g. very strong and very weak correlations, 'exotic averaging', etc., may bring one nowhere by missing essential dynamical effects. Finally, a most serious complication is due to the use of partial/limited information. Among the consequences is a serious misuse and interpretational (and also terminological) *abuse of observations, which is essentially "aided" by the absence of genuine theory*. It is not unusual that in the process of validation the hard data (both from physical observations and numerical) is considered as inferior as compared to 'models' so that the former are tested' against the latter and not vice versa as is done even in fields being in possession of rigorous theories. Unfortunately, not many care about the observational evidence. The approach in reality is in some sense reversed. The widespread view of both mathematicians and theoretical physicists is that the main

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function of all experiments/observations (both physical and numerical) is to "validate theories", these are so far paradoxically nonexistent. In such a situation the issues of interpretation and validation as concerns the right results for the right reasons (or "theories" versus hard evidence) becomes of more than of utmost importance. This is because in absence of true theory based on first principles and in view of the highly dimensional nature of the problem the issue of confirmation/validation of 'theories' in turbulence is far more serious than just checking the 'validity' of some formulae or any other theoretical matter via comparison with limited experimental evidence. The latter is of special importance and includes a number of aspects.

Most of the evidence was obtained at moderate Reynolds numbers for finite non-trivial non-isotropic and non-homogeneous systems, e.g., jets which consist of coexisting turbulent–non-turbulent regions. Theoreticians claimed 'explanations' based on "infinite" objects/boxes assuming some properties such as isotropy and homogeneity. Today they say that the effects of the finite box are of special interest and importance. Hence the insistence on various kinds of locality.

To reiterate, many examples of attempted validation, etc., fall under the category of interpretational abuse of one kind or another due to use of limited experimental information evidence for different reasons. It is one of the most acute and general problems especially in the absence of a true theory based on first principles and in view of the highly dimensional nature of the problem. The issue of confirmation/validation of 'theories' in turbulence is far more serious than just checking the 'validity' of some formulae or any other theoretical matter via comparison with limited experimental evidence.

An important issue is related to ergodicity.

As mentioned statistical methods are practically the only methods employed in turbulence research be it theory (whatever this means) or experiment. Along with the high dimensional nature, very large number of strongly and nonlocally interacting degrees of freedom a number of issues take on a special significance. Following is the list of those we consider as especially important for basic research in turbulence. Each are explained mostly via short comments illustrated by way of exemplification.

6.1 Interpretation and Validation or What About the Right Results for the Right Reasons or "Theories" Versus Hard Evidence

The issues of interpretation and validation refer not just to "theories", but in the first place to the factual information and the relation between the two. Among the serious problems is that most of the "theories" claiming explanations of some specific aspects of observations in fact appear to be mere descriptions of these same particular observations or using these same observations for justifications/confirmations of these same theories. There are 'theories' – many with contradictory premises – but all agreeing well with *some* experimental data. The issue is more serious as there are

many situations in which agreement with experiment may not help too much even if the agreement between "theories" and experiment is excellent as the correspondence with the experimental results may occur for the wrong reasons as happens from time to time in the field of turbulence.

6.1.1 Interpretation

This refers not just to "theories" but in the first place to experiments. The main point here is that the right results should be interpreted and related to the right reasons at least as concerns fundamental studies. The correspondence with experimental results may (and very frequently does - judging by massive agreement between "theories" on one hand and the absence of genuine theory, on the other) occur for the wrong reasons[1] i.e. this correspondence is at best a necessary condition. One of the problems is misinterpretation, which is "aided" by the extremely complex nature of the problem and, of course, the absence of the basic theory. Another acute problem is that most of the theories claiming explanations of some specific aspects of observations are in fact mere descriptions of these same particular observations.

Before getting to specific examples we must again emphasize the general issue of the use of statistical methods together with excessive (over)generalizations such as functional analysis introducing objects with unknown relevance to real physical objects and unclear physical meaning which bring the inherent problems of the interpretation of experimental data.

The first example concerns the celebrated $k^{-5/3}$ energy spectrum which is considered as one of the basic attributes of many turbulent flows. In some sense this is correct, but there exist a multitude of phenomena not even fluid dynamical, for more examples see Tsinober (2009) Chaps. 5 and 7, possessing the same spectrum. Moreover, an extreme example is a single sharp change in velocity, which is local in physical space. Represented in Fourier space it has an energy spectrum $k^{-6/3}$ which is not so easy to distinguish from $k^{-5/3}$. This is true not only of the above particular scaling exponent,[2] but of scaling exponents generally, as there exists no one-to-one relation between simple statistical manifestations and other more subtle properties (such as the underlying structure(s) of turbulence), so that qualitatively different phenomena can and do possess the same set of scaling exponents. For example, there exist numerous models that attempt to reproduce the anomalous scaling for higher

[1] For example, *it is clear that if a result can be derived by dimensional analysis alone. . . then it can be derived by almost any theory, right or wrong, which is dimensionally-correct and uses the right variables*. Bradshaw (1994).

[2] It is noteworthy that this spectrum is not precisely the "right" one. Indeed, if one looks at the data by Grant et al. (1962), especially unpublished, but see Long (2003), the error bar is not that small as to exclude the $k^{-6/3}$ spectrum which correspond just to a single sharp change in velocity, see also Tsinober (2009) p. 334 and references therein for recent results on the "approximately" $k^{-5/3}$. Moreover, the "small" differences are essential and increase as concerns higher order quantities, derivatives and extreme/strong events.

order statistics which are based on qualitatively different premises/assumptions, but all of which are in good agreement with the same experimental and numerical evidence, see Tsinober (2009) pp. 211–216 and references therein. Likewise, similar PDFs of some quantities can correspond to qualitatively different processes, structure(s) and quantitatively different values of Reynolds numbers. The overabundance of the $k^{-5/3}$ energy spectrum and its misinterpretation contributes enormously to the misleading aspects.

Another issue is the use of some theoretical considerations frequently called "theories" for idealized homogeneous and isotropic flows (called sometimes "reasonably homogeneous and isotropic") for "interpretation" of experiments which are neither homogeneous nor isotropic and even worse: most of turbulent flows are only partly turbulent flows (PTF) with the coexistence of regions with laminar and turbulent states in the same flow, such as jets, wakes, mixing layers and some boundary layer flows. Moreover, among the main problems is the inherent property of nonlocality questioning the validity of the hypotheses on local homogeneity/isotropy in bounded flow domains (though "not lying close to the boundaries of the flow or its other special regions") *surrounded by nonhomogeneous/nonisotorpic flow regions,* see below the example of a flow in the midplane in plane channel. In particular the "quasi-homogeneous" behavior of some quantities in bounded regions is misleading due to several factors, the main being the nonlocal nature of turbulence. From the conceptual point the nonlocality makes local homogeneity, isotropy, etc. impossible unless the whole (infinite extent) flow is such, which is trivially impossible. The idealized configurations such as global homogeneity, etc. are attractive for theoretical approaches, but they are useful for systems with dominating properties of local nature. This seems not to be the case with turbulence. Hence the problem with the utility of idealized configurations, not to mention that they were not so useful in view of the absence of theory from first principles.

In other words, there is a serious misuse of experimental data.

Several examples are given below.

The first example belongs to the category of spatially developing flows, self-similarity versus true time evolution. It concerns the use experimental data in partly turbulent flows and grid turbulence. Both are evolving in space statistically stationary *inhomogeneous* flows in spatially bounded domains *with some boundary conditions*.

It should be stressed that the inflow conditions (and also outflow) are indeed boundary conditions *per se* - they are **not** initial conditions. Nevertheless, it is a common rather problematic practice to replace the flow evolution in space (in the streamwise coordinate) by the evolution in time. In particular, the grid turbulence data are mostly considered as decaying in *time* with the flow at each location x in the streamwise direction representing an imaginary statistically spatially homogenous turbulent flow in all three dimensions of infinite extent, which is obviously incorrect. Indeed, on one hand the $\partial/\partial t \neq 0$ for the imaginary three-dimensional state as it evolves, but not the x-dependence $(\partial/\partial x)$ when looking at such a state at each time moment. Moreover, the problematic aspect is not only just with the streamwise direction, but also in the other two directions influenced by the boundaries. In addition, as mentioned the "quasi-homogeneous" behavior of some quantities in

bounded regions is misleading due to several factors, the main being the nonlocal nature of turbulence, see Chap. 7. From the conceptual point the nonlocality makes local homogeneity, isotropy, etc. impossible unless the whole (infinite extent) flow is such, which is trivially impossible. In other words the "reasonably homogeneous isotropic turbulence" employed in pretty limited space in all experiments is more than just problematic especially when estimated from limited data (and Re) and quantities like a single velocity component and its time derivative, the latter being especially acute when dealing with issues involving velocity derivatives, e.g. dissipation and alike.

A second example is the so called multifractal formalism (MF), Frisch (1995), using the experimental data in turbulent flows of the kind mentioned above such as in the proximity of the centerline of a jet at moderate Reynolds numbers. The data is used to justify the MF supposed to be valid at very large Reynolds numbers and at the same time the MF is used to "explain" the data, see below.

The above extends into misinterpretations of analogies (for this and more see Tsinober (2009) Chap. 9, Analogies, Misconceptions and ill-defined concepts, such as between the genuine (e.g. NSE) turbulence and passive "turbulence", i.e. evolution of passive objects in random (or just not too simple) velocity fields. The differences are more than essential, though there are numerous claims for *the well-established phenomenological parallels between the statistical description of mixing and fluid turbulence itself* , Shraiman and Siggia (2000) which are a consequence of multiple repetition of this claim without almost any factual basis. The first example is the famous verse by Richardson (1922) p. 66 related to the cascade picture of turbulent flows: *... we find that convectional motions are hindered by the formation of small eddies resembling those due to dynamical instability. Thus C.K.M. Douglas writing of observations from aeroplanes remarks: "The upward currents of large cumuli give rise to much turbulence within, below, and around the clouds, and the structure of the clouds is often very complex". One gets a similar impression when making a drawing of a rising cumulus from a fixed point; the details change before the sketch is completed. We realize thus that: big whirls have little whirls that feed on their velocity, and little whirls have lesser whirls and so on to viscosity—in the molecular sense.*

The point is that this observation was made by looking at the structure of clouds, i.e., condensed water vapor, at the interface between laminar and turbulent flows in their bulk, which today is known as not necessarily reflecting the structure of the underlying velocity field. The analogy between genuine and passive turbulence is illusive and mostly misleading, see above Fig. 1.3, Chap. 1 and Tsinober (2009) Sects. 9.3, 9.4, 9.5.4 and references therein. In this context of special interest is a recent statement by Eyink and Frisch (2011) p. 362: *Kraichnan's model of a passive scalar advected by a white-in-time Gaussian random velocity has become a paradigm for turbulence intermittency and anomalous scaling (the authors mean the genuine NSE turbulence)... The theory of passive scalar intermittency has not yet led to a similar successful theory of intermittency in Navier-Stokes turbulence. However, the Kraichnan model has raised the scientific level of discourse in the field by providing*

a nontrivial example of a multifractal field generated by turbulence dynamics. It is no longer debatable that anomalous scaling is possible for Navier Stokes.

There are here three points of problematic nature. First, the Kraichnan model it is not a paradigm for turbulence intermittency and anomalous scaling - it is even not a paradigm for intermittency and anomalous scaling for passive objects due to use of unphysical velocity field. Second, it is not an example of multifractal model, but rather an example of anomalous scaling which are definitely not synonymous.[3] And third, it is definitely not debatable that anomalous scaling is possible for Navier Stokes, and even more the anomalous scaling **is in** the Navier Stokes as it is observed in experiments for quite a while in real systems which are described by NSE. However, the real issue is about the reasons/interpretation or even better the underlying mechanisms. The true problem is reflected by the term "anomalous scaling" as it is observed in what is called "inertial range" which in reality is an object in non-existence as not well defined due to severe contamination of the conventionally defined inertial range by strong dissipative events at whatever large Reynolds numbers, see Chap. 8. Far more can be found on misuse and misinterpretations of analogies in Chap. 9, Tsinober (2009).

An additional example of interest is by Berdichevsky et al. (1996) in which mean velocity distributions were obtained '*from the first principles*' for *turbulent* Couette and Poiseuille flows, which are in very good agreement with experimental results for real *three-dimensional flows*. The problem is that these theoretical results are based on a *two-dimensional* model lacking any essential phenomena specific for three-dimensional flows whatsoever such as self amplification of strain and vorticity.

6.1.2 Validation or 'Theories' Versus Hard Evidence

An intimately related to the issue of interpretation is the one concerning experimental validation of 'theories' understood as any theoretical treatment including modelling, understood literally in the sense of theories based on the first principles, i.e. strictly speaking there is nothing to validate. This is directly related to the question on how meaningful and in what sense is the experimental 'confirmation' and/or 'validation' of a 'theory' as the first thing one is wondering about what one is supposed to validate and/or confirm. This is primarily because in turbulence it is especially true that experimentalists observe what cannot be explained whereas theoreticians claim to explain these observations on the basis of idealizations which, however, can not be realized. This statement is far more specific than it seems as we will see througout this text.

[3] In this context it is of interest to quote Goto and Kraichnan (2004): *Multifractal models of turbulence have not been derived from the NS equation but they are supported by theoretical arguments and their parameters can be tuned to agree well with a variety of experimental measurements... Multifractal cascade models raise the general issue of distinction between what is descriptive of physical behavior and what can be used for analysis of data... Multifractal models may or may not express well the cascade physics at large but finite Reynolds numbers.*

Next we reiterate two general problems (i) that most of what are called theories claim explanations of some specific aspects of observations, while being in fact mere descriptions of these same particular observations, (ii) it is not unusual that when considering validation a frequent phenomenon is that the hard data (both from physical observations and numerical) is considered as inferior compared to 'models' so that the former are tested against the latter and not vice versa as it is done even in fields being in possession of rigorous theories.

A special issue of concern is the massive employment of limited experimental evidence. Limited in a variety of meanings. The first example is found in Monin and Yaglom (1971, p. 21): *...we have avoided introducing experimental results which have no theoretical explanation* (!) *and which do not serve as a basis for some definite theoretical deductions* (!) *, even if these data are in themselves very interesting or practically important* (!). This is because *... one of the principal incentives for writing this book* (1600 pages in two volumes) *was a desire to summarize the development of the idea of a universal local structure in any turbulent flow for sufficiently large Reynolds number.* In other words, they looked for confirmation of something very important as everybody does, but using only partial information – that fits what they think/expect of the issue. Unfortunately, just like stated by Liepmann (1979), digging a bit deeper into the observation evidence proved that what Monin and Yaglom were seeking appeared as to be problematic as all previous and subsequent ideas though far more popular. However, quoting Saffman (1978), *It must of course be kept in mind that the achievements or intrinsic value of a theory is not decided by democratic means. The majority view in science is not necessarily the right one*. Unfortunately, this stands in drastic contradiction with the modern 'culture' of evaluation such as 'ratings' and similar bureaucratic 'inventions', which have contributed significantly to the alarming overabundance and continuing major over-production of publications.

The next example stems from the highly dimensional nature of turbulence which is one of the main reasons for (and obstacles to) assessment of the conceptual validity/reliability of any theory let alone low-dimensional (LD) modeling. From a conceptual point of view the main question remains whether it is at all possible and why does it 'work'? Of course, there is a serious concern about the meaning of the term 'work'. Any LD model or a "theory" that represents a corresponding LD part/aspect of some particular kind/class of turbulent flows (but not necessarily for the right reason) will be (and usually is) inadequate when applied to other flows. Just as simple interpolation/fits polynomials, etc. describe faithfully the behavior of data without any physical reason (purely empirical/technical), so many models do precisely the same. Mostly they are postdictions (rather than predictions) and, quite often, successful and useful semi-empirical interpolation schemes. There are many theories – many with contradictory premises – but all agreeing well with *some* experimental data. The issue is more serious as there are many situations in which agreement with experiment may not help too much even if the agreement between "theories" and experiment is excellent as correspondence with experimental results may occur for the wrong reasons as happens from time to time in the field of turbulence. For example, there are quantities/properties that are insensitive/invariant to some specific properties of the flow field whether it is real or in some sense

synthetic, Gaussian/quasi-normal, Markovian, etc. For example, addressing issues associated with Gaussian/quasi-normal manifestations of turbulent flows regarding velocity field and having a perfect agreement with some theory based on quasi-gaussianity and/or quasi-normality an experimentalist in fact encounters a dilemma as to whether his measurements are perfect or just a nice Gaussian noise, unless addressing also the behavior of odd moments along with the even ones, e.g. Vedula et al. (2005) and references therein. A more elaborate recent example is found in Pumir et al. (2016) where a relation between the third moment of the rate of change of energy of a tracer particle and enstrophy production is obtained employing a number of problematic assumptions. These include among others the Gassianity of the velocity field and the "*physically justified approximation that velocity u and strain S are uncorrelated*" because "*In a turbulent flow, the velocity field u and the rate of strain S are expected to be independent of each other*" and assuming equivalence of strain production to that of vorticity on the basis of mean relations. These and other examples are a prompt to remember that "uncorrelated" is not synonymous either to "independent of each other" or to "decoupled" and the like: in basic research the right result is expected to be obtained for the right reasons.

The third example is the above mentioned multifractal formalism (MF), Frisch (1995). It is claimed to be an explanation of 'anomalous scaling' and that the multifractal model is well supported by experimental evidence. In fact, it is another description of the observed anomalous scaling, i.e. of the experimental evidence employing at best some general properties such as basic symmetries and dimensional arguments, conservation laws and some other general properties etc. as the Navier–Stokes and even Euler equations. *One of the important aspects here is that the experiential data used are all at best at moderate Reynolds numbers, whereas the MF is designed for* $Re \to \infty$. As mentioned there are numerous alternative descriptions – sometimes standing in contradiction with each other – which are also "well supported" by the experimental evidence. A notable one is called breakdown coefficients/multipliers by Novikov (1990). It is should be stressed that in all of the above one of the key symmetries - the very existence of scaling exponents in the statistical sense, e.g., in the inertial range **is assumed**, i.e. it is a hypothesis only and is taken for granted, so it is a problem by itself. In other words such models are more or less successfully mimicking the experimental observations on the anomalous scaling, the multifractal formalism being (probably) the most successful as having the most freedom – a whole range of exponents and a function - claimed to be universal – for adjustment to the experimental data - at this stage the universality is forgotten. This issue (and some other related) is even more serious as there are problems concerning the experimental evidence itself.

It is also noteworthy that such models (like the MF formalism) is in possession of considerable freedom for adjustment to the experimental data. There are many other attempts to "solve" some "equations of turbulence" by closing them using other "hidden closures", e.g. exploiting Lie group formalisms, Rosteck and Oberlack (2011), She et al. (2017) and references therein.

It seems that validation of "theories" which are not based on NSE (or more generally some other first principles - if such exist) is not that meaningful from the

fundamental point of view. The remaining are theories involving severe idealizations, which by themselves have to be tested far more carefully than is usually done. These involve assumptions of statistical homogeneity and isotropy, and other symmetries and assumptions such as self-similarity or self-preservation originated by von Karman and Howarth (1938) and subsequent different versions and related, see references in George (2012), Klewicki et al. (2014), Djenidi et al. (2017a, b). These employed other "hidden symmetries/closures", etc. possessed by the original NSE with and/or without boundary conditions and other constraints, but not necessarily statistical properties of flows in question. Hence the term "assumptions" in above.

An outstanding example of different nature is the Kolmogorov 4/5 law Kolmogorov (1941b). Its validity at high Reynolds numbers in the form $S_3 \equiv \left\langle (\Delta u)^3 \right\rangle = -4/5 \langle \epsilon \rangle r$, i.e. with negligible viscous term $VT \equiv 6\nu d \left\langle (\Delta u)^2 \right\rangle / dr$, which was interpreted in favour of existence of the inertial range. However, this common view is not acceptable, since the negligible viscous term VT in the Kolmogorov 4/5 law *does not contain all the viscous contributions*: the PDF of Δu and consequently S_3 contains a nonnegligible contribution from dissipative events which keep the 4/5 law precise - **without the dissipative events mentioned above the 4/5 law does not hold!** The bottom line is that the 4/5 law is not a purely inertial relation even at $Re_\lambda \approx 10^4$. We address this and related issues in Chap. 8. Two more examples are the Yaglom 4/3 law for the passive scalar and the Richardson pair diffusion law which are true also for any random isotropic velocity field including the Gaussian ones.

The presence of boundaries prompted Kolmogorov (1941a) to postulate restoring in some statistical sense of all the symmetries locally in space/time[4] except for one involving scaling: *...we think it rather likely that in an arbitrary turbulent flow with sufficiently large Reynolds number* $Re = \frac{LU}{\nu}$ *the hypothesis of local isotropy is realized with good approximation in sufficiently small regions* G *of the four-dimensional space* (x_1, x_2, x_3, t) *not lying close to the boundaries of the flow or its other special regions.*

It is noteworthy that though assuming sufficiently large Reynolds numbers Kolmogorov did not assume anything about the scaling properties at the outset. In order to address this issue he introduced the concept of the inertial range (IR) for which he assumed independence of the statistical flow properties of viscosity and postulated the validity of the scaling symmetry for statistical properties. This appeared to be in (approximate) agreement for second order statistics, but not for orders equal or higher than four with deviations increasing with the order. It appeared that one of the

[4]Frisch (1995) presents this in the form of his hypothesis **H1** (p. 74), but omits to mention that it is due to Kolmogorov: there is no presentation of the hypothesis of local isotropy in his book.

It is noteworthy that Kolmogorov theory in reality is based on *similarity and dimensionality* and has no connection to NSE, see e.g. Monin and Yaglom (1971), p. 21: *The great attention paid in this book to, similarity and dimensionality is also conditioned by the fact that Kolmogorov's theory of locally isotropic turbulence (which is based entirely on these methods) is given a great deal of space here.* In other words, experimental validation of Kolmogorov (1941a) theory, as all theories of this kind, has a limited value. Again, *it is clear that if a result can be derived by dimensional analysis alone. . . then it can be derived by almost any theory, right or wrong, which is dimensionally-correct and uses the right variables*, Bradshaw (1994).

main problems is that the concept of the IR is ill-posed: it is postulated and widely believed that (ideally) there is no dissipation and viscosity does not play any role in IR. However, there is hard evidence that at least some properties of the conventionally defined IR do depend on the nature of dissipation at whatever large Re and that strong dissipative events, (which live in the IR and which appear to be not so rare), make a nonnegligible impact on the behavior of traditionally inertial characteristics (such as structure functions) and are at the origin of the so-called anomalous scaling. The relation for the third order - the so called 4/5 law (Kolmogorov 1941b) - is a direct consequence of NSE, so that at high Reynolds numbers it is indeed confirmed experimentally. The experimental results for the second order and the validity of the 4/5 law enhanced the belief as concerned the higher order statistics, but not for long. As mentioned, recent experimental evidence shows *the 4/5 law is not a purely inertial relation*, see Chap. 8.

To put it simply, there is no such an object as an IR with properties independent of viscosity, so that it is meaningless to look for models explaining (not just describing - there are great many of such) the intermittency in such a nonexistent object (i.e. IR), as multifractal formalism, for more see Chap. 8 and Tsinober (2009) Chap. 5, Sect. 5.3 pp. 102–110 and Chap. 7, p. 215.

An important issue in the context of using limited experimental evidence is the Reynolds number dependence. In particular, it is disturbing that in the process of validation/interpretation the experimental evidence was obtained at moderate Reynolds numbers for finite nontrivial statistically non-homogeneous systems, including such flows as jets (which are only partially turbulent as they consist of coexisting turbulent and nonturbulent regions). Nevertheless, theoreticians claimed an 'explanation' of these same observations based on infinite statistically homogeneous objects/boxes and large Reynolds numbers $Re \gg 1$ and even $Re \to \infty$. Today they say that these same effects of finite box are of special interest. One of the latest examples of this kind is the "multifractal formalism". Another disturbing aspect of globally (and also locally) homogeneous turbulent flows is that in such flows, e.g. correlations of quantities which are in the heart of any turbulent flow like $\langle \boldsymbol{\omega} \times \mathbf{u} \rangle$ and/or $\langle (\mathbf{u} \cdot \nabla)\mathbf{u} \rangle$, $\langle \omega_j s_{ij} \rangle$ and/or $\langle (\boldsymbol{\omega} \cdot \boldsymbol{\nabla})\mathbf{u} \rangle$, $\langle s_{ik} s_{kj} \rangle$ and $\langle \partial^2 p / \partial x_i \partial x_j \rangle$ vanish. In particular, homogeneous turbulence with non-zero mean shear has constant Reynolds stresses with zero divergence, so that a field of homogeneous turbulence can have no effect on the field of mean velocity, if it stays homogeneous. In other words, generally and especially due to effects of nonlocality, one cannot apply results for globally homogeneous turbulent flows to flows which are approximately homogeneous in a bounded region of flows, and which are otherwise non-homogeneous. As mentioned the attraction of the Kolmogorov hypotheses is that they concern the local properties of any turbulence, not necessarily homogeneous or isotropic or decaying or stationary, provided the Reynolds number is large enough. The consistency of these hypotheses has been debated for some time both from the theoretical point of view, but as with other similar issues without much progress from the fundamental point, Gkioulekas (2007), Hill (2006) and references therein. Similar problems are encountered with anisotropy. One of the main problems is the inherent property of nonlocality questioning the validity of the above hypotheses on local homogeneity/isotropy in bounded flow

domains surrounded by nonhomogeneous/anisotropic flow regions, which is supported by experimental evidence, see Chaps. 7 and 8.

Returning to the problematic use of limited experimental evidence in the context of Reynolds number dependence among the latest examples is the criticism of the Kolmogorov "equilibrium assumption", see references in George (2014), Vassilicos (2015), Goto and Vassilicos (2016) and references therein among others. The main point is that Kolmogorov is blamed/ascribed, in addition to his locality hypotheses, to have made an additional assumption, e.g. *The key equilibrium assumption made by* Kolmogorov (1941b) *is that the small-scale motions* (r$\ll$ L) *evolve very quickly compared to the timescale of the overall turbulence evolution and are therefore in statistical equilibrium, The merit of Kolmogorov's work is the formulation of a theory for the case of an arbitrary turbulent flow with sufficiently large Reynolds number by introducing hypotheses of local homogeneity, local isotropy, and local stationarity*, Vassilicos (2015). The community has misinterpreted the footnote in the K41a: *Within small time intervals it is natural to consider this regime as approximately stationary even in the case, when the flow in the whole is nonstationary*"[5] as an additional missing assumption of local stationarity! *The main point here is that the limit of "small domains" and large Reynolds numbers is also a locally stationary limit!* This was discussed in details by Batchelor (1947), for more see, e.g. Hill (1997, 2002a, b), though without the term "universal equilibrium" introduced by him in Batchelor (1953). The origin of the term "non-equilibrium turbulence" seems to have recently come from the modeling community, though the term was used in different meanings for quite a while.

In any case, all the criticism of Kolmogorov 41a in the above context does not seem to be justified for at least two reasons.

First, it is concentrated on the nonexistent "equilibrium" assumption (approximately statistically stationary regime) as separate from the rest, which is the locality assumption in four dimensional space at large Reynolds numbers.

Second, until recently nobody has so far been able to achieve large enough Reynolds numbers referring only to "evidence" at pretty modest Reynolds numbers, but see Gulitskii et al. (2007a, b, c), Iyer et al. (2017). It does not seem appropriate to ascribe to Kolmogorov the "local equilibrium hypothesis" and to test it e.g."qualitatively", Goto and Vassilicos (2016) at Reynolds numbers not large enough such that at which turbulence does not have to be in any "equilibrium" whatsover. In fact it is not clear from the literature as to what is meant by "nonequilibrium turbu-

[5]This seems to be the reason why Batchelor (1953) called this statistical regime as "universal equilibrium" - a somewhat misleading term.Kolmogorov would never use the term "equilibrium" especially as in a variety of "explanations" followed his publications. He would definitely not use the "truncated" Fourier version of the so called Lin equation as did Batchelor (1953) and some later authors for the high wave numbers $T(k) = 2\nu k^2 E(k)$ - 'equilibrium range', Eq. 6.6.5, p. 126. A "small" $\partial E/\partial t$ **c**an (and mosly will) make an essential difference turning the 'parabolic' equation into an 'elliptical' one. In employing statistics a "small" difference between approximately and exactly stationary is not necessarily synonymous to 'unimportant' especially in the context of statistical predominance versus dynamical significance. Kolmogorov would also never use the Fourier transform either.

lence". Is it that almost in all turbulent flows[6] we encounter at finite Re(so far) seem to be "non-equilibrium" with a variety of other different properties resulting from boundary/inflow and initial conditions, nature/type of excitation/forcing (LS, SS, broadband etc.) and so forth as expected to be of importance in finite domains along with the NSE themselves as in any standard initial boundary value problem having some troubles with inflow boundary conditions and alike? The Kolmogorov locality hypotheses can be seen as an attempt to provide some (!) "freedom" from things like boundary/inflow and initial conditions, etc... the price being high Reynolds numbers. At present, however, there is evidence that at least in part the locality properties as assumed by Kolmogorov are not valid, see Chaps. 7 and 8 below. In particular, the "quasi-homogeneous" behavior of some quantities in bounded regions is misleading due to several factors, the main one being the nonlocal nature of turbulence. From the conceptual point the nonlocality makes local homogeneity, isotropy, etc. impossible unless the whole (infinite extent) flow is such, which is trivially impossible.

A final note is that the idealized configurations such as global homogeneity, etc. are attractive for theoretical approaches, but they are useful for systems with dominating properties of local nature. This seems not to be the case with turbulence. Hence the problem with the utility of idealized configurations, not to mention that they were not so useful in view of the absence of theory from first principles.

6.2 Ergodicity and Related

The general approach to statistical methods in theory is assumed to employ the concept of ensembles consisting of realizations of the "same" turbulent flow under nominally identical external conditions.[7] The problem is which probability should be assigned to each realization, so that the ensemble would correspond to most closely to physical reality and what is the relation between the statistics based on ensembles and those statistics obtained from space-time information from a limited number of realizations because experiments cannot usually be repeated a sufficiently large number of times for statistics large enough ensembles to be made. The ergodicity hypothesis is the tool which is used to cope with this problem.

For statistically stationary flows ergodicity is roughly the equivalence of 'true' statistical properties – not only means/averages, but 'almost' all statistical properties, of an ensemble to those obtained using time series in one very long realization. A similar property is defined in space by replacing time by space coordinate in which the flow domain has an infinite extension, at least in one direction. In other words, the ergodicity hypothesis in a way eliminates the necessity to study the problem *en masse:* one "good" realization/or snapshot may be and in many cases is good enough.

[6]One cannot be sure even whether one can always consider the statistically stationary turbulent flows as "equilibrium" except those with large scale time independent excitation.

[7]In the language of mathematicians *invariant probability measures*, and there is an unsolved question/problem *which one is selected in experiments*. Ruelle (1983).

Although it is not known whether three-dimensional turbulent flows are ergodic.[8] it is common to use the ergodicity hypothesis in turbulence research, e.g., in physical and numerical experiments: turbulent flows are just believed to be ergodic at least the statistically stationary ones. In other words, in statistically stationary situations the time statistics obtained in experiments is believed to correspond to a unique probability measure invariant under time evolution. There also seems to exist no direct evidence regarding the validity of the ergodicity hypothesis in turbulent flows except of one exploiting the property of a turbulent flow which is both statistically stationary in time and homogeneous in space, Galanti and Tsinober (2004). In such a flow its temporal and spatial statistical properties such as temporal statistics corresponding to a time series at a single point in space and spatial statistics based on a single time snapshot over the flow domain should be the same if the ergodic hypothesis is correct, see Fig. 6.1. One sees that indeed the temporal and spatial statistical properties shown in this figure are close to identical. However, we repeat that though the statistics are the same there is far more one has to explore as concerns individual realizations - either one long time realization or a single time spatial snapshot. It should be emphasized that though this example is in favour of the ergodicity hypothesis, there are non-trivial problems such as outlined in Tsinober (2009) Sect. 3.7.

A tempting simplified interpretation or even better just illustration (no proofs, etc. so far) of the ergodicity property of statistically stationary flows is in terms of an attractor assumed to exist,[9] which is a very complex non-trivial object, Doering and Gibbon (2004), Foiaş et al. (2001), Robinson (2001). Almost any particular long enough flow realization comes very close to almost (i.e. with exception of measure zero) points of the attractor, and so will do almost any other (very long) realization. In other words, the solutions corresponding to the trajectories in the phase space will asymptotically visit (almost) all the attractor provided that the time span is long enough. In this sense almost all realizations will be almost the same. Hence almost the same statistical properties - not just means, i.e. ergodicity. These realizations will differ in their instantaneous, i.e. particular appearance not only due to (the popular) sensitive dependence on initial conditions, but also due to instabilities/*complexity of the behavior due to the complex structure of the attractor*, Arnold (1991) at almost any

[8] *The problem with this ergodicity assumption is that nobody has ever even come close to proving it for the Navier–Stokes equation* , Foiaş (1997) though some mathematical results, which are claimed to be relevant to turbulence are given in Foiaş et al. (2001). Nameley, they have shown that there are measures – in the language of physicists ensembles – on a function space that are time-invariant. However, invariance under time evolution is not enough to specify a unique measure which would describe turbulence. Another problem is that it is not clear how the objects that the authors have constructed and used in their proofs are relevant/related or even have anything to do with turbulence.

[9] Turbulent flows possess (empirically) stable statistical properties (SSP), not just averages but almost all statistical properties. In case if statistically stationary flows the existence of SSP seems to be an indication of the existence of what mathematicians call attractors. But matters are more complicated as many statistical properties of time-dependent in the statistical sense turbulent flows (possessing no attractor, but stable SSP) are quite similar at least qualitatively to those of statistically stationary ones as long as the Reynolds number of the former is not too small at any particular time moment of interest. This can be qualified as some manifestation of qualitative temporal universality/memory.

Fig. 6.1 Top - The 'tearing drop' pattern, which is the joint PDF of the invariants R, Q of the velocity gradient tensor $\partial u_i/\partial x_j$, $R = -1/3\{s_{ij}s_{jk}s_{ki} + (3/4)\omega_i\omega_j s_{ij}\}$, and $Q = (1/4)\{\omega^2 - 2s^2\}$. On the left is shown temporal statistics corresponding to a time series at single point in space. On the right is shown spatial statistics based on a single time snapshot over the flow domain. Bottom - An example of joint statistics of three quantities related by incompressibility relation - joint PDF of the eigenvalues of the rate of strain tensor Λ_1, Λ_2, Λ_3 in the plane $\Lambda_1 + \Lambda_2 + \Lambda_3 = 0$. Note that the time statistics in the figure on the right shows clear traces of time evolution, whereas nothing of the kind is observed with the spatial statistics on the left as it corresponds to one time snapshot and in this sense has nothing to do with the time evolution. Galanti and Tsinober (2004)

time and point of phase space and physical space. Tritton (1988) defines turbulence as a *state of continuous instability*. Noteworthy is that the "same" above is essentially "statistically", their instantaneous "appearance" cannot be expected to be close in every respect. It is noteworthy that one cannot give a similar interpretation of spatially homogenous flows as any single time snapshot does not reflect any time evolution, see again Fig. 6.1.

A related issue is predictability and uncertainty with its quantification being a generic issue in science, technology and applications. This is a central concern in modeling of all complex, highly-dimensional systems, Boumans et al. (2013), including systems governed by deterministic laws, as classical NSE turbulence. The appar-

ently random behavior of turbulence in incompressible media is a manifestation of the deterministic law of nature, adequately described by NSE. Nevertheless, turbulence is traditionally considered as containing inherent uncertainty, being chaotic. An important point is that complex (chaotic) behavior makes the law neither probabilistic nor indeterminate. The lack of predictability (and uncertainty) in chaotic deterministic systems is related to what we, humans, are able to observe, analyze and compute, i.e., it is in a sense subjective, independently of whether or not this lack of predictability is due to sensitive dependence on initial conditions. In other words, the common accusation of these being uncertain because they are chaotic, Palmer and Hardaker (2011) is a misinterpretation of chaotic behavior—"chaotic" and "uncertain" are far from being synonymous, Tsinober (2016). Predictability in meteorology dealing with a more complicated issue is ideally attempting prediction of individual realization in spite of the general claim and evidence that two initially nearly (but not precisely) identical turbulent flows become unrecognizably different on the time scale of dynamical interest. In reality, predictability in meteorology deals with the prediction of (statistical) large scale properties of some "narrow sub-ensemble" of realizations based on models (like LES) again - eliminating the "small scales", but not totally: *The problem faced by anyone trying to model weather and climate is that we cannot totally ignore the unresolved scales of motion*, Palmer (2005). Instead, as in any modelling approach *we try to represent the unresolved scales in climate models by imagining an ensemble of sub-grid processes in approximate secular equilibrium with the resolved flow. The ensemble-mean (or "bulk") effect of these sub-grid processes is then given by a set of relatively simple (e.g. diffusive-like) deterministic formulae. We call such formulae "parameterizations" of the sub-grid processes*, Palmer (2005). The grand question is about adequacy and the like with particular regard to nonlocality and the inherent dissipative and rotational nature of turbulent flows, see Chap. 7 below.

Concerning modeling, the origin of uncertainty is not in the behavior of real systems, such as described by NSE, but in their simplified, low dimensional models(due to finite computing resources) and underestimation of the role of unresolved scales: the belief that the unresolved scales and processes associated with them can be adequately represented by relatively simple (e.g., diffusive-like) deterministic formulae as suggested above, Palmer (2005), or other "parameterizations", which represent key processes in climate models without resolving them, McFarlane (2011). In such an approach the small/unresolved scales are essentially abused assuming that they are 'slaved' to the explicitly treated part of the flow and serve mostly as a passive sink of energy. This is a major misconception and oversimplification.

One of the fascinating issues closely related to the issue of the predictability of fundamental nature is the dynamics of an initial error, which is the measure of the differences between some two realizations of a turbulent flow under almost (hence the error) the same conditions. An important aspect is that the error also possesses stable statistical properties in the sense that errors corresponding to different pairs of realizations have the same statistical properties e.g. see Tsinober and Galanti (2003).

Another related issue concerns reproducibility beyond just the overall statistical, which, in fact, to a large extent is the same as the one of predictability in meteorology.

Far better predictions may be possible when a system is under strong influences like rotation, stable stratification, Cullen (2006) and similar; with high level of control of, e.g. inflow conditions as in a recent example in a boundary layer flow, Borodulin et al. (2011) and a far broader general theme of flow control including turbulent flows mainly in engineering, Gad-el-Hak and Tsai (2006), and mathematical sciences, Barbu (2011) and references therein. For more on ergodicity see Sect. 3.7 Ergodicity in Tsinober (2009).

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Part III
Issues of Paradigmatic Nature II—Specific Features

This is a continuation of the previous part, which included a discussion of the consequences of the complex behavior of systems described by purely deterministic equations including the necessity of changing the paradigmatic meaning of apparent randomness/stochasticity of turbulence which is roughly just the complexity due to a large number of strongly interacting degrees of freedom governed by the Navier–Stokes equations. It is devoted to more specific issues than in the previous part, but not less important. What follows comprises or are related to the Part II of the major qualitative universal features of turbulent flows as briefly described/listed in Chap. 1. These include the N's of turbulence with the emphasis on nonlinearity and nonlocality and the impact of the latter on such issues as large Reynolds number behavior and related. Here as before, we also make a distinction between the issues of paradigmatic nature and those which are apparently/seemingly (pseudo-) paradigmatic such as the most popular problematic "cascade," whatever this means, which by definition is a local process.

As almost everywhere the key quantities are the vorticity and strain production possessing the property of self-amplification independent of the "mediators" such as the mean flow and the nature of flow excitation if not too small in space/time scales. In other words to maintain large dissipation, there is no (most tempting) need for cascade as a means of "transferring energy from large to small scales." It is the process of self-amplification of velocity derivatives which is responsible for the dissipation anomaly of three-dimensional turbulence. It also underlies the property of qualitative universality of small scales in turbulent flow.

A noteworthy feature is that the arguments in Chaps. 7–9 are supported by results from experiments at high Reynolds numbers not accessible to DNS of NSE with pointwise access to the full tensor of velocity derivatives, e.g., vorticity, strain, and many others. Intermittency and structure(s) of and/in turbulence are overviewed in the last chapter taking into account the relevant aspects from the previous material.

Chapter 7
The N's of Turbulence

Abstract We start with the N's of turbulence. These comprise most of the reasons why turbulence is so impossibly difficult along with the essential constructive aspects facilitating all that is found in this book, i.e to a large extent the "essence" of turbulence. Whatever the approach there are important common issues, difficulties, features. Most of theses belong to the following categories: nonlinearity, nonlocality (and consequently "nondecomposabilty") and non-integrability, non-Gaussianity and non-Markovianity, non-equilibrium and (time) irreversible, no scale invariance and no other symmetries, no small parameters and no low-dimensional description. As a consequence there no theory based on first principles as such NSE equations – a real frustration for a theoretician. In other words, the terms without the "non"s (e.g., non-linearity, non-locality, etc.) do belong to the category that theory can handle, but this seems unfortunately to exclude turbulence. There are also other closely related issues as, e.g. uncertainty and unpredictability.

To begin we shall concentrate on the (in our view) most acute: nonlinearity and nonlocality with brief remarks on the rest, which is not to say they are unimportant.

Among a number of important aspects of nonlinearity we must emphasize that it is responsible for the key properties of turbulence as an essentially rotational and strongly dissipative phenomenon in contrast to the beliefs that, e.g. *turbulence is an inviscid, inertial phenomenon, and is uninfluenced by the precise nature of the viscous mechanism*, Holmes et al. (1996). The property of nonlinearity as manifested in self-amplification of strain (along with vorticity) aided by nonlocality is responsible for the dissipation anomaly of three-dimensional turbulence in contrast to the most common belief on the role of vorticity and cascade, the latter being mostly a fictitious creature as a means of energy transfer from large to small scales.

Nonlocality is a generic internal property of turbulent flows and exists independently of the presence of mean shear or other external factors, but has different and rich manifestations for different external factors. One of such manifestations is the direct and bidirectional coupling between large and small scales. We comment on the problematic issue of a variety of claims for locality which are not unrelated the interpretational abuse of observations.

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7.1 Non-integrability

> *One owes to Euler the first general formulas for fluid motion ... presented in the simple and luminous notation of partial differences... By this discovery, all fluid mechanics was reduced to a single point analysis, and if the equations involved were integrable, one could determine completely, in all cases the motion of a fluid moved by any forces.* Lagrange J-L (1788) Mécanique analitique, Sect. X, p. 271, Paris.

The "if" in the above quotation is crucial: the Navier–Stokes equations are not integrable. Integrable systems, such as those having a solution 'in closed form' exhibit regular organized behavior, even those having an infinite number of strongly coupled degrees of freedom. A prominent example is provided by the solitons in the systems described by the Korteveg–de Vries and Shrödinger equations. Two other examples are the Burgers and the so-called restricted Euler equations, which are integrable equations, and exhibit random behavior only under random forcing and or initial conditions, otherwise their solutions are not random.[1] That is, these examples represent the response of nonlinear systems to random forcing and which otherwise are not random: they should be distinguished from problems involving genuine turbulence. Navier–Stokes equations at sufficiently large Reynolds number having the property of intrinsic stochasticity (apparent) in the sense that they possess mechanisms of self-"randomization" (i.e. needing no random forcing) most probably at all scales, which are not fully understood. An important point is that non-integrability is intimately related to chaotic/complex behavior, thereby making a considerable contribution to the difficulties of turbulence is so impossibly difficult.

7.2 Nonlinearity

The general aspect of nonlinearity, the chaotic/complex behavior of the fluid flow was discussed in Part I. This is the most frequently fingered as the main 'guilty party' for the difficulties and almost everything else concerning turbulence. This is definitely true as without nonlinearity there are no instabilities, bifurcations, transitions, or turbulence itself. It is nonlinearity which makes life difficult due to the massive use of decompositions giving rise to the most problematic issue of "cascade" and the like. But, as we will see below, nonlinearity is not alone. Furthermore, there are several 'howevers'.

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[1] There is no consensus on the meaning of the term integrability, but it is mostly agreed that integrable systems behave nicely and are globally 'regular', whereas the nonintegrable systems are not 'solvable exactly' and exhibit chaotic behavior, see Zakharov (1990) and Kosmann-Schwarzbach et al. (2004) for more examples and discussion on what integrability is. The latter write *It would fit for a course entitled "Integrability" to start with a definition of this notion. Alas, this is not possible. There exists a profusion of definitions and where you have two scientists you have (at least) three different definitions of integrability but mention the definition by Poincaré: to integrate a differential equation is to find for the general solution a finite expression, possibly multivalued, in a finite number of functions.*

First, as mentioned above there are nonlinear problems that are completely integrable. Well-known examples, are systems displaying solitons or solitary waves. In these systems the many degrees of freedom are so strongly coupled that they do not display any chaotic/irregular behavior. Instead they are entirely organized and regular, see Zakharov (1990), Kosmann-Schwarzbach et al. (2004) and references therein. By a questionable and problematic analogy it was thought for some time (without almost any constructive output) that the so-called coherent structures in turbulent flows may be viewed and treated in a similar way, e.g. Newton and Aref (2003).

Second, nonlinearity is frequently blamed for the difficulties in the closure problem which is associated with some form of decomposition, such as the Reynolds decomposition of the flow field into the mean and the fluctuations, or similar decompositions into resolved and unresolved scales associated with large eddy simulations. The essence of the problem is that the equations for the mean field (resolved scales) contain moments of the fluctuations (unresolved scales) due to the nonlinearity of the NSE. However, a similar problem exists for the so-called advection-diffusion equation describing the behavior of a passive scalar in some flow field. But this equation is linear. Here the problem arises due to the multiplicative nature of the velocity field, since velocity enters this equation as its coefficients.

A noteworthy issue is that the inertial nonlinearity in the Euler setting, $(\mathbf{u}\cdot\nabla)\mathbf{u}$ has a relative nature depending on the frame of reference – it is not Galilean invariant and in pure Lagrangian setting is not even present - the acceleration in pure Lagrangian setting is linear so that the inertial nonlinearity consists only of a term with pressure.

Getting back to the Eulerian setting it is noteworthy that the nonlinearity $(\mathbf{u}\cdot\nabla)\mathbf{u}$ consists of two parts, irrotational and solenoidal, $(\mathbf{u}\cdot\nabla)\mathbf{u} = \nabla(\alpha + u^2/2) + \nabla\times\beta$, where $\nabla\alpha$ and $\nabla\times\beta$ are the irrotational and solenoidal parts of the Lamb vector $\boldsymbol{\omega}\times\mathbf{u} = \nabla\alpha + \nabla\times\beta$. We also point out that there is strong cancellation between $\mathbf{a}_l \equiv \partial\mathbf{u}/\partial t$ and $\mathbf{a}_c \equiv (\mathbf{u}\cdot\nabla)\mathbf{u}$ (Tsinober et al. 2001) so that the irrotational part of the sum $\mathbf{a} = \partial\mathbf{u}/\partial t + (\mathbf{u}\cdot\nabla)\mathbf{u}$, i.e. the acceleration is dominated by pressure, but in the norm $\left\langle(\ldots)^2\right\rangle$, Vedula and Yeung (1999). *However, it is the rotational part of acceleration* $\mathbf{a}_l = \partial\mathbf{u}/\partial t + \nabla\times\beta = \nu\nabla^2\mathbf{u}$ *without which there is no turbulence*!

Among the most important aspects of nonlinearity is the production of small-scale fluctuations aiding the enhanced dissipation of turbulence (and enstrophy). The common claims are that these processes involve a "cascade" as a means of energy 'delivery' from large to small scales. The idea comes from Richardson (1922, p. 66) and Kolmogorov (1941a) as a purely qualitative phenomenological idea, see below. However, there are a number of paradigmatic non-trivial issues/difficulties concerning cascades. These start with the definition of the notions of 'scale' and other issues related to such ill defined concepts as 'the interscale energy transfer'. One of the most acute issues is associated with the existence of the process of *self-amplification* of velocity derivatives (small scales) via the terms $\omega_i\omega_j s_{ij}$ (enstrophy

production) and $-s_{ij}s_{ij}s_{ij}$ (strain production). Among the consequences of the latter (!) is that there is no need for any mediators (such as cascades) to maintain the high level of dissipation – turbulence as a whole (!) "handles" the issue without any unnecessary mediators. This is because the interaction of large and small scales is both bidirectional and nonlocal, see below.

7.2.1 Nonlinearity Plus Decompositions Gives Birth to "Cascades" [2]

> *Most embodiments of Kolmogorov's idea of turbulent cascade rest on heuristic modelling rather than mathematical treatment of the Navier–Stokes equations.* Kraichnan (1991).
>
> *All this cascade in Fourier space is a dream of linearized physicists.* Betchov (1993).
>
> *...however attractive the notion of energy cascades, though, it must be taken mostly as pedagogical imagery.* Falkovich and Sreenivasan (2006) .
>
> *...the small-scale dynamics are strongly coupled to the large-scale phenomena. This may be a reason for a serious reexamination of the very concept of the turbulence energy cascade which, within the framework of the present development, seem neither possible nor needed.* Yakhot (2006).

Cascade is an essentially phenomenological creature, see e.g. the quotations above. Though phenomenology is not a genuinely fundamental problem/issue, see Chap. 5 in Tsinober (2009), there are several aspects of a paradigmatic nature. This is a major reason for a digression involving several issues. Additionally, historically "cascades" were motivated by the concern/necessity for some mechanism for the delivery of energy from large to small "scales".[3] The cascade picture of turbulent flows takes its origin from Richardson (1922, p. 66) and Kolmogorov (1941a), the

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[2]Otherwise, i.e. in the physical space there are no cascades and there is no necessity for such creatures. In other words it is a "technical issue" - turbulence "handles" the issue without any unnecessary mediators.

[3]For example, see Tennekes and Lumley (1972): *dissipation of energy at small scales occurs only if there exists a dynamical mechanism that transfers energy from large scales to small scales* p. 60; *the internal dynamics of turbulence must transfer energy from large scales to small scales* p. 68; *there exists a dynamical mechanism that transfers energy from large scales to small scales ...the energy transfer from large eddies to small eddies may be considered in terms of vortex stretching* p. 75.

The latest example is by Jiménez et al. (2017), Jimenez (2018): "the three-dimensional energy cascade of the viscous Navier-Stokes equations is an attempt by the flow to fill the more numerous wavenumbers at small scales, frustrated by the vigorous viscous dissipation at those scales"

Again one encounters the question about the need for cascades!

This necessity as the only option is due to the view that small scales are a kind of passive sink of energy which is a major misconception: there is a rich direct and bidirectional coupling between large and small scales comprising an essential part of the complex interaction between the multitude of the degrees of freedom (which as mentioned is preferable to terms like eddies, scales and so forth) in turbulent flows. The small scales in turbulence (i) are very far from being simple as, e.g. objects used in statistical mechanics and (ii) interact non-trivially, bidirectionally and non-locally, with the rest of the flow. In other words, contrary to the common view, the small scales cannot be seen as

term cascade is due to Onsager (1945). Both proposed a phenomenological qualitative picture in physical space, but both gave up any further treatment,[4] for later numerous claims of the same kind see e.g. references in Vassilicos (2015) and Jiménez et al. (2017). However, the first and the main emphatic statement here is that there is no need for such a mechanism to maintain the high level of turbulent dissipation (dissipation "anomaly") in 3D-turbulence, see next section. The right alternative is that most neglected by the community the mechanism of self-amplification of velocity derivatives (both vorticity and strain) and particularly strain, see the next section. In other words, "cascade" is not the only alternative and appears to be not the right alternative. The discussion on the paradigmatic issues concerning the ill posed concept of "cascade" is expanded below due to its tremendous popularity and because the issue is a typical one that belongs to the category as in the warning by Saffman (1978): *It must of course be kept in mind that the achievements or intrinsic value of a theory is not decided by democratic means. The majority view in science is not*

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a kind passive sink of energy and/or as 'slaved' to the large scales—the small scales react back in a nonlocal manner and (iii) they carry lots of the basic physics of turbulence, such as those associated with its fundamental properties as a rotational and dissipative phenomenon rather than just an essentially inviscid, inertial phenomenon, as these properties are mainly excited just by the nonlinearity in NSE, see next section.

The particular aspect of dissipation is because dissipation is strain, and the role of the field of strain is not limited just by dissipation - it has a number of other roles as it is an active field, see next section, and Tsinober (2009) Chap. 6, Sect. 6.2.2 Why strain too.

Among other things this results in an alternative to the conventional "cascade" (being inherently local) with an option of a nonlocal exchange between LS and SS of energy and not just energy as just mentioned above. This is due to the inherently non-local nature of turbulence - the just mentioned direct and bidirectional coupling between LS and SS being one of its important manifestations . For more on the issue of nonlocality, see Tsinober (2009), Sect. 6. For a discussion of a number of issues on "cascade" see Sect. 5 Cascade in Tsinober (2009) and the next section on nonlocality below.

[4]The observation by Richardson was made by looking at the structure of clouds, i.e. condensed water vapour at the interface between laminar and turbulent flows in their bulk, which do not necessarily reflect the structure of the underlying velocity field and other dynamical variables.

Richardson (1922): *Thus, because it is not possible to separate eddies into clearly defined classes according to the source of their energy; and as there is no object, for present purposes, in making a distinction based on size between cumulus eddies and eddies a few metres in diameter (since both are small compared with our coordinate chequer), therefore a single coefficient is used to represent the effect produced by eddies of all sizes and descriptions.*

Kolmogorov (1941a) formulated a cascade picture supplemented by the assumption about the chaotic nature of cascade was used by Kolmogorov (1941a) in a 2/3 page footnote as a qualitative justification of his hypothesis on the local isotropy of turbulent flows for very large Reynolds numbers. The cascade picture is based on the intuitive notion that turbulent flows possess a hierarchical structure consisting of 'eddies' (Richardson's 'whirls', Kolmogorov's 'pulsations', etc.) as a result of successive instabilities. The essence of this picture is in its successive hierarchical process, and in this sense it is the same as the Landau–Hopf picture of transition to turbulence as a 'cascade' of successive instabilities. The difference is that the Richardson-Kolmogorov cascade refers to a process at some fixed Reynolds number, whereas the Landau–Hopf picture describes the process of changes occurring as the Reynolds number increases.

Kolmogorov (1941a)*: It is natural that in so general and somewhat indefinite a formulation the just proposition cannot be rigorously proved . We may indicate here only certain considerations speaking for the advanced hypothesis.*

necessarily the right one.[5] Compare the statement by Vassilicos (2015) *The one mechanism for turbulence dissipation at high Reynolds numbers that has dominated turbulence textbooks and research activity over the past seven decades is indeed the Richardson-Kolmogorov cascade... the Richardson-Kolmogorov cascade is present (or at least believed to be present) far enough from the wall in many such flows, as in boundary-free turbulent flows, and this review therefore concentrates on the pervasive turbulence dissipation mechanism caused by this cascade.* A latest example is found in the announcement of the Euromech-Ercoftac Colloquium 589 "Turbulent Cascades II", 5–7 December 2017. Ecole Centrale Lyon: *This colloquium will bring together scientific experts from the weather-forecasting, applied mathematics, hydrodynamics and geophysics communities in order to progress together on the understanding and modeling of the common feature of all these flows: the turbulent cascade.*

Before proceeding we would like to clarify here the meaning of the term "decompositions". Namely, these are the true decompositions that constitute a representation basis for a flow, i.e. these are rigorous decompositions which adequately represent the whole flow field in question! For example, see the rigorous treatment of cascade between the components based on eigensolutions of the Stokes operator in Foiaş et al. (2001). This treatment - as all the others of this kind - is of essentially statistical nature. Hence the "complaint" on *ruling out any possibility of attributing the ongoing cascade to specific whirls visible where the flow actually evolves: the real space,* Cardesa et al. (2017), which is *primarily done by isolating individual intense structures, and tracking them in space and time (and occasionally across scale),* Jiménez et al. (2017). In the latter approach only parts of the flow are used - *individual intense structures*, Being not a representation basis for a flow such an approach cannot be considered as reliable for addressing issues of cascade existence (*especially in physical space in which no representation basis does exist*) as well as other issues as (non) locality and so forth. There is also a problematic aspect concerning the identification of *specific whirls, individual intense structures,* and the like, i.e. the nature of their identity as finite structures only defined by their energy and the *tracking of them in space and time*.

Returning to the issue of decompositions any decomposition results in a non-trivial bidirectional relation between the small and the large scales (whatever this term means, degrees of freedom sounds better, but does not resolve the associated problems), and which is non-local (functional) both in space and time, i.e. history-dependent. Nonlinearity along with any decomposition/representation of a turbulent field results in a bi-directional process of interaction/exchange of (not necessarily only) energy, see Lumley (1992). This occurs between the components of the decomposition and, in particular, in a "cascade" with its properties depending on the "physics" (e.g. energy exchange/transfer) of the specific decomposition, although one would expect that energy transfer, just like any physical process, should be

[5]The citations at the beginning of this section belong to the minority. But even in the minority some authors change their minds and return to exploiting the cascade concept, see e.g. Falkovich (2009) and Goto and Kraichnan (2004).

invariant of particular decompositions/representations of a turbulent field. Therefore it is unlikely that quantities like energy flux are well defined either. In this sense - with all the critics - Kolmogorov's choice of dissipation (and energy input) are well defined and decomposition independent quantities, whereas the energy flux is (generally) not, since it is decomposition dependent.

The above together with nonlocality (see below) makes the popular concept of cascade ill posed – cascade by definition is a local process. This along with other important issues emphasizesthe acute and generic problems arising from employing decompositions, see Sect. 3.3.1 On the utility(and futility) of various decompositions. The exceptional role of physical space.

On the contrary the production of strain, i.e. dissipation which is assumed to be the final destination of the assumed "cascade" does not care about decompositions, see below. In other words, the energy 'cascade' (whatever this means, if anything) is associated primarily with the quantity $-s_{ij}s_{jk}s_{ki}$ responsible for the strain production. i.e. dissipation, rather than with the enstrophy production $\omega_i\omega_j s_{ij}$ and, moreover, that vortex stretching suppresses the cascade rather than aiding it, at least in a direct manner. Furthermore, it is the vortex compression, i.e. $\omega_i\omega_j s_{ij} < 0$ (rather than vortex stretching $\omega_i\omega_j s_{ij} > 0$) that aids the production of strain/dissipation and, in this sense, the 'cascade', for more on this see below in the next section.

One process deserves special attention. It is the continuous transition of laminar flow into turbulent flow via the entrainment process through the boundary between laminar/turbulent regions in the partly turbulent flows. In all these flows there is a distinct Lagrangian aspect: the abrupt transition of fluid particles (i.e. Lagrangian objects) from the laminar to turbulent state when passing across the laminar/turbulent 'interface'. Abrupt is a key word, i.e. without any cascade whatsoever. This class of partly turbulent flows comprises a strong counter example to the cascade. In all such flows the fluid becomes turbulent in 'no time' without any cascade whatsoever. This is a clear indication that one can hardly speak about the Lagrangian nature of the cascade as the fluid particles are purely Lagrangian objects.

Moreover, in purely Lagrangian description the inertial effects are manifested only by the term containing pressure in the ENSE. Therefore, the nonlinearity in the Lagrangian representation cannot be interpreted in terms of some local cascade, there is an essentially nonlocal relation between the fields of pressure and velocity, see Sect. 3.4. An extreme example is just the absence of any "cascade" in Lagrangian chaotic, but *Eulerian laminar* flows.

That the cascade concept is ill-posed is emphasized also in the case of passive objects, whose evolution is governed by linear equations, with the velocity field entering multiplicatively, thus making them 'statistically nonlinear'.

We should also mention two examples of stability related to the above. The first is that of spatially periodic flows, which may destabilize directly into small-scale three-dimensional structures, Pierrehumbert and Widnall (1982). The second example, Ott (1999) shows that significant variations down to very small scale can be produced by a single instability at much larger scale without any 'cascade' of successive instabilities.

There is much "disorder" and confusion in the context of terminology due to the massive use of 'cascade picture' of energy (and not only energy, energy flux/transfer, etc.) without much distinction/justification both in physical and Fourier space as well in physical space both in Eulerian and Lagrangian settings, see Vassilicos (2015), Xu et al. (2016), Jiménez et al. (2017) and references therein. The list of contexts is enormous with multitudes of discussions, disagreements, etc. But on one thing there is a total agreement, that, e.g. if energy is fed in large scales, it has to be transferred to small scales to be dissipated by viscosity, i.e. there should be a cascade.

There is still a special question whether there is a cascade in physical space? The question is prompted largely by the issue of terminology, in particular on the claims of "cascades" everywhere, see e.g. the announcement of "Turbulent Cascades II" , 5–7 December 2017. Ecole Centrale Lyon: *This colloquium will bring together scientific experts from the weather-forecasting, applied mathematics, hydrodynamics and geophysics communities in order to progress together on the understanding and modeling of the common feature of all these flows: the turbulent cascade.*

The answer is found already in von Neumann (1949, pp. 452–3), see also Onsager (1949) who recognized that this process occurs not in physical space, but in Fourier space: *…the system is "open" at both ends, energy is being supplied as well dissipated. The two "ends" do not, however, lie in ordinary space, but in its Fourier-transform. More specifically: The supply of energy occurs at the macroscopic end—it originates in the forced motions of macroscopic (bounding) bodies, or in the forced maintenance of (again macroscopic) pressure gradients. The dissipation, on the other hand, occurs mainly at the microscopic end, since it is ultimately due to molecular friction, and this is most effective in flow-patterns with high velocity gradients, that is, in small eddies …Thus the statistical aspect of turbulence is essentially that of transport phenomenon (of energy)—transport in the Fourier-transform space.* We remind that the Richardson–Kolmogorov cascade picture was formulated in physical space, but this was done in a purely qualitative form, see above.

That is, due to the nonlinearity in the NSE energy is "distributed" among the *Fourier modes* (or components of other decompositions) *not scales in the physical space* as is massively claimed, unless the 'scale' is defined just as an inverse of the magnitude of the wave-number of a Fourier mode or alike, which is not easy for everybody to swallow. Moreover, we point out that any decomposition (be it in physical space, Fourier or any other) brings the 'cascade' back to life. However, one of the problems with decompositions is that the energy distribution among the components of a particular decomposition is different for different decompositions, i.e., the energy exchange/transfer (as any other process) is decomposition dependent.

We recall the elementary illustration with a statistically stationary homogeneous turbulent flow in Euler setting with energy fed in large scales L. It is straightforward to see that in such a flow the *mean energy* of volume of *any scale* is changing due to external forcing and dissipation only – there is no contribution in *the mean* of the nonlinear term, which includes the term with the pressure. That is, if one chooses to define a 'scale', ℓ, in physical space as a volume, say, of order $\sim\ell^3$,

$\ell < L$, then in a such a flow there is no cascade in physical space in the sense that, *in the mean*, there is no energy exchange between different scales ℓ. However, in a statistically stationary state $\int \mathbf{F} \cdot \mathbf{u} d\tau = \int \epsilon d\tau$, $\epsilon = 2\nu s_{ij} s_{ij}$, i.e., energy input, which is associated with large scales, equals dissipation, which occurs mostly in the "small scales". So (almost) everybody is shouting cascade, cascade!...

Is there a contradiction? Does the equality $\int \mathbf{F} \cdot \mathbf{u} d\tau = \int \epsilon d\tau$ mean that the energy should be somehow 'transferred' from large to small scales via some multi-step process? Not necessarily – for example, a statistically stationary homogeneous turbulent flow with large scale excitation two big *neighboring* "eddies" (in the statistically stationary homogeneous turbulent flow with large scale excitation) can dissipate energy directly through encounters with each other at small scales – much smaller than their own scales, i.e. via a nonlocal interaction. Such a process will appear in Fourier space as continuous energy transfer from modes with small to modes with large wave numbers, for more see Sect. 5.4.2 Is there cascade in physical space? and Sect. 5.4.3 What are the 'small scales' in turbulent flows? in Tsinober (2009).

This apparent contradiction is due to the ambiguities in the meanings of the term 'scale', and the like. The issue is directly related to the decomposition/representation of the turbulent flow field. Indeed, the reason for the above result on the absence of energy exchange between different scales in physical space is because no decomposition is involved in the above 'definition of scale'. This concerns not only something as ℓ, but most importantly also involves the field of strain s_{ij}. We return to this issue below when discussing why turbulence does not need "cascades" to maintain large dissipation at small scales, i.e. who is the guilty party for the dissipation anomaly.

Meanwhile, some comments on the related question of the role of nonlinearity in the physical space, which is mainly due to its conservation properties in the NSE. The latter is because it has the form of a spatial flux. The conserved quantities (energy and momentum) are "redistributed" energy just in physical space – there is no need for decompositions and no specific "direction" for the flow of conserved quantities . Some authors use for these fluxes the term "cascades" and in this manner bring into existence "cascades" in physical space, see e.g. Jiménez et al. (2017) and references therein. It is hard to agree with this kind of "broadening" of the term "cascade", whirls, etc. though it is fair to admit that such a broadening is far from being the largest among the host concerning the issue.

In the above context an interesting observation related issue was made by Pumir et al. (2014). It concerns the behavior of the average of the pressure-gradient term $\mathbf{u} \cdot \nabla p$ in the energy balance equation conditioned on the kinetic energy of fluid elements in Euler setting. We emphasize the term "fluid elements in Euler setting", reminiscent of those by Cardesa et al. (2017), as the authors insist throughout their paper on massive use of the term "fluid particles", though their work has been performed exclusively in Euler setting. We kept the authors' terminology in quotes and in Fig. 7.1 – their figure 3(b). The main result as summarized by authors is as follows: *The conditional average* $\langle \mathbf{u} \cdot \nabla p | u^2 \rangle$ *reveals striking differences in the role of pressure forces in 2D and 3D. Whereas in 2D pressure forces do not result in an*

Fig. 7.1 The average of the pressure-gradient term conditioned on the kinetic energy of the fluid particle, $\langle \mathbf{u}\cdot\nabla p|u^2\rangle$, in 3D turbulence (DNS data at $Re_\lambda = 115$ and 430). Contrary to the 2D case, the conditional mean of the pressure term is negative for particles with small u^2, and becomes strongly positive for larger values of u^2 , which implies that, on average, the pressure term in 3D turbulence takes energy from *slow* particles and gives to *fast* particles. This leads to a runaway effect and can be stopped only by viscous forces Pumir et al. (2014)

average transfer of energy towards slow or fast particles, they tend to accelerate the fastest particles in 3D. The latter is of special interest interest and is shown in Fig. 7.1.

The behavior of the conditional averages $\langle \mathbf{u}\cdot\nabla p|u^2\rangle$ has – in our opinion – an alternative interpretation as follows. The pressure and its gradient are fully determined as a functional of the field of velocity derivatives (!) rather than just the velocity field – $p = -\rho\nabla^{-2}(\omega^2 - 2s_{ij}s_{ij})$. Therefore one can expect the behavior of $\langle \mathbf{u}\cdot\nabla p|u^2\rangle$ as the one shown in Fig. 7.6 *left* for conditional averages of enstrophy ω^2 and total strain $s_{ij}s_{ij}$ conditioned on magnitude of velocity fluctuations vector, u. The essential difference with the 2-D case is due to the absence of the mechanisms of self amplification of velocity derivatives $\omega_i\omega_j s_{ij}$, $-s_{ij}s_{jk}s_{ki}$ in this case. In other words, just like velocity derivatives the $\mathbf{u}\cdot\nabla p$ is on the average larger for larger u^2. Far more work is needed in the interpretational contexts as e.g. to justify the claim that *the pressure term in 3D turbulence takes energy from slow particles and gives to fast particles.* As just mentioned the pressure is fully defined by the field of velocity derivatives and cannot be considered as just a primary "commander" over the flow field *redistributing energy among fluid particles, taking energy away from slow particles and giving it to fast particles,* etc. and *possibly leading to a runaway mechanism of the kinetic energy of particles in high Reynolds number flows*.

A bit more general and important interpretational aspect concerns the use of the energy balance equation. The nonlinear term in NSE is $(\mathbf{u}\cdot\nabla)\mathbf{u}+\nabla p \equiv \omega\times\mathbf{u}+\nabla(p+u^2/2)$. An important detail is that only the term $\mathbf{u}\cdot\nabla(p+u^2/2)$ remains in the equation of energy, since the term $\mathbf{u}\cdot(\boldsymbol{\omega}\times\mathbf{u})\equiv 0$ vanishes *locally*. That is there is no contribution to the energy balance from the Lamb vector $\boldsymbol{\omega}\times\mathbf{u}$ which is in the heart of any turbulent motion. This is true for the energy balance equation in any turbulent flow *locally*, in contrast to the NSE in which only the correlation $\langle\boldsymbol{\omega}\times\mathbf{u}\rangle=0$ and only for homogeneous/periodic BC flows. Among other possible consequences the above calls for cauton in making excessive/hasty conclusions from the statistics (!) *based solely on the energy balance equation!* One is tempted to ask, e.g. whether the Lamb vector has anything to do with many of the conclusions including the one by Pumir et al. (2014) drawn from the statistical processing and interpretations based solely/mostly on the energy balance equation.

Just as the energy equation above there is a related popular equation named as scale-by-scale (SBS) "energy" budget treated in physical space as the true energy equation. The SBS "energy" budget equation takes its origin from the incremental velocity two-point statistics of K41a. The term "energy" budget is not precise as the equation is not for the velocity energy, but rather for the energy of the velocity increment $S_2\equiv\left\langle(\Delta\mathbf{u})^2\right\rangle;\ \Delta\mathbf{u}\equiv\mathbf{u}(\mathbf{x}+\mathbf{r})-\mathbf{u}(\mathbf{x}),\ \Delta u\equiv[\mathbf{u}(\mathbf{x}+\mathbf{r})-\mathbf{u}(\mathbf{x})]\mathbf{r}/r$, see e.g. Sects. 2.4 and 6.2 in Frisch (1995). Note that one has to make a clear distinction between the energy flux in the Fourier space and the quantity defined by the Eq. (6.7) by Frisch named as the *physical-space energy flux,* which is not since one cannot define an *energy flux in physical space* - hence the use of Fourier space - see von Neumann (1949). A similar concern is about Frisch's statement on the nature of the Karman–Howarth–Monin relation (6.8): *Actually, as we shall now see, (*6.8*) for $\ell\neq 0$ is essentially an energy-flux relation*. This is absolutely correct, but ***it is not an energy flux relation in physical space!***.

Use of the SBS equation for the quantity $(\Delta\mathbf{u})^2$ requires far more intensive computational and interpretational effort than before and lead to non-trivial interpretational sues far from being resolved. This is clearly seen already from the conclusion of the paper by Cimarelli et al. (2016): *As a final comment, we stress that the terminology we have adopted throughout the paper should be considered as suggestive of the physical meaning of the statistical objects we were considering. In particular, as recommended in Sect. 2, the terms scale energy and scale-energy flux should not be taken too literally. Actually, energy is by definition an additive quantity. The scale energy is not. Its name is only intended to convey the idea that the scale energy or, more technically, the second-order structure function, obeys a conservation equation where the divergence of the relevant flux is determined by suitable sources. At small scales, the scale energy could be approximately accepted as a measure of the intensity of the eddies living at those scales. At larger scales this interpretation is misleading. Anyhow, the flux that is eventually dissipated by viscosity at small scales can be rightfully referred to as the flux of scale energy.* Cimarelli et al. (2016) and references therein, see also Jiménez et al. (2017).

There is an enormous potential for criticism and failure, so those intending such an undertaking may consider thinking over whether such an effort makes any sense at all, abandoning this endeavor and returning back to more transparent single point statistics with normal additive energy.

Further, some general notes mostly in the context of this section. We repeat that the issue is treated in physical space so that the claim about *the energy cascade among scales* is problematic at least for two reasons: (i) there is no such a creature as *cascade* in physical space as explained already by von Neumann (1949) and (ii) ambiguities in using term *scale* . The 'representation' of the flow field using the notion of 'scales' has a number of problematic entries starting with the problem that *Velocity increments do not constitute a representation basis for a flow*. Goto and Kraichnan (2004). They continue *In contrast to the invertible transformation between, say, wavevector components and wavelet amplitudes, the transformation from the velocity field in x space to sums and differences of velocities at two space points is overcomplete. If the velocity sums are struck out, leaving only the increments, the transformation is not invertible. Analysis that derives results by working with velocity increments rests on the implication of corroboration in a representation basis from which the velocity increments are constructed*.

Finally, we point out the inherent limitations on interpretations based solely/mostly on just on the energy balance equation and even more for $\langle(\Delta\mathbf{u})^2\rangle$. An important aspect in using two-point statistics is that it "destroys" the very basis of the cascade ideology as based on its locality. For example, computing $\Delta\mathbf{u}$ one also encounters large and even very large instantaneous dissipation at the ends $\mathbf{u}(\mathbf{x})$, $\mathbf{u}(\mathbf{x}+\mathbf{r})$. Such strong dissipative events (SDE) are considered unimportant in the inertial range, i.e. when $\eta \ll r \ll L$ - by definition the statistical properties of IR are (hypothesized to be) independent of viscosity However, it was observed experimentally at large Reynolds numbers that this assumption is incorrect, invalidating both the IR as a concept and particularly its locality, for details see Chap. 8.

The bottom line is that cascade and interscale energy flux do not make sense in physical space as they are not well defined in physical space. There is no need for them as a means - as any other mediators - for some mechanism of delivery of energy from large to small scales because of the mechanism of self-amplification of velocity derivatives both of strain and vorticity. In other words, "cascade" is not the only alternative and appears to be incorrect, see next section and below.

It just remains to add that at best the cascade picture of turbulence is more a reflection of the unavoidable (due to the nonlinear nature of the problem) hierarchic structure of various models of turbulent flows rather than of reality. Most of these models have no connection with Navier–Stokes equations. Others rely severely on closures.

7.2.2 Three-Dimensional Turbulence is an Essentially Rotational and Strongly Dissipative Phenomenon

Nonlinearity is responsible for the two most important properties of 3-D turbulence - it is an essentially rotational and strongly dissipative phenomenon. This statement is in contrast with one of the most common beliefs that turbulence is a purely inertial (essentially inviscid) phenomenon, e.g. Holmes et al. (1996). Wrote: *In fact, turbulence is an inertial phenomenon. That is, turbulence is statistically indistinguishable on energy-containing scales in gases, liquids, slurries, foams, and many non-Newtonian media. These media have markedly different fine structures, and their mechanisms for dissipation of energy are quite different. This observation suggests that turbulence is an essentially inviscid, inertial phenomenon, and is uninfluenced by the precise nature of the viscous mechanism.*

A formal objection is that dissipation is associated with terms of higher order derivatives such a the Laplacian in the NSE. But our concern is with the nonlinearity.

It is the Eulerian setting (rather than Lagrangian) which enables one to observe the following key paradigmatic properties of turbulence associated with the nonlinearity of NSE - turbulence is an essentially dissipative and rotational phenomenon.

There are two concomitant qualitatively universal physical mechanisms turning turbulence into a strongly dissipative and rotational phenomenon. These are the predominant *self-amplification* of the rate of strain tensor, s_{ij} ($\langle -s_{ij}s_{jk}s_{ki}\rangle > 0$) and nonlocal amplification of vorticity, ω_i ($\langle \omega_i\omega_j s_{ij}\rangle > 0$). The rate of strain tensor, s_{ij} and vorticity, ω_i are just the symmetric and antisymmetric parts of the tensor of velocity derivatives $A_{ij} = \partial u_i/\partial x_j \equiv s_{ij} + \frac{1}{2}\varepsilon_{ijk}\omega_k$. Though formally the two representations are trivially equivalent, it is also trivially obvious that it is more appropriate from the physical point of view to use strain and vorticity as corresponding to the two fundamental properties of turbulence as strongly dissipative and essentially rotational, with essentially different roles in the turbulence phenomenon. We emphasize that the direct causal relation to dissipation is not the only role played by strain $\epsilon = 2\nu s_{ij}s_{ij}$ in turbulent flows, for more see Sect. 6.2.2 Why strain too in Tsinober (2009), rather than to enstrophy. See Meneveau (2011) and references therein on the approaches employing the full tensor of velocity derivatives A_{ij}.

The mechanisms of strain and vorticity production are reflected by the terms $-s_{ik}s_{kj}$ and $\omega_j s_{ij}$ in the equations for s_{ij} and ω_i correspondingly. It should be emphasized that these terms are Galilean invariant, unlike the term $(\mathbf{u}\cdot\nabla)\mathbf{u}$ they take their origin is not Galilean invariant. Likewise the terms $-s_{ij}s_{jk}s_{ki}$ and $\omega_i\omega_j s_{ij}$ responsible for the production of the total strain $s^2 \equiv s_{ij}s_{ij}$ and the enstrophy ω^2 correspondingly. These are the outstanding quantities of third order and the empirical fact of paradigmatic importance is that both are positively skewed, so that both $-\langle s_{ij}s_{jk}s_{ki}\rangle$ and $\langle \omega_i\omega_j s_{ij}\rangle$ are positive. One of the key properties of velocity derivatives in turbulent flows is that it is not just produced - it is *self-amplified*. There are two aspects concerning the term of *self-amplification*. The first is about the self-amplification of the field of velocity derivatives (i.e. $A_{ij} = \partial u_i/\partial x_j \equiv s_{ij} + \frac{1}{2}\varepsilon_{ijk}\omega_k$) as largely independent on the field of velocity. The second concerns the self-amplification of the

field of strain as mostly produced via the term $-s_{ij}s_{jk}s_{ki}$ and the vorticity amplification via the interaction with strain $\omega_i\omega_j s_{ij}$. Hence the term *non-local amplification of vorticity* as compared with the *self amplification of strain*. See below.

In contrast to the common view: *It seems that the stretching of vortex filaments must be regarded as the principal mechanical cause of the high rate of dissipation which is associated with turbulent motion*, Taylor (1938) [6] it is the production of strain $-s_{ij}s_{jk}s_{ki}$ which is responsible *both* for (i) the enhanced dissipation of turbulence and in particular, for what is called "cascade" which is *believed* to result in enhanced dissipation moderating the growth of turbulent energy is achieved by the build up of strain of sufficient magnitude and (ii) the enstrophy production $\omega_i\omega_j s_{ij}$. In other words, *the true guilty party for the "dissipation anomaly" in three-dimensional turbulence is the strain self-amplification*. Apart from dissipation the strain field plays the role among several others, see Sect. 6.2 in Tsinober (2009) of an engine producing the whole field of velocity derivatives, both itself and the vorticity, with *compression rather than stretching!* aiding the prevalent production of strain and stretching aiding the prevalent production of enstrophy. We reiterate that it is of special importance on paradigmatic level that it is the strain production which is responsible for the finite overall dissipation at (presumably) any however large Reynolds numbers in contrast to two-dimensional flows where s^2 is an inviscid invariant.

The fascinating aspect of the above non-conformist statements is that it becomes literally obvious when one takes the time to look at both equations, i.e. for ω^2 and also for s^2, Tsinober (2000, 2009). The latter was missed by the community, because attention was concentrated on the search within the two-point statistical equations such as the Karman–Howarth equation, "energy balance" equation (scale-by-scale) for the second structure function and the like, see e.g. Chap. 7 in McComb (2014), Vassilicos (2015), Antonia et al. (2017) and references therein. This was motivated by the success of Kolmogorov (1941b) with his 4/5 law as an explicit relation between the third order longitudinal velocity structure function and the *mean* dissipation ϵ, $\left\langle(\Delta u)^3\right\rangle = -4/5\epsilon r$ for *large* Reynolds numbers for globally homogeneous and isotropic flows or assuming local homogeneity and isotropy. Such an approach, however did not bring a satisfactory answer to the question on the *principal mechanical cause of the high rate of dissipation which is associated with turbulent motion*, resulting in the massive phenomenological beliefs in fictitious "cascades" as the only means of the delivery of energy from "large to small scales" along with blam-

[6] This belief was massively adopted in the community along with some closely related. For example, Tennekes and Lumley (1972) believed that: *the energy transfer from large eddies to small eddies may be considered in terms of vortex stretching* p. 75; *vortex stretching is the only known spectral energy-transfer mechanism* p. 91; *the existence of energy transfer from large eddies to small eddies, driven by vortex stretching and leading to viscous dissipation of energy near the Kolmogorov microscale* p 256.

Nevertheless, Lumley wrote in (1992)

What part of modeling is in serious need of work? Foremost, I would say, is the mechanism that sets the level of dissipation in a turbulent flow, particularly in changing circumstances.

ing vortex stretching for being *the principal mechanical cause of the high rate of dissipation* following Taylor (1938).

Returning to the issue of *the principal mechanical cause of the high rate of dissipation,* i.e. to the true guilty party for the "dissipation" with related key aspects it is straightforward to observe from the Eqs. (7.1)–(7.2) below that (i) the *statistically dominant* terms (as e.g. in the sense of some balance, but not only) explicitly, i.e. $\omega_i\omega_j s_{ij}$ and $-s_{ij}s_{jk}s_{ki}$ plus (ii) the terms with the mean flow MF, pressure Hessian PH and external forcing EF. These latter are fairly small so that the former, i.e. $\omega_i\omega_j s_{ij}$ and $-s_{ij}s_{jk}s_{ki}$ are essentially *self-production* of the field of velocity derivatives, see Sect. 6.3 in Tsinober (2009) and references therein[7]

$$\frac{1}{2}\frac{D\omega^2}{Dt} = \omega_i\omega_j s_{ij} + \nu\omega_i\nabla^2\omega_i + MF + EF, \tag{7.1}$$

$$\frac{1}{2}\frac{Ds^2}{Dt} = -s_{ij}s_{jk}s_{ki} - \frac{1}{4}\omega_i\omega_j s_{ij} + \nu s_{ij}\nabla^2 s_{ij} + s_{ij}F_{ij} + MF + PH + EF. \tag{7.2}$$

Certain important aspects are noteworthy.

The enstrophy production $\omega_i\omega_j s_{ij}$ appears in RHS of the Eq. (7.2) for s^2 with the negative sign, *so that the vortex stretching is opposing the production of dissipation/strain: all instantaneous positive values of* $\omega_i\omega_j s_{ij}$ *make a* ***negative*** *contribution to the RHS in the equation for* s^2, *i.e. enstrophy production* $\omega_i\omega_j s_{ij}$ *has an additional role as drain of "energy" of strain, i.e.* s^2 *needed just for the enstrophy production and correspondingly appearing with the positive sign* in the Eq. (7.1)

Contrary to common beliefs, *the energy "cascade" (whatever this means, if anything) is associated primarily with the quantity* $-s_{ij}s_{jk}s_{ki}$, *rather than with the enstrophy production* $\omega_i\omega_j s_{ij}$ *and that vortex stretching suppresses the "cascade" and does not aid it. Moreover, it is the vortex compression, i.e.,* $\omega_i\omega_j s_{ij} < 0$, *that aids the production of strain/dissipation and, in this sense, the 'cascade' if such exists.*

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[7]The dominance of the terms $\omega_i\omega_j s_{ij}$ and $-s_{ij}s_{jk}s_{ki}$ appears (empirically) very robust in a broad range of Reynolds numbers Re_λ from 60 to 10^4 almost everywhere in the flow field except in irrotational regions where the pressure-Hessian acts instead to produce the strain, e.g. on the almost irrotational side of the entrainment interface Holzner et al. (2009) and close to the grid element centerline in the turbulence producing grid Paul et al. (2017)

Quantities directly related to the mean flow MF, pressure Hessian pH and external forcing EF in other – not "energy" equations are not negligible almost in any sense in a multitude of other aspects…and even in the equations below at least in several contexts different from the one discussed here.

Generally, the "energy" balance equations alone and alike are not "exhaustive" and cannot be saved from this malady by any "cascade". Also they require some closure, unless , e.g. using DNS of NSE and analyzing the data in terms of balance equations , e.g. scale by scale budgets etc , involving painful efforts in interpretation of the chosen statistical info. The terms $(\frac{1}{2})D\omega^2/Dt$, $(\frac{1}{2})Ds^2/Dt$ are not necessarily statistically dominant even in non-stationary turbulent flows, but play essential role and, obviously cannot be just neglected even if not statistically dominant.

Moreover, *all terms are (potentially) important if conditioned on different ranges of the normalized dissipation and/or enstrophy* Yeung et al. (2015), the latter being potentially true for multitude of other conditioning criteria.

Negative enstrophy production is associated with strong tilting of the vorticity vector and large curvature of vortex lines, which in turn are associated with large magnitudes of the negative eigenvalue, Λ_3, of the rate of strain tensor Kholmyansky et al. (2001), Tsinober (2000, 2009). *This is in full conformity with the fact that* Λ_3 *is responsible for most of the 'cascade'. i.e., predominant compressing rather than stretching! In other words since* $\omega_i\omega_j s_{ij}$ *is essentially a positively skewed quantity, its mean contribution to the strain production is negative!*

*Reiterating, the energy 'cascade' (whatever this means) is associated primarily with the quantit***y** $-s_{ij}s_{jk}s_{ki}$, *rather than with the enstrophy production* $\omega_i\omega_j s_{ij}$ *and that (in contrast to substantial beliefs!) vortex stretching suppresses the cascade and does not aid it without mentioning the claims that vortex stretching is the only known energy-transfer mechanism*[8] *Consequently, it is the vortex compression, i.e.* $\omega_i\omega_j s_{ij} < 0$, *that along with the strain production aids the production of strain/dissipation and, in this sense, the 'cascade'. One does not have to be confused that, e.g. for homogeneous turbulence* $\langle s_{ij}s_{jk}s_{ki}\rangle = -3/4\,\langle\omega_i\omega_j s_{ij}\rangle$ due to Betchov (1956) or even by the similarity of their PDFs (Fig. 7.2, top left), *since their pointwise relation is strongly nonlocal due to the nonlocal relation between vorticity and strain* Constantin (1994), Novikov (1967), Ohkitani (1994). *Consequently, locally they are essentially different, as can be seen from their joint PDF and scatter plots* (Fig. 7.5, top right): *they are only weakly correlated and there are a great many points with small* $\omega_i\omega_j s_{ij}$ *and large* $-s_{ij}s_{jk}s_{ki}$ *and vice versa*, for more see Tsinober (2009) Sect. 6.2.2 pp. 128–133.

The term $pH \equiv s_{ij}\frac{\partial^2 p}{\partial x_i \partial x_j}$, with $\Pi_{ij} \equiv \partial^2 p/\partial x_i \partial x_j$, is a divergence, so it is negligible in the mean in "locally"-homogeneous flows. However, $s_{ij}\Pi_{ij}$ is still of importance as, e.g. the PDF of $s_{ij}\Pi_{ij}$ is asymmetric and is positively skewed at large s^2, Tsinober (2000). The term $s_{ij}\Pi_{ij}$ becomes more important in inhomogeneous flows especially in purely irrotational inhomogeneous ones, e.g. in the nonlocal production of strain on the laminar side (not only in the proximity) of the turbulent non-turbulent "interface" in partly turbulent flows, Holzner et al. (2009) and Paul et al. (2017). It is noteworthy that the evolution equations for $\omega_i\omega_j s_{ij}$ and $-s_{ij}s_{jk}s_{ki}$ contain important nonlocal terms $\omega_i\omega_j\frac{\partial^2 p}{\partial x_i \partial x_j}$ and $s_{ik}s_{kj}\frac{\partial^2 p}{\partial x_i \partial x_j}$, see (7.5) and (7.6) below.

[8]The above is consistent with the results on a Lagrangian experiment using a 3D particle tracking velocimetry with access to velocity derivatives (Lüthi et al. 2005; Guala et al. 2006). Namely, it was found that the statistical evolution of strain and enstrophy can be interpreted as a kind of a life-cycle for strain and enstrophy and can be summarized in a sequence of processes starting with the strain self-amplification in low strain low enstrophy regions. This is followed by enstrophy production and growth, leading to the formation of high strain high enstrophy regions. The depletion of both strain and its production in parallel to the growth of enstrophy is related to the evolution of these regions into high enstrophy low strain regions, i.e. to the evolution of vortex sheets (shear layers) into vortex filaments. These regions evolve into weak enstrophy - strain regions since the enstrophy production, in presence of low strain and preferential alignment between ω and λ_2, cannot oppose the viscous destruction of enstrophy. This cyclic sequence consists of local and non-local processes of different Lagrangian time scales which governs the dynamics of small-scale turbulence.

An important subtler aspect is that the field of strain is efficient in the above two missions only with the aid of vorticity, i.e. only if the flow is rotational, since otherwise the strain (self-)production, $-s_{ij}s_{jk}s_{ki}$, for an irrotational flow field is just a divergence $s_{ij}s_{jk}s_{ki} = \partial\{...\}/\partial x_i$. In this context the vortex stretching is necessary to support the rotational nature of turbulence. Otherwise. e.g. it is the term $s_{ij}\frac{\partial^2 p}{\partial x_i \partial x_j}$ which is mainly responsible for the strain production on the laminar side of the turbulent non-turbulent "interface" in partly turbulent flows, see the upper part of Fig. 1.1. The above does not exhaust the roles of strain, see Sect. 6.22 in Tsinober (2009).

The first essential point above is that the rotational and dissipative nature of turbulence is due to the process of self-amplification of the field of velocity derivatives. There is a conceptual and qualitative difference between the nonlinear interaction between vorticity and strain, e.g. $\omega_i\omega_j s_{ij}$ *and the self-amplification of the field of strain,* $-\ s_{ij}s_{jk}s_{ki}$*, which is a specific feature of the dynamics of turbulence having no counterpart (more precisely analogous – not more) in the behavior of passive and active objects. This process* (i.e., $-s_{ij}s_{jk}s_{ki}$) *is* ***local*** *in contrast to* $\omega_i\omega_j s_{ij}$*, as the field of vorticity and strain are related* nonlocally.

Unlike the strain self-production the interaction of vorticity and strain involve important issues of geometrical nature which are complicated by the nonlocal relation between them. For example, $\omega_i\omega_j s_{ij} = \omega^2\Lambda_k\cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_k)$ so that the enstrophy production is essentially dependent on (i) the magnitude of ω^2, (ii) the eigenvalues Λ_i of the rate of strain tensor s_{ij} ($\Lambda_1 > \Lambda_2 > \Lambda_3$; due to incompressibility $\Lambda_1 + \Lambda_2 + \Lambda_3 = 0$ so that $\Lambda_1 > 0$, $\Lambda_3 < 0$) , (iii) the alignments between vorticity $\boldsymbol{\omega}$ and the eigenframe $\boldsymbol{\lambda}_i$ of the rate of strain tensor s_{ij} and correlations between the three (i)–(iii). It appears that the main contribution to the enstrophy production and its rate is due to the first term associated with the $\boldsymbol{\omega}, \boldsymbol{\lambda}_1$ alignment, see Tsinober (2009) and references therein. For example, in the field experiments with $Re_\lambda \approx 10^4$ the relation between the mean of the three contributions $\langle\omega^2\Lambda_1\cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_1)\rangle : \langle\omega^2\Lambda_2\cos^2(\boldsymbol{\omega}, \lambda_2)\rangle : \langle\omega^2\Lambda_3\cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_3)\rangle = 3.1 : 1.0 : -2.1$. The dynamical dominance of the term associated with the $\boldsymbol{\omega}, \boldsymbol{\lambda}_1$ alignment is much stronger for the corresponding rates, i.e. $\omega_i\omega_j s_{ij}/\omega^2 = \Lambda_k\cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_k)$; $\langle\Lambda_1\cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_1)\rangle : \langle\Lambda_2\cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_2)\rangle : \langle\Lambda_3\cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_3)\rangle = 4.9 : 1.0 : -3.8$, which exhibits far stronger role of strain and $\boldsymbol{\omega}, \boldsymbol{\lambda}_i$ alignments. For PDFs of involved quantities see Fig. 7.2.

Similar results were obtained for lower Reynolds numbers in grid flows ($Re_\lambda \approx 10^2$) and recently in a circular pipe flows at the CICLOPE facility ($Re_\lambda \approx 4 \cdot 10^2$)

It therefore appears that the flow patterns with ω, λ_1 *alignment are dominant in enstrophy production in spite of the well known observed overall statistical predominance of the* ω, λ_2 *alignment.* In other words, the predominant vortex stretching is indeed due to alignment $\boldsymbol{\omega}, \boldsymbol{\lambda}_1$, but for this there is no need for the statistical predominance of this alignment as generally expected, since statistical dominance is not synonymous to dynamical relevance.[9] This apparent "contradiction" is resolved by noting that: (i) the intermediate eigenvalue, Λ_2, assumes both positive and nega-

[9]This expectation takes its origin mainly from the incorrect analogy with material lines and other passive vectors, see Sect. 9.4 Vorticity versus passive vectors in Chap. 9 on analogies and misconceptions in Tsinober (2009). In particular, the nonlocality of the relation between vorticity and strain

Fig. 7.2 **a** Histograms of the total rate of enstrophy production $\omega_i\omega_j s_{ij}/\omega^2$ (EPR) and separate contributions $\Lambda_k \cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_k)$, $k = 1, 2, 3$. It clearly seen that the main contribution to the total on the positive part comes from $\Lambda_1 \cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_1)$. **b** Conditional averages of $EPR = \omega_i\omega_j s_{ij}/\omega^2$ on $\cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_k)$. We preferred to use the evidence as obtained for real physical fields to avoid any abuse by some additional processing such as decompositions, etc. We stress that this is qualitatively different from the $\boldsymbol{\omega}, \boldsymbol{\lambda}_1$ alignment for some "parts" of the flows processed in some way, e.g. by removing some 'local' part of strain or using band pass filtering, see Kevlahan and Hunt (1997), Porter et al. (1998) Hamlington et al. (2008), Leung et al. (2012)

tive values thus reducing the terms $\langle \omega^2 \Lambda_2 \cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_2)\rangle$ and $\langle \Lambda_2 \cos^2(\boldsymbol{\omega}, \boldsymbol{\lambda}_2)\rangle$ which involve both stretching and slightly less compressing; whereas Λ_1 is positive; and (ii) the magnitude of Λ_1 is much larger, e.g. $\langle \Lambda_1^2\rangle : \langle \Lambda_2^2\rangle : \langle \Lambda_3^2\rangle = 10.2 : 1.0 : 13.7$, see Table 6.6 and Fig. 6.7 in Tsinober (2009).

The above is clearly seen from Fig. 7.3ab showing the $\boldsymbol{\omega}, \boldsymbol{\lambda}$ alignments conditioned on $\omega_i\omega_j s_{ij} > 0$ and $\omega_i\omega_j s_{ij} < 0$.

We would like to recapitulate the qualitative differences between the enstrophy and strain production. It is the strain production (rather than vortex stretching) that is directly responsible for the enhanced dissipation of turbulent flows and it is a local process with predominant compressing whereas the enstrophy production is a nonlocal process with predominant stretching. To emphasize the point, vorticity is commonly given more stress whereas in reality strain and vorticity are almost "equal partners". Almost because it is the strain production that plays the role of an engine producing the whole field of velocity derivatives provided that the flow field is not entirely vorticity-free.

The above answers only the first part of the question as formulated by Taylor (1938) concerning *the principal mechanical cause of the high rate of dissipation which is associated with turbulent motion* and *this is the strain self-amplification* $-s_{ij}s_{jk}s_{ki}$. The second part of the question is found, e.g. in Lumley (1992): *What*

- which does not have an analogue with passive vectors - play an important role in the issue of $\boldsymbol{\omega}, \boldsymbol{\lambda}_i$ alignments. Hamlington et al. (2008).

An important general aspect is that the strongest interaction between vorticity and strain occurs in regions with $\boldsymbol{\omega}, \lambda_1$ alignments and large strain, see Chap. 6, pp. 150–153 in Tsinober (2009).

Fig. 7.3 PDFs of ω, λ alignments conditioned on positive **a** $\omega_i\omega_j s_{ij} > 0$ and negative **b** $\omega_i\omega_j s_{ij} < 0$. There is no (statistical) ω, λ_1 alignment for the whole field and for $\omega_i\omega_j s_{ij} < 0$, but there is essental ω, λ_1 alignment for $\omega_i\omega_j s_{ij} > 0$. Note the strong $\boldsymbol{\omega}, \boldsymbol{\lambda}_2$ alignment for all situations including $\omega_i\omega_j s_{ij} < 0$ as well

part of modeling is in serious need of work? Foremost, I would say, is the mechanism that sets the level of dissipation in a turbulent flow, particularly in changing circumstances, which is not less important from the basic point of view either. The answer comes from the Eq. (7.2) - it is due to the "dissipation" of the strain production $-\nu(\partial s_{ij}/\partial x_k)(\partial s_{ij}/\partial x_k)$, the only *mechanism that sets the level of dissipation in a turbulent flow.* At large Reynolds numbers one obtains a relation $\left\langle -s_{ij}s_{jk}s_{ki}\right\rangle \approx \nu\left\langle(\partial s_{ij}/\partial x_k)(\partial s_{ij}/\partial x_k)\right\rangle$ analogous to the Tennekes and Lumley balance for the enstrophy production, see Tsinober (2009). The above answer does not provide anything like an upper bound, see Doering (2009) and references therein. This explains the continuing interest in the question of scaling of the mean dissipation $\epsilon = \langle\varepsilon\rangle$ since Taylor (1935) who proposed $\epsilon \approx C_\epsilon U^3/L$ with $C_\epsilon \approx const$ with all the problems of defining the remaining quantities (i.e. velocity, length scale) depending or not on the BC's, IC's, forcing, etc. This also led to a host of discussions resulting from the temptation to use universality and searching for, e.g. phenomenological means, etc. such as "cascades". In this context this appeared not so useful, e.g. Vassilicos (2015), but see also Djenidi et al. (2017b) where the above estimate was obtained employing the assumption of self-preservation.

The bottom line here is that on the paradigmatic level it is the nonlinearity that is responsible for most such basic key properties of 3-D turbulence as essentially rotational and strongly dissipative phenomenon due to the self-amplification of strain and vorticity $\omega_i\omega_j s_{ij}$ and $-s_{ij}s_{jk}s_{ki}$ - the most important quantities representing genuinely nonlinear processes in turbulence such as the excitation of small scales due to the nonlinearity of the NSE and making turbulence not amenable to quasi-nonlinear approaches such as RDT and alike.

Reiterating, in contrast to Taylor (1938), see also Taylor (1917):

• The important point that *the principal mechanical cause of the high rate of dissipation which is associated with turbulent motion is the generic mechanism of strain self-amplification of strain (SAS)* $-s_{ij}s_{jk}s_{ki}$ which is one of the two nonlocally connected and weakly correlated processes - $\omega_i\omega_j s_{ij}$ and $-s_{ij}s_{jk}s_{ki}$.

• Moreover, vortex stretching ($\omega_i\omega_j s_{ij} > 0$) is opposing whereas vortex compressing ($\omega_i\omega_j s_{ij} < 0$) is aiding production of strain/dissipation.

• In contrast to enstropy production SAS is a local process.

• SAS does not require a large scale flow and there is no need for a fictitious "cascade" to 'deliver' the energy from LS to SS. It is independent of existence (or not) of such a cascade, but depends on specific factors, such as the nature of turbulence excitation (LS, SS, broadband, etc - which may exclude "cascades" at all from the outset) and on BC'c and IC'c. There no need for two-point statistics either.

• SAS is a specific feature of genuine turbulence having no counterpart as, e.g. in the behavior of passive objects.

• Enstrophy production $\omega_i\omega_j s_{ij}$ is important in aiding strain since in an irrotational flow $s_{ij}s_{jk}s_{ki} = \{\ldots\}/\partial x_i$.

7.3 Nonlocality

The term nonlocality is used here in several related meanings which will become clear in the course of discussing of the issues throughout this section and in the sequel. This includes anisotropy especially in small scales, statistical dependence of small and large scales, nonlocality versus decompositions, intermittency and structure(s), helicity, flows with additives, memory effects, flow history and predictability and effects of boundary including inflow conditions, closures and nonlocal relations between resolved and unresolved scales and constitutive relations and some other related issues. These are described, in Tsinober (2009), Sects. 1.3.5 and 6.6. Here we concentrate on the issues which touch upon paradigmatic issues with some update. For example, the inherent property of nonlocality is questioning the validity of the hypotheses on local homogeneity/isotropy in bounded flow domains surrounded by nonhomogeneous/nonisotropic flow regions.

7.3.1 Introductory/General Remarks

Turbulence is a generically nonlocal process. One can already see clear hints to this from the term $(\mathbf{u}\cdot\nabla)\mathbf{u} \equiv \boldsymbol{\omega}\times\mathbf{u} + \nabla(u^2/2)$ and terms appearing in a number of precise consequences of NSE such as $(\boldsymbol{\omega}\cdot\nabla)\mathbf{u} \equiv \partial/\partial x_j\{u_i\omega_j\}$ - so it is natural to call $u_i\omega_j$ as helicity tensor, $\nabla^2 p = \rho(\omega^2 - 2s_{ij}s_{ij}) = -\rho\frac{\partial^2 u_i u_j}{\partial x_i \partial x_j}$ among many others.

Nonlocality is a generic internal property of turbulent flows and exists independently of the presence of mean shear or other external factors, but has different manifestations for different external factors. For example, in the presence of a mean shear the small scales become anisotropic, whereas if the small scales are artificially excited the overall dissipation and mixing rate of the turbulent flow increase substantially. The direct interaction/coupling of large and small scales is in full conformity and is the consequence of the generic property of Navier–Stokes equations, which are integro-differential. It appears that the Kolmogorov 4/5 law can be interpreted as one of the manifestations of nonlocality in the above sense, see Sect. 8.1 below. Nonlocality is associated also with 'kinematics' due to the nonlocal relations between, e.g. velocity and its increments and between vorticity and strain. The non-local nature of the inertial nonlinearity is clearly seen looking at purely Lagrangian setting in which the inertial nonlinearity consists of the term with pressure only. We point out that from the formal point of view a process is called local if all the terms in the governing equations are differential. If the governing equations contain integral terms, then the process is nonlocal. The Navier–Stokes equations are *integro-differential* for the velocity field in both physical and Fourier space or any other. Therefore, generally, the Navier–Stokes equations describe nonlocal processes. Indeed, since $\nabla^2 p = \rho(\omega^2 - 2s_{ij}s_{ij}) = -\rho\frac{\partial^2 u_i u_j}{\partial x_i \partial x_j}$, the relation of pressure and velocity is nonlocal due to the nonlocality of the operator ∇^{-2}. This aspect of nonlocality is strongly associated with the essentially non-Lagrangian nature of pressure. It is therefore it is not surprising that replacing in the Euler equations the pressure Hessian $\Pi_{ij} \equiv \frac{\partial^2 p}{\partial x_i \partial x_j}$, which is both nonlocal and non-Lagrangian, by a local quantity $\delta_{ij}\nabla^2 p = \rho/2\{\omega^2 - 2s_{ij}s_{ij}\}$ turns the problem into a local and integrable one. This allows integration of the equations for the invariants of the tensor of velocity derivatives $\partial u_i/\partial x_j$ in terms of a Lagrangian system of coordinates moving with a particle, see Cantwell (1992), Meneveau (2011) and references therein. One of the reasons for the disappearance of turbulence (and formation of singularity in finite time) in such models, called restricted Euler models, is that the eigenframe of s_{ij} in these models is *fixed* in space, Novikov (1990b), whereas in a real turbulent flow it is oriented 'randomly' in space and time. This means that nonlocality due to pressure is essential for (self-)sustaining turbulence: no pressure Hessian – no turbulence.

In view of special/paradigmatic status of the strain and enstrophy production it is of interest to mention that the pressure Hessian Π_{ij} enters the equations for the rate of change of strain and enstrophy production, i.e. $D(s_{ij}s_{jk}s_{kj})/Dt$ and $D(\omega_i\omega_j s_{ij})/Dt$ respectively, in the form $s_{ij}s_{jk}\Pi_{ij}$, $\omega_i\omega_j\Pi_{ij}$. This reflects the interaction between strain and vorticity and the pressure Hessian with a nonlocal contribution stemming from the deviatoric part of the pressure Hessian, see Eqs. (7.5) and (7.6) below.

The fluid particle acceleration $\mathbf{a} \equiv D\mathbf{u}/Dt$ - a kind of small scale quantity, which is dominated by the pressure gradient, ∇p and thereby the fluid particle acceleration is also related in a nonlocal manner to the velocity field. Hence the scaling properties of the acceleration variance do not obey Kolmogorov-like scaling. From the kinematic point $\mathbf{a} \equiv D\mathbf{u}/Dt = \partial\mathbf{u}\backslash\partial t + (\mathbf{u}\cdot\nabla)\mathbf{u} = -\frac{1}{\rho}\nabla p + \nu\nabla^2\mathbf{u} = \partial\mathbf{u}\backslash\partial t + \boldsymbol{\omega}\times\mathbf{u}$

$+\nabla(u^2/2)$, i.e. it is a 'mixed' quantity due to presence of both velocity and velocity derivatives . Thus the impact of nonlocality on the behavior of acceleration.

There is a large set of fluid flows with "stronger" nonlocality. These are flows possessing additional mechanisms as the ability of supporting waves such as rotating, stably stratified and magnetohydrodynamic. Flows with special properties related to helicity such as with helical forcing are also of this kind.

Though turbulence is inherently nonlocal there are attempts to single out at least some aspects which are in some sense "local" by putting forward various hypotheses.

The main reason for such attempts becomes clear because nonlocality is among the main reasons for the absence of a sound theory of turbulence based on first principles. This state of affairs is not unique for turbulence. Landau (1960) wrote: *It is well known that theoretical physics is at present almost helpless in dealing with the problem of strong interactions... and that it is necessary to consider "distributed", non-local, interactions… Unfortunately, the non-local nature of the interaction renders completely useless the technique of the present existing theory*(ies).

It is probably one of the reasons why Kolmogorov (1985) wrote: *I soon understood that there was little hope of developing a pure, closed theory, and because of absence of such a theory the investigation must be based on hypotheses obtained on processing experimental data.*

Other reasons are more "practical". With nonlocality it is far from trivial if not impossible to use the experimental data - which are all limited in space and time - for "validation" of theoretical developments for, e.g. homogeneous flows, i.e. in 'infinite' domains. Also, locality is necessary for the 'physical foundation' of large-eddy simulation (LES) modeling of turbulence.

Even just looking at the equations for the small/unresolved scales it is straightforward to realize that the small/unresolved scales depend on the large/resolved scales via nonlinear space and history-dependent functionals, i.e. essentially non-local both spatially and temporally. It is therefore unlikely - and there is accumulating evidence for this - that relations between them (such as "energy flux") would be approximately local in contradiction to K41a hypotheses and surprisingly numerous efforts to support their validity. The other side of the nonlocality is the equally important back reaction of the small scales on the rest of the flow as will be seen below.

7.3.2 On Locality and Related

Before proceeding on the subject of the nonlocal nature of turbulence several comments are given here on the issue of relevance and utility of locality in basic turbulence research invariably claiming things like *the general idea that the Navier–Stokes equations are local PDEs in physical...space,* Jiménez et al. (2017). The first attempt of this kind was made by von Kármán (1937) and von Karman and Howarth (1938) as an attempt to "solve" the famous Karman–Howarth (KH) equation, containing both second and third order correlations. This required a hypothesis to close the KH equation which is known as the so-called self-preservation (and/or self-similarity)

hypothesis which is in reality a kind of "hidden closure". The main physical basis for such a closure is the assumption that the turbulent motion at some point in time and space is defined by its immediate proximity. There are a number of publications on the issue of locality and related, see Sect. 16 in Monin and Yaglom (1975), George (2012), Klewicki et al. (2014), Djenidi et al. (2017a, b), Yasuda and Vassilicos (2018) and references therein, although from the fundamental point of view there is little justification for the above if any. Among the most popular is the "hope that at least part of its (i.e. turbulence) dynamics can be described in terms of a relatively small number of more elementary processes than the full Navier-Stokes equations and that the evolution of the flow is largely local, and to look for solutions that are intense enough to evolve on their own relatively independently from other solutions far away", Jimenez (2018). However, from the basic point "hope" and the like are insufficient and lead to misconceptions and interpretational abuse concerning "cascades", (mis)use of two-point statistics, dissipation anomaly, "coherent structures" and strong/intense events, etc.

The next attempt of this kind was made by Kolmogorov (1941a) putting forward the statistical hypothesis of local homogeneity and isotropy: ... *we think it rather likely that in an arbitrary turbulent flow with sufficiently large Reynolds number* $Re = \frac{LU}{\nu}$ *the hypothesis of local isotropy* (i.e. small regions in the four dimensional space-time space) *is realized with good approximation in sufficiently small regions... not lying close to the boundaries of the flow or its other special regions.* It is noteworthy that the hypothesis was formulated for *an arbitrary (i.e. not homogeneous isotropic, etc.) turbulent flow with sufficiently large Reynolds number* $Re = \frac{LU}{\nu}$. The **consequence** of the above assumptions, i.e. both local homogeneity and isotropy and large Reynolds numbers is that the flow is in *approximate statistical equilibrium.* This is the reason that the K41 theory was called by Batchelor (1953) an "universal equilibrium theory". The result in the community was to erroneously ascribe to Kolmogorov a third assumption on equilibrium with the birth of the term "non-equilibrium turbulence" causing quite a bit of confusion until recently, see above Sect. 6.1.2 Validation or theories versus hard evidence. The attraction of the Kolmogorov hypotheses is that they concern the local properties of any turbulence, not necessarily homogeneous or isotropic or decaying or stationary, provided the Reynolds number is large enough. The consistency of these hypotheses has been debated for quite a while both from the theoretical point of view, Gkioulekas (2007), Hill (2006) and references therein, but also experimentally, as with other similar issues without much progress from the fundamental point ov view. One of the main problems that escaped the community is the inherent property of nonlocality questioning the validity of the above hypotheses on local homogeneity/isotropy in bounded flow domains (though "not lying close to the boundaries of the flow or its other special regions") surrounded by nonhomogeneous/nonisotropic flow regions. In particular, the "quasi-homogeneous" behavior of some quantities in bounded regions is misleading due to several factors, the main being the nonlocal nature of turbulence. From the conceptual point the nonlocality makes local homogeneity, isotropy, etc. impossible unless the whole (infinite extent) flow is such, which is trivially impossible. A final note is that the idealized configurations such as global homogeneity, etc. are attractive for theoretical approaches, but they are useful for systems with dominating properties of local nature. This seems

to be not the case with turbulence. Hence the problem with the utility of idealized configurations, not to mention that they were not so useful in view of the absence of theory from first principles.

Locality is assumed in all treatments of "cascades" starting from Onsager (1945, 1949), for later references see Eyink and Drivas (2018), Jiménez et al. (2017), see also the meetings on Turbulent Cascades in Lille 2015 and Lyon 2017 and references therein.[10] This is done via employment of some decomposition and essential assumptions on the existence of inertial range as defined by Kolmogorov (1941a) and locality of interactions in the cascade between components of similar magnitude. Most of these concern how local the energy transfer (spectral energy flux) is with invariable claims on its locality in a statistical sense. This in spite of the general expectation on nonlocality of interactions, which was convincingly demonstrated by, e.g. Laval et al. (2001) on an example of one particular decomposition of the flow field. We mention also the so called "local equilibrium approximation" in wall bounded flows such that "statistically energy is dissipated close to where it is produced" at some locations along the normal to the wall, McKeon and Morrison (2007), Marusic et al. (2010), Smits et al. (2011), Jimenez (2012).

As mentioned the motivation for locality at least in some sense is understandable, see above the quotation by Landau (1960), but the reality - which is the consequence of the nonlocal nature of turbulence - does not seem to fit the predominantly wishful thinking on this issue. This is seen to some extent from what follows in the sequel. The bottom line is that generally locality cannot be assumed due to the inherently non-local nature of turbulence. For more on the issue of nonlocality as discussed above, see Tsinober (2009), Sect. 6.6, pp. 163–182. To put this differently, locality belongs to the same category as "small parameters", etc. No theory based on some locality, small parameters, RDT etc., has any chance to succeed, though attempts to do so still continue to the present day, see McKeon (2017) and references therein.

There is some evidence that the K41 locality hypothesis at very large Reynolds numbers breaks down at least in some flows. If so the guilty party is the inherent nonlocality of turbulent flows, see below and Sect. 6.6 in Tsinober (2009).

7.3.3 A Simple Example of Paradigmatic Importance Lacking Properties of Local Isotropy and Homogeneity at Large Reynolds Numbers

We start from a simple example. Taking the position that velocity fluctuations represent the large scales and the velocity derivatives represent the small scales, one can state that, in homogeneous (not necessarily isotropic) flows the large and the small scales do not correlate. This can be expressed quantitatively by a correla-

[10]There is a kind of tautological aspect since "cascade" by definition is local, i.e. the consequence of assuming a cascade implies that the Navier–Stokes equations are local. The latter is generally not correct in any space - be it physical, Fourier or any other. This is one of the main themes of the present section.

Fig. 7.4 Dependence of the mean Reynolds stress $\langle u_1 u_2\rangle$ on the distance from the wall in turbulent flows in channels of cross section with large aspect ratio. Adapted from Wei and Willmarth (1989), see also Shen and Warhaft (2000) and Ferchichi and Tavoularis (2000)

tion between velocity and vorticity. For example, in a homogeneous turbulent flow the Lamb vector $\langle \boldsymbol{\omega} \times \mathbf{u}\rangle = 0$ and also $\langle (\mathbf{u}\cdot\nabla)\mathbf{u}\rangle = 0$. If the flow is statistically reflectionally symmetric, then $\langle \boldsymbol{\omega}\cdot\mathbf{u}\rangle = 0$ as well. However, as mentioned, vanishing correlations do not necessarily mean absence of dynamically important relations. Indeed, the quantities $(\mathbf{u}\cdot\nabla)\mathbf{u} \equiv \boldsymbol{\omega}\times\mathbf{u} + \nabla(u^2/2)$ and $\boldsymbol{\omega}\times\mathbf{u}$, are the main 'guilty parties' responsible for all we call turbulence. Both contain the large scales (velocity) and small scales (velocity derivatives, strain, vorticity). Therefore some kind of essential coupling between the two is more than unavoidable.

To illustrate the dynamical nature of this coupling we consider a unidirectional *in the mean* fully developed turbulent shear flow, such as the flow in a plane channel in which all statistical properties depend on the coordinate normal to the channel boundary, x_2, only. In such a flow, a simple precise kinematic relation is valid

$$d\langle u_1 u_2\rangle/dx_2 \equiv \langle \boldsymbol{\omega}\times\mathbf{u}\rangle_1 = \langle \omega_2 u_3 - \omega_3 u_2\rangle \neq 0,$$

which is just a consequence of the vector identity $(\mathbf{u}\cdot\nabla)\mathbf{u} \equiv \omega\times\mathbf{u} + \nabla(\frac{u^2}{2})$ in which incompressibility and $d\langle\cdots\rangle/dx_{1,3} = 0$ where used, and $\langle\cdots\rangle$ means an average in some sense (e.g. time or/and over the planes $x_2 = const$, etc.). The *dynamic* aspect is that in turbulent channel flows $d\langle u_1 u_2\rangle/dx_2 \neq 0$ is essentially different from zero at *any arbitrarily large* Reynolds number as far as the data allow us to make such a claim, see Fig. 7.4.

One can therefore see from the above equation that at least some correlations between velocity and vorticity in such flows are essentially different from zero, see also Priyadarshana et al. (2007) and references therein. The important point is that without these correlations the mean flow would not "know" about the fluctuations at all however small the corresponding correlation coefficients.

Let us look at the properties in the proximity of the midplane, $x_2 \approx 0$, of the turbulent channel flow. In this region $dU/dx_2 \approx 0$ and $\langle u_1 u_2\rangle \approx 0$, but - contrary to common assumptions *the flow is neither homogeneous nor isotropic even at the level of velocity derivatives*, since the gradient $d\langle u_1 u_2\rangle/dx_2$ is essentially $\neq 0$ and is *finite independently of the Reynolds number*. This is also a clear indication of nonlocality, since in the bulk of the flow, i.e. *far from the boundaries*, $dU/dx_2 \sim 0$,

a clear counterexample to the hypothesis of the local isotropy: even in the proximity of the centerline of the channel this hypothesis does not hold for whatever large Reynolds numbers. The absence of the $k^{-5/3}$ spectrum at the highest available Reynolds numbers, $Re = 230000$, based on the mean velocity and half channel width Compte-Bellot (1965) is consistent with the above. It is noteworthy that the presence of two walls is essential.

Similar behaviour of Reynolds stress is observed in turbulent flow in a circular pipe so that the assumptions on local isotropy in the proximity of flow axis as made by Morrison et al. (2016) are not justified.

We point out that in the above example (as in many others) one does not have to decide that everything in this flow should be anisotropic: there are examples of almost isotropic behavior. Among the latest see Johnson et al. (2017) for a turbulent channel flow in which *above 100 viscous units from the wall, stretching statistics are equal to their isotropic values, in support of the local isotropy hypothesis*.

Among the most important points is that the inherent property of nonlocality is questioning the validity of the hypotheses on local homogeneity/isotropy in bounded flow domains (though "not lying close to the boundaries of the flow or its other special regions") surrounded by nonhomogeneous/nonisotropic flow regions, i.e. the "quasi-homogeneous" behavior of some quantities in such bounded regions is misleading. From the conceptual point the nonlocality makes local homogeneity, isotropy, etc. impossible unless the whole (infinite extent) flow is such, which is trivially impossible, i.e., generally one cannot apply results for globally homogeneous, isotropic turbulent flows to flows which are approximately homogeneous/isotropic in a bounded region of flows and otherwise are non-homogeneous/non-isotropic. In other words the idealized configurations such as global homogeneity, etc. are attractive for theoretical approaches, but they are useful for systems with dominating properties of local nature. This seems to be not the case with turbulence. Hence the problem with the utility of idealized configurations, not to mention the absence of any theory from first principles anyhow.

7.3.4 Direct and Bidirectional Coupling Between Large and Small Scales

Intermittency is an important feature of the common nonlocal life of large and small and scales/structure(s)

By nonlocality is meant (among other things) the direct an bidirectional coupling between large (resolved) and small (unresolved) scales, see Chap. 6 in Tsinober (2009) and below Chap. 9. This generally accepted division of turbulent flows into large and small scales (and similarly into larger number of "regimes") is to a large extent artificial and in some sense even unphysical. This is due to the strong coupling between the two and to the multitude of problems/ambiguities defining the term 'scale' along with the problematic nature of the decomposition approach to the phenomenon of turbulence and the necessity of handling turbulence as a whole in one sense or another. Similarly, the "decomposition" in more "ranges" such as:

the injection regime in which the energy injection rate dominates the right hand side of NSE, the energy flow regime in which the flow rates dominate the righthand side of NSE, and the dissipation regime in which the dissipation side of NSE, is to the same degree artificial mainly due to the role of nonlocal effects making such decompositions useless.

Here we concentrate on nonlocality as manifested in direct and bidirectional interaction/coupling between large and small scales. We use the term "coupling" rather than "correlation" due to the essential difference between the two which are not synonymous (see Sect. 5.2 above) as claimed until now, see e.g. Pumir et al. (2016).

There exists massive evidence that the above really is the case as there are many indications that this interaction is bidirectional. We should first mention the well known effective use of fine honeycombs and screens in reducing large scale turbulence in various experimental facilities, Laws and Livesey (1978), Tan-Attichat et al. (1982). The experimentally observed phenomenon of strong drag reduction in turbulent flows of dilute polymer solutions and other drag reducing additives is another example of such a 'reacting back' effect of small scales on the large scales. Further, one can substantially increase the dissipation and the rate of mixing in a turbulent flow by *directly* exciting the small scales experimentally, e.g. in a jet and in DNS in a periodic box, Suzuki and Nagano (1999), Vukasinovich et al. (2010). Similar effects are observed with small-scale acoustic excitation. Moreover, recent developments in wall bounded shear flows revealed the importance of the interactions of structure(s) in the flow, such as inner-outer interactions including the large-small scales, generally, and with the nearwall region, see e.g. Klewicki (2010), Marusic et al. (2010), Smits et al. (2011), Jimenez (2012), Dogan et al. (2016), Pathikonda and Christensen (2017), Takuya Kawata and Alfredsson (2018).

As one of the premises we would like to point out that vorticity and strain are not just velocity derivatives and not just "small scales". They are special for several reasons. As discussed above they reflect the rotational and dissipative nature of turbulence. In the context of nonlocality the property to be stressed here is that the whole flow field is determined entirely by the field of strain and/or vorticity with appropriate boundary conditions

$$\nabla^2 u_i = 2\partial s_{ij}/\partial x_j;\ \nabla^2 \mathbf{u} = -curl\boldsymbol{\omega},$$

i.e. the velocity field is a linear functional of strain and/or vorticity, see Tsinober (2009), Eqs. C.14, C.14'

$$u_i = \mathfrak{G}\{s_{ij}\};\quad \mathbf{u} = \mathfrak{F}\{\omega\} \tag{7.3, 7.4}$$

i.e. the field of velocity $\mathbf{u}(\mathbf{x},t)$ in each space point $\mathbf{x}$ is defined by the whole field of strain and/or vorticity. It is a kinematic relation and as such is independent of time and forcing. Alteration of the field of velocity derivatives reflects on the velocity field, i.e. vorticity and strain are not passive – they react back and not only for the above (kinematic) reason. This in turn means that the large scales as represented by the velocity field and the small scales as represented by vorticity and strain, for purely

kinematic reasons should be and are strongly coupled (which is not the same as being correlated), as indeed is the case. However, this is only the kinematic aspect of the issue.[11] The important aspect along with the *kinematic* relations above $u_i = \mathfrak{G}\{s_{ij}\}$ (and $\mathbf{u} = \mathfrak{F}\{\boldsymbol{\omega}\}$) is the *dynamic* one: the *self-amplification* of strain $-s_{ij}s_{jk}s_{kj}$ and enstrophy $\omega_i\omega_j s_{ij}$ 'reacts back' in creating the corresponding velocity field leading to the dynamic aspects of the coupling of the fields of velocity and velocity derivatives[12] including the coupling between the kinetic energy and dissipation in Newtonian fluids with dissipation $\epsilon = 2\nu s_{ij}s_{ij}$.[13] This means that the small scales are not just 'swept' by the large ones. Therefore, it is incorrect to treat the small scales as a kind of passive object (e.g. passive sink of energy) just 'slaved' to the large scales (e.g. the velocity field), which are not decoupled from the small ones nor vice versa. Moreover, at the level of velocity derivatives the inertial, $-s_{ij}s_{jk}s_{kj}$, $\omega_i\omega_j s_{ij}$ and viscous terms, $\nu\omega_i\nabla^2\omega_i$, $\nu s_{ij}\nabla^2 s_{ij}$, do not act as if they were additive and independent - their interaction is crucial and among other things is manifested in the (approximate) Tennekes and Lumley (1972) balance for the enstrophy production and a similar relation for strain production (7.1a, 7.2a), see Sect. 6.3 in Tsinober (2009). For a statistically stationary and homogeneous flow with large scale forcing

$$0 \approx \langle\omega_i\omega_j s_{ij}\rangle - \nu\left\langle(\partial\omega_i/\partial x_k)^2\right\rangle \quad 0 \approx -(2/3)\langle s_{ij}s_{jk}s_{ki}\rangle - \nu\left\langle(\partial s_{ij}/\partial x_k)^2\right\rangle \tag{7.1a, 7.2a}$$

An important aspect is that this balance appears to be valid in different meanings (not only in the mean) as follows. The first feature that the TL balance holds at Re_λ as low as ≈60. Second, it holds pointwise in time, i.e. the integrals over the flow domain of the enstrophy production and of its viscous destruction are approximately balanced at any moment of time, $\int \omega_i\omega_j s_{ij} dV \approx -\nu \int \omega_i \nabla\omega_i dV$. The features concerning the TL balance appear to be true for temporally modulated turbulent flows and for flows with hyperviscosity of different orders, $h = 2, 4, 8$, with $h = 1$ corresponding to Newtonian fluid. The approximate balance as described above between the 'inertial' and diffusive processes shows that they are very far from being additive and point to strong mutual interaction. In particular, this is a strong indication that the nature of dissipation is important in the enstrophy/strain production and the properties of

[11] This cannot be underestimated for several reasons. The main is that the Eqs. (7.2, 7.3) imply a nontrivial relation between the fields of velocity (energy) and that of strain (dissipation). One of the problems here is that in real fluids the role of the strain field is not limited just by dissipation with a number of other important functions. One of the consequences is that the nature of dissipation makes an essential qualitative difference including nonlocal effects so that different dissipative mechanisms result in different outcomes including the "inertial range" contrary to common belief. Hence problems in interpretation of results when replacing Newtonian viscous term by some "surrogate" such a the hyperviscous one. For example, this concerns studying properties of "inertial range" in fluids with hyperviscosity, Barjona and da Silva (2017), and the so called truncated Euler equations with the smaller scales considered as thermalized modes, Frisch (2008).

[12] Hence the problematic character of the claims like "physically justified approximation that velocity **u** and strain **S** are uncorrelated" because "In a turbulent flow, the velocity field **u** and the rate of strain **S** are expected to be independent of each other" Pumir et al. (2016).

[13] In non-newtonian fluids the issue is more complicated, e.g. in hyperviscous case the viscous dissipation $\epsilon = \nu_h(\nabla^h\mathbf{u})$.

the vorticity/strain in the whole flow field. This in turn means that it should also be important in the properties of the velocity as the latter is fully determined by the field of vorticity/strain. We return to these issues below, also for more on this see Tsinober (2009), Chap. 6, pp. 135–141. Note that Eq. (7.1a, 7.2a) indicate that self-amplification of vorticity and strain is influenced by the nature of dissipation since, e.g. in hyperviscous cases the viscous terms in (7.1a, 7.2a) are different for different values of h.

It is also noteworthy, that one of the claims is that the also TL balance is the reason for the predominant enstrophy (and strain) production. This argument is misleading and puts the consequences before the reasons, since it is known that, for Euler equations, enstrophy production increases very rapidly with time, apparently without limit, see references in Tsinober (1998a, b, 2000). Another rather common view is that the prevalence of vortex stretching is due to the predominance of stretching of the material lines, but this is due to an erroneous analogy, for more on the differences between these processes see Tsinober (2009) Chap. 9, pp. 307–310.

In other words, the presence of viscosity qualitatively changes the nature of the enstrophy/strain production and the properties of the vorticity/strain field. This in turn means that the nature of dissipation/viscosity is important in the properties of the velocity (including structure functions, inertial range, etc.) since – as already mentioned - the latter is fully determined by the field of vorticity/strain. In other words, the nonlocal impact of the small (including dissipative) scales on the whole flow field is realized through the chain $-s_{ij}s_{jk}s_{kj}$, $\omega_i\omega_j s_{ij}$ to ω^2, s^2 (and ω_i, s_{ij}) and the velocity field as described above, including what can be termed the back reaction of dissipation on the whole flow field, see Chap. 8 below.

A noteworthy aspect is the nonlocality of (purely kinematic) relations between the fields of vorticity and strain and similar relations such as between $-s_{ij}s_{jk}s_{kj}$ and $\omega_i\omega_j s_{ij}$, see Fig. 7.5.

The above and other aspects of nonlocality contradict the idea of cascade in physical space, which is local by definition, e.g. see Frisch (1995), p. 104. For example, the commonly assumed statistical independence between large and small scales (i.e. sweeping decorrelation hypothesis) both of (i) structure functions $S_p \equiv \langle(\Delta u)^p\rangle$; $\Delta u \equiv [\mathbf{u}(\mathbf{x}+\mathbf{r}) - \mathbf{u}(\mathbf{x})]\mathbf{r}/r$) on the (nature) of dissipation, i.e. strain, in the 'inertial range' and (ii) the small scales on the large ones stand in contradiction with the relation $u = \mathfrak{G}\{s_{ij}(x,t)\}$ together with the process of self-production of strain in turbulent flows. Hence both the ill-posed concepts of cascade and inertial range with the former being the reason for the so called 'anomalous scaling', see Chap. 8.

Direct and bidirectional coupling between large and small scales is observed in diverse manifestations. These are described in Sect. 6.6. in Tsinober (2009). One of the issues concerns the statistical dependence of small and large scales. The impact of large scales on the small ones has been known for a while. It was addressed by Kolmogorov (1962) following the famous remark by Landau and Lifshits (1944)[14]

[14]Kolmogorov did these modifications following the Landau objection to universality in the first Russian edition of Fluid Mechanics by Landau and Lifshits (1944) about the role of large-scale fluctuations of energy dissipation rate, i.e., non-universality of both the scaling exponents and the

Fig. 7.5 Top: Left - PDFs of $3/4\omega_i\omega_j s_{ij}$, $-s_{ij}s_{jk}s_{ki}$, and $-17.5(\partial u_1/\partial x_1)^3$ normalized on their means, $Re_\lambda = 10^4$, Right – Joint PDF and scatter plot of $3/4\omega_i\omega_j s_{ij}$ versus $-s_{ij}s_{jk}s_{ki}$, normalized on their means. Note that though the uni variate PDFs of $3/4\omega_i\omega_j s_{ij}$ and $-s_{ij}s_{jk}s_{ki}$ are practically the same, their Joint PDFs show that the strongest activity in strain production corresponds to the weakest to that of enstrophy and vice versa, which is not the case for the strain and enstrophy themselves as seen from the figure on bottom right. Bottom: Left – PDFs of ω^2 and s^2, Right – their Joint PDF. Field experiment at, $Re_\lambda = 10^4$, Gulitski et al. (2007)

and some earlier experimental data, Monin and Yaglom (1971), Obukhov (1962). Kolmogorov formulated modified K41 into refined similarity hypotheses (RSH) assuming among other things the log-normal distribution coarse-grained dissipation,[15] i.e. averaged over a sphere of radius r, with r in the conventionally defined inertial range (CDIR), i.e. $L > r > \eta$ with some 'external scale' L and the 'internal

prefactors: *important part will be played by the manner of variation of* ϵ *over times of the order of the periods of large eddies (of size* ℓ*)*, see Landau and Lifshits (1987), p. 140.

[15]This assumption is due to Obukhov (1962) because as he wrote it is *not very restrictive as an approximate hypothesis since the distribution of any essentially positive characteristic can be represented by a logarithmically Gaussian distribution with correct values of the first two moments*.

This is correct for empirical purposes, but when it goes about the right results for the right reasons it is not sufficient.

Fig. 7.6 Left – conditional averages of enstrophy ω^2 and total strain $s_{ij}s_{ij}$ conditioned on magnitude of velocity fluctuations vector, u. The fit is in the spirit of the Kolmogorov refined similarity hypothesis, though it is a fit in the first place. This fit cannot be expected to be quantitatively universal and should at least have different coefficients a and b for flows with different large-scale properties in the spirit of the Landau remark. Right – conditional averages of squared acceleration magnitude a^2 on magnitude of velocity fluctuations vector, u Gulitskii et al. (2007a, b)

scale' $\eta = (\nu^3/\langle\epsilon\rangle)^{1/4}$. The main result is that the statistics of structure functions in the CDIR depends on the large scale structure of the flow both in scaling exponents and prefactor: $S_p \equiv \langle(\Delta u)^p\rangle = C_p(\mathbf{x},t)(L/r)^{\frac{1}{2}kp(p-3)}(r\langle\epsilon\rangle)^{p/3}$; for $p = 2$ giving $S_2 = C_p(\mathbf{x},t)(L/r)^k(r\langle\epsilon\rangle)^{2/3}$. The factor $C(\mathbf{x},t)$ "depends on the macrostructure of the flow", k is the so called "intermittency" exponent. This model qualitatively captures the correction to the structure function scaling, though it is known that turbulence is not lognormal. For an analog of the RSH in Lagrangian setting see Sawford and Yeung (2015).

It is also worth noting that 1962 is not in realit a correction of 1941, but a kind of different "theory", McComb (2014). Moreover, the "corrections" appear to be not universal due to the reasons noted by Antonia et al. (2017).

Flows with inhomogeneous large scales, large scale or mean strain, shear etc. can exert stronger impact on small scales - the issue goes back to Corrsin (1958), see references, in Saddoughi (1997), Hill (2006) and Tsinober (2009). However there is strong statistical dependence of small scales on the large ones in weakly-inhomogeneous/anisotropic flows, e.g. Fig. 7.6 below.

We point out an interesting result related to the discussed here issue was obtained in Pumir et al. (2014). It concerns the behavior of the average of the pressure-gradient term $\mathbf{u}\cdot\nabla p$ in the energy balance equation conditioned on the kinetic energy of fluid elements in Euler setting. The main result as summarized by tht authors is as follows: *The conditional average* $\langle\mathbf{u}\cdot\nabla p|u^2\rangle$ *reveals striking differences in the role of pressure forces in 2D and 3D. Whereas in 2D pressure forces do not result in an average transfer of energy towards slow or fast particles, they tend to accelerate the fastest particles in 3D,* see Fig. 7.1.

We reiterate that the behavior of the conditional averages $\langle \mathbf{u} \cdot \nabla p | u^2 \rangle$ has – in our opinion – an alternative interpretation as follows. The pressure and its gradient are fully determined as a functional of the field of velocity derivatives (!) rather than just the velocity field $-p = -\rho \nabla^{-2}\{\omega^2 - 2s_{ij}s_{ij})$. Therefore one can expect the behavior of $\langle \mathbf{u} \cdot \nabla p | u^2 \rangle$ as the one shown in Fig. 7.6 *left.* The difference in the 2-D case is due to the absence of the mechanisms of amplification of velocity derivatives $\omega_i \omega_j s_{ij}$, $-s_{ij}s_{jk}s_{ki}$ in this case. In other words, just as the velocity derivatives $\mathbf{u} \cdot \nabla p$ is on the average larger for larger u^2. Far more work is needed in an interpretational context as e.g. to justify the claim that *the pressure term in 3D turbulence takes energy from slow particles and gives it to fast particles.* As mentioned the pressure is fully defined by the field of velocity derivatives and cannot just be considered as a primary "commander" over the flow field *redistributing energy among fluid particles, taking energy away from slow particles and giving it to fast particles,* etc. and *possibly leading to a runaway mechanism of the kinetic energy of particles in high Reynolds number flows*.

The above is devoted to the influence of large scales on the small ones. A far less trivial aspect was until recently neglected as unimportant: the impact of small scales, especially in the dissipative range with $r < \eta$, on the conventionally defined inertial range (CDIR), which appears to be contaminated also by strong dissipative events from the conventionally defined dissipative range (CDDR). This, indeed, is found in recent experiments at large Reynolds numbers up to $Re_\lambda \approx 10^4$ with access to the tensor of velocity derivatives $\partial u_i / \partial x_j$ and in particular local strain (i.e. dissipation) and vorticity, see Borisenkov et al. (2011, 2015), Kholmyansky and Tsinober (2009), Tsinober (2009) and references therein. The main underlying reason is again nonlocality. We address the particular evidence on this issue in Chap. 8 the main reason being that it is of special importance at large Reynolds numbers. The material in the following Chap. 8 is of a broader nature and for this reason is placed just below.

7.4 On Dissipation Anomaly in 3D Turbulence or the Guilty Party for This Amazing Property

As mentioned above the common belief in the community is that the anomalous dissipation of kinetic energy in turbulent flows is due to local cascade, see references in Jiménez et al. (2017), Eyink and Drivas (2018), and that *It seems that the stretching of vortex filaments must be regarded as the principal mechanical cause of the high rate of dissipation which is associated with turbulent motion*, Taylor (1938), see also Taylor (1917).

Neither appeared to be correct as the main guilty party *is the generic mechanism of self amplification of strain* (SAS) $-s_{ij}s_{jk}s_{ki}$, $\langle -s_{ij}s_{jk}s_{ki} \rangle > 0$ in physical space, which is one of the two concomitant key properties and processes of self-amplification of

the field of velocity derivatives $-s_{ij}s_{jk}s_{ki}$ and $\omega_i\omega_j s_{ij}$. Both are observed in a rather straightforward manner, see above.[16]

In other words there is no need for "cascades", decompositions or other mediators in physical space since neither of the processes need, e.g. mean flow. Due to the self-amplification nature of velocity derivatives - the issue is a kind of "technical" one - turbulence "handles" the issue without any unnecessary intermediary, such as mean flow, neither there is need for two-point statistics to deliver energy from "large to small scales". Moreover, the energy does not need to cascade because vorticity and strain self-amplification are not just a creation of the field of velocity derivatives – the velocity field arising in the process of self-amplification of the field of velocity derivatives is the one which is associated with small scales. This process is the one which could be called "cascade" but it is essentially a nonlocal and bidirectional process as there is plenty of nonlocality so that the "returned" energy just appears where it should be, and the dissipation (i.e. strain) does not need large scale flow to be produced due to the self-amplification. Hence no need for the "cascade" and "interscale energy transfer" either. Indeed, there is no indication whatsover on "mediators" in the equations in physical space, but one must not look not solely at the energy balance or similar. These latter ("mediators") so far mainly obscure rather than help to " understanding the physics of cascades", to say nothing about the positively skewed nature of the $-s_{ij}s_{jk}s_{ki}$ and $\omega_i\omega_j s_{ij}$, $\langle -s_{ij}s_{jk}s_{ki}\rangle > 0$ and $\langle \omega_i\omega_j s_{ij}\rangle > 0$. This is at the heart of the physics of turbulence in general and what is called *dissipation anomaly* in particular. These quantities are typical representing genuinely nonlinear processes in turbulence and making it not amenable to quasi-nonlinear approaches such as RDT and the like.

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[16]We point out that SAS does not require a large scale flow and there is no need for a "cascade" to 'deliver' the energy from LS to SS. It is independent of the existence (or not) of such a cascade, but depends on specific factors, such as the nature of turbulence excitation (LS, SS, broadband, etc - which may exclude "cascades") and on BC'c and IC'c. Neither is there no need for two-point statistics.

SAS is a specific (!!) feature of genuine turbulence having no counterpart in e.g. the behavior of passive objects.

Among the main reasons that true quilty party was missed by the community is that because their attention was concentrated elsewhere. The search was focused on the two-point statistical equations such as the Karman–Howarth equation, the "energy balance" equation (scale-by-scale) for the second structure function and the like, see e.g. Chap. 7 in McComb (2014), Vassilicos (2015), Antonia et al. (2017) and references therein. This was motivated by the success of Kolmogorov (1941b) with his 4/5 law. This allowed an explicit relation to be obtained between the third order longitudinal velocity structure function and the *mean* over the scale r dissipation ϵ, $\langle(\Delta u)^3\rangle = -4/5\epsilon r$ for *large* Reynolds numbers for globally homogeneous and isotropic flows or assuming local homogeneity and isotropy. The above shows that one cannot handle the velocity field and/or its increments without the field of velocity derivatives when dealing with issues like those disscussed here.

The features concerning the TL balance like the (7.1a, 7.2a) appear to be true for temporally modulated turbulent flows and for flows with hyperviscosity of different orders, $h = 2, 4, 8$, $h = 1$ corresponds to Newtonian fluid. This means that the self amplification of velocity derivatives will depend on index h.

Among the consequences is that the above makes the statements about the existence of "pathways of energy from production to dissipation" meaningless. This becomes clear due to the kinematic nonlocality. The fields of velocity and strain are related at each time moment by a kinematic relation (7.3) independent of time and forcing, in which velocity at each point in space is defined by an integral over the whole flow domain. At any time moment the field of strain is contributing at each point to the velocity field in a nontrivial way from the whole flow field. Indeed, filtering the energy field as in Cargdese et al. (2017), Jiménez et al. (2017), Eyink and Drivas (2018) and references therein, is accompanied by "filtering" the dissipation in a complex manner over the flow field.[17] The important point is that the dissipation is already "appointed" to the energy at any time moment due to (7.3). There is no need for any "delivery" of the former to the latter - dissipation is "on guard" at any moment and all the time![18]

The amount is on the strain self-amplification $-s_{ij}s_{jk}s_{ki}$, which is a *dynamical* process. It is this process and its relation to the whole flow (along with the $\omega_i\omega_j s_{ij}$) that needs to be studied profoundly. The task is far from being simple, which is illustrated by the equations for $-s_{ij}s_{jk}s_{ki}$ and $\omega_i\omega_j s_{ij}$, see Eqs. C.23 an C.24 at p. 366 in Tsinober (2009).

$$\frac{D(\omega_i\omega_j s_{ij})}{Dt} = W^2 - \omega_i\omega_j \frac{\partial^2 p}{\partial x_i \partial x_j} + VT + MF + EF, \tag{7.5}$$

$$\frac{D(s_{ij}s_{jk}s_{kj})}{Dt} = -3\left\{s^4 - \frac{1}{4}s^4 - \frac{1}{4}(W^2 - s^2\omega^2) + s_{ik}s_{kj}\frac{\partial^2 p}{\partial x_i \partial x_j}\right\} + VT + MF + EF. \tag{7.6}$$

Here $W^2 = \omega_j s_{ij}\omega_k s_{ik}$, $s^4 = s_{ik}s_{kj}s_{il}s_{lj}$ and VT, MF and EF stand for viscous terms, terms from mean flow and external force. The potential importance of the pressure Hessian $\Pi = \frac{\partial^2 p}{\partial x_i \partial x_j}$ is seen already from Eqs. (7.1, 7.2) above.

So far *the botom line is that the guilty party for the dissipation anomaly in 3-D turbulence is the self-amplification of the field of strain rather than fictitious "cascades" and the like*.

[17] It would be interesting to see these "dissipation images" of the *specific whirls, individual intense structures identified in real space* as claimed by Cardese et al. (2017) and Jiménez et al. (2017).

[18] Thus the Eq. 7.3, 7.4 alone prevent the pretty popular kind of interpretation of Kolmogorov as e.g.given by Sawford and Yeung (2015):

Kolmogorov similarity theory is the starting point for our discussion of Lagrangian intermittency. According to this theory, the small scales of turbulence have a universal structure depending only on the viscosity and the mean rate at which energy is transferred down the spectrum from the largest scales where turbulence is created to the smallest scales where viscosity dissipates this energy. Yeung et al. (2015).

7.5 Non-Gaussian Nature and Irreverisibility of Turbulence

An interesting feature is that usually both come together, Betchov (1974, 1976).

7.5.1 Non-Gaussianity

The non-Gaussian nature of turbulence is another 'N' contributing to the difficulty of turbulence – in addition to those discussed above.

> *Most problems in classical stochastic processes are reduced to solubility by statistical independence, or the assumption of a normal distribution (which is equivalent) or some other stochastic model; because of the governing differential equations, the turbulent velocity at two space-time points is, in principle, never independent—in fact, the entire dynamical behavior is involved in the departure from statistical independence. The equations, in fact, preclude the assumption of any ad hoc model, although this is often done in the absence of a better idea.* Lumley (1970).

> *...it would be a miracle if the usual procedure of imposing stationarity, truncating the resulting system of equations, and looking for a Gaussian solution, would lead to results much related to physics.* Ruelle (1976).

> Moreover, *with a large number of degrees of freedom, a large class of plausible phase-space distributions have low-order moments that are indistinguishable from the low-order moments of the Gaussian distribution. This result is just a special case of the central limit theorem.* Orszag (1977). See also Lumley (1970, pp. ix, 50, 91 and 1972), Leonov and Shiryaev (1960).

In other words the nature of turbulence as governed by NSE is essentially non-Gaussian.[19] though turbulence research has a solid history of attempts to exploit the Gaussian fields in one way or another. The "better ideas" of employing some kind of Gaussianity started at least in the 1950, see Hopf (1952), Wiener (1958). Too many Gaussianity and similar "physically justified approximations" remain in use even today Meneveau (2011), Wilczek and Meneveau (2014), Wilczek (2016), Pumir et al. (2016), Wilczek et al. (2017) and references therein. All this in spite of the fact that turbulence is an essentially non-Gaussian phenomenon at the outset. Another phenomenon/approach is looking at the "deviations" from Gaussianity as if turbulence is "slightly" non-Gaussian without any justification of such an expectation which is more than just nonphysical. Why on earth turbulence must be Gaussian or even quasi-Gaussian - it is like using a kind of "small parameter" looking at small "deviations". There are attempts to study transition to non-Gaussianity using Gaussian initial conditions (the most unrealistic ones, see below) and to 'explain' the origins of non-Gaussianity of fluid turbulence, e.g. using "simple" models, though it follows from the NSE, e.g. Novikov (1967), Lumley (1970), Betchov (1974, 1976) among others: today it is clear that Gaussianity and (apparent!) randomness/stochasticity are not synonymous and one must explain why one would expect Gaussian behaviour in

[19] Simply otherwise the flow does not obey the NSE!

genuine turbulent flows and not the other way around, for more and references see Sect. 6.8. Non-Gaussian nature of turbulence in Tsinober (2009).

Among the most important aspects is the dynamical one: a purely Gaussian velocity field is dynamically impotent. Indeed, in such a field, all the odd moments vanish. This invalidates the Kolmogorov 4/5 law, turns the Karman–Howarth equation into a linear one, and prevents the self-production of velocity derivatives, i.e. production of enstrophy and strain (i.e. dissipation): in a Gaussian velocity field $\langle \omega_i \omega_j s_{ij} \rangle \equiv 0$, $\langle s_{ij} s_{jk} s_{ki} \rangle \equiv 0$, *which are known empirically to be positively skewed, see Fig. 7.5 above. Gaussian fields lack vorticity-strain alignments and essential structure(s), to mention a few. Another important aspect is that Gaussian field lacks intermittency and structure by definition.*[20]

One of the basic properties underlying structure(s) and intermittency is the essentially non-Gaussian nature of turbulence for which there are numerous experimental observations, see e.g. Sect. 6.8 in Tsinober (2009) on non-Gaussian nature of turbulence. For exmple, the outstanding non-zero odd moments mentioned above: the strain and enstrophy production $-\langle s_{ij} s_{jk} s_{ki} \rangle$, $\langle \omega_i \omega_j s_{ij} \rangle$. It has to be stressed that (i) non-Gaussianity and intermittency/structure are not synonymous as not any non-Gaussian field is turbulent and/or asymmetric in PDFs of $-s_{ij} s_{jk} s_{ki}$, $\omega_i \omega_j s_{ij}$ and (ii) non-Gaussianity is only a statistical manifestation of intermittency/structure and also a special feature of irreversibility of turbulence (see below). An important aspect is that the non-Gaussianity of turbulence becomes stronger with decreasing scales, e.g. in the field of velocity derivatives. Therefore closures based on Gaussian random fields are expected to be physically inadequate even if exhibiting some agreement with DNS.

Even if the flow field is initially Gaussian, the dynamics of turbulence makes it non-Gaussian *with finite rate*. This is seen from the following simple illustration looking at the at the equation for $\langle \omega_i \omega_j s_{ij} \rangle$ (dropping the viscous term and terms with mean flow, and external forcing).

$$D\langle \omega_i \omega_j s_{ij} \rangle / Dt = \langle \omega_j s_{ij} \omega_k s_{ik} \rangle - \langle \omega_i \omega_j \Pi_{ij} \rangle, \tag{7.5a}$$

[20]However, any nonlinear function, functional and alike of a variable, which is Gaussian, is non-Gaussian. For instance the enstrophy, dissipation, pressure, etc. of a Gaussian velocity field possess *exponential* tails and their flatness is quite different from 3. For example, for a Gaussian velocity field $F_G(\omega^2) = \langle \omega^4 \rangle / \langle \omega^2 \rangle^2 = 5/3$ and $F_G(s^2) = \langle s^4 \rangle / \langle s^2 \rangle^2 = 7/10$. But this by no means indicates that, for a Gaussian velocity field, these quantities are intermittent and possess structure as sometimes claimed. These and similar "non-Gaussian features" resulting by using Gaussian objects do not cure the problem from the original impotences of the Gaussian fields even if producing results in agreement with, say, DNS. In basic research the right results should be for the right reasons. Among the reasons is that multiplicative models are able to produce intermittency for a purely non-intermittent field as is the Gaussian velocity field. See YaB et al. (1990) on interesting observations on this and related matters. We also to point out a similar concern regrading "Lagrangian" intermittency, e.g. "intermittency" of a purely Eulerian Gaussian field in its Lagrangian representation, see Sect. 3.4 above and Sect. 9.1.3 below.

The above is not a serious concern for engineering applications as *even wrong theories may help in designing machines* Feynmann (1996).

For recent examples see Wilczek and Meneveau (2014) Wilczek (2016) and references therein.

For a Gaussian velocity field $\langle \omega_i \omega_j s_{ij} \rangle_G = 0$, $\langle \omega_i \omega_j \Pi_{ij} \rangle_G = 0$ and $\langle \omega_j s_{ij} \omega_k s_{ik} \rangle_G = \frac{1}{6} \langle \omega^2 \rangle^2 > 0$. Since the quantity $\omega_j s_{ij} \omega_k s_{ik} \equiv W^2$, $W_i = \omega_j s_{ij}$, it is positive pointwise for any vector field. Hence for an initially Gaussian field

$$\left\{ D \langle \omega_i \omega_j s_{ij} \rangle / Dt \right\}_{t=o} = \left\{ \langle \omega_j s_{ij} \omega_k s_{ik} \rangle \right\}_{t=0} > 0. \tag{7.5b}$$

It follows that, at least for a short time interval t, the mean enstrophy production (and similarlly strain) will become positive. This shows that Gaussian IC's are the "worst", and also unrealistic, since no flow state existing in reality is Gaussian.

There are numerous "proofs" of the "emergence of non-Gaussianity in turbulence", e.g. Wilczek et al. (2017) among many along with numerous experimental observations on the non-Gaussianity of turbulence, see e.g. Sect. 6.8 in Tsinober et al. (2001); Tsinober (2009) on non-Gaussian nature of turbulence, e.g. the outstanding non-zero odd moments: the strain and enstrophy production $-\langle s_{ij} s_{jk} s_{ki} \rangle$, $\langle \omega_i \omega_j s_{ij} \rangle$.

Turbulence – being essentially non-Gaussian – is such a rich phenomenon that it can 'afford' a number of Gaussian-like manifestations, some of which are not obvious and even nontrivial, for examples see Sect. 6.8.2 in Tsinober (2009). Thus the importance of parameters related to the non-Gaussian nature of turbulence and not only in the abovementioned contexts mentioned above.

To reiterate: the quantities $-s_{ij} s_{jk} s_{ki}$, $\omega_i \omega_j s_{ij}$ are indeed outstanding as reflecting the processes responsible for the dissipative and rotational nature of turbulence along with its non-Gaussianity and time irreversibility.

7.5.2 Irreversibility

It was Betchov (1974, 1976) who realized the intimate relation between the non-Gausianity and irreversibility of turbulence:

> *Since the positive spikes of the velocity derivative represent the principal statistical difference between recording of u(t) and a recording of u(-t) they represent the irreversible part of the signal. Note that it is precisely this skewness of du/dt which measures the rate of spectral energy transfer as well as the rate of concentration of the vorticity by stretching. Perhaps it is significant that a major portion of this process occurs in small fluid masses, separated by distances comparable to the integral scale of turbulence.* Betchov (1974, p. 1512).

In other words the odd moments are the quantities expected to be particularly useful in studying the manifestations of time irreversibility of turbulent flows. Hence the reason to discuss irreversibility and non-Gaussianity one along with the other.

It is noteworthy that as any dissipative system, see e.g. Nicolis (1986), on statistical aspects see Novikov (1974) and Kondepudi et al. (2017), turbulence is time irreversible, i.e. the equations describing turbulence are not invariant under time reversal. Thus the manifestations of the time irreversibility are expected to be observed via studying properties associated in one way or another with the NSE, both in their Eulerian and Lagrangian setting and corresponding experiments.

As the main guilty party for irreversiblity is dissipation one of the features of utmost importance here is that of the processes enhancing the field of velocity derivatives, i.e. the rotational and dissipative properties especially the latter. The first property is associated with the production of vorticity $\omega_i\omega_j s_{ij}$, $\langle\omega_i\omega_j s_{ij}\rangle > 0$, (i.e. its rotational nature), whereas the second property is due to the production of strain $-s_{ij}s_{jk}s_{ki}$, $-s_{ij}s_{jk}s_{ki}\left\langle -s_{ij}s_{jk}s_{ki}\right\rangle > 0$, i.e. its dissipative nature. Both processes are known empirically to be irreversible (changing sign under transformation $t \to -t$), i.e. with prevalent positive production, see e.g. Fig. 7.6 above. These quantities ***are special in that they possess the property of self-amplification***, i.e. not requiring any mediators to maintain the high level of dissipation (and enstrophy) in the flow, such as the most popular *cascade*and related like the claim that *in three-dimensional turbulent flows, the flux of energy from large to small scales breaks time symmetry energy flux over the range of scales, intermediate between energy injection and dissipation as a source of time irreversibility* as massively claimed, e.g. Xu et al. (2016) and references therein .

The bottom line is that the turbulence irreversibility is due to its strongly dissipative nature. The term "strongly" is due to the process of self-amplification of strain $-s_{ij}s_{jk}s_{ki}\left\langle -s_{ij}s_{jk}s_{ki}\right\rangle > 0$ just as it is responsible for the dissipative anomaly in 3-D turbulent flows.

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Chapter 8
Large Reynolds Number Behavior, Symmetries, Universality

Abstract The large Reynolds number behavior is of special importance from several points of fundamental nature which include such issues as restoring (or not) in some sense the symmetries of Navier–Stokes equations (e.g. locally in Kolmogorov, Dokl Akad Nauk SSSR 30:299–303, (1941a)), universality, the role of viscosity/dissipation and the concept of inertial range, the role of the nature of forcing/excitation, inflow, initial and boundary conditions. In view of the arguments and experimental results on nonlocality and the direct and bidirectional coupling between large and small scales in the previous section a natural question arises what is the impact of nonlocality on all the above and whether there are enough reasons and evidence for a discussion and reexamination of the above issues, generally, and in relation to the nonlocal properties of turbulent flows among others, especially. Navier–Stokes equations at sufficiently large Reynolds number have the property of intrinsic mechanisms of becoming complex without any external aid such as strain and vorticity self-amplification. There is no guarantee that the outcome is the same from, e.g., natural "self-randomization" and with random forcing, on one hand, and different kinds of forcing, boundary and initial conditions, on the other hand. Moreover, there is serious evidence that the outcome may be and indeed is different.
We have chosen to start with the issue of the role of viscosity and/or dissipation (including their nature) and the concept of inertial range as most convincing to demonstrate that the concern is justified. Indeed, there is every reason to expect that the small scales from the dissipative range, at least those which are strong, have an important impact on the larger scales including the conventionally defined inertial range (CDIR). Among of the main points below is that due to nonlocality at least some key nonlinear terms are not purely inertial in the CDIR, in contrast to common beliefs, the 4/5 Kolmogorov law at large Re being an outstanding example of not purely inertial relation and consequently a victim of interpretational abuse.

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8.1 Inertial Range, the Roles of Viscosity/Dissipation and Related Issues

How a theoretical attack on the inertial-range problem should proceed is far from clear. No formal analysis by means of perturbation theory, the moment equation hierarchy, or renormalization techniques can settle whether $E(k) = C\epsilon^{2/3}k^{-5/3}(kL)^{-1/9\mu}$ *is a valid equation and, if so, whether* $\mu = 0$ *or not.* Kraichnan (1974).

The main physical problem is to understand to what extent the statistics in the inertial interval is universal, i.e., independent of the conditions of excitation and dissipation. The scale of injection, where turbulence is excited, usually differs dramatically from the scale of damping, where dissipation takes place. Nonlinear interactions strongly couple the degrees of freedom by transferring excitations from the injection to the damping scale throughout the so-called inertial range of scales. Falkovich et al. (2001).

There two most problematic aspects in massive quotations like the above are: the object called "inertial range"; and that "inertial range" itself it is not well defined. According to some recently obtained evidence there is no such an object in existence. Among the problems is that in real fluids the role of strain field is not limited just by dissipation. As a the nature of dissipation makes an essential qualitative difference including nonlocal effects so that different dissipative mechanisms result in different outcomes implying the nonexistence of the "inertial range" contrary to common beliefs. This and related issues are addressed below.

We start with pointing out the common view that turbulence is an essentially inertial phenomenon such that how energy is dissipated in the small scales does not influence the large scales as long as the amount is correct. In other words, the small scales are just a passive sink of energy and the nature of dissipation does not matter for large scales and in particular in the conventionally defined inertial range (CDIR), see references and citations on this kind of statements at pp. 103 and 335 in Tsinober (2009). Some more are given below in the Chap. 11 Appendix essential quotations.

One of the popular arguments for the existence of the CDIR at large Reynolds numbers is the Kolomgorov 4/5 law which is the consequence of the NSE and under some assumptions (including isotropy) takes the following form, for a precise form, see Eq. (34.20), p. 139 in Landau and Lifshits (1987), McComb (2014), Antonia et al. (2017)

$$S_3 = -(4/5)\,\langle\epsilon\rangle\, r + 6\nu dS_2/dr, \tag{8.1}$$

and in which the last term is negligible at large Reynolds numbers.

$$S_3 = -(4/5)\,\langle\epsilon\rangle\, r, \tag{8.2}$$

Fig. 8.1 Normalized third-order longitudinal velocity structure function: **a** - Field experiments: 102 Kfar Glikson measurement station, Israel - $Re_\lambda = 0.7 \cdot 10^5$; Falcon, Airborne experiment Germany - $Re_\lambda = 1.6 \cdot 10^3$; SNM 12 and SNM 11 Sils-Maria experiment, Switzerland - $Re_\lambda = 5.9 \cdot 10^3$ and $Re_\lambda = 3.4 \cdot 10^3$; **b** - DNS of NSE at 8192^3, $Re_\lambda = 1300$. The dashed horizontal line corresponds to the plateau at 4/5. Vertical lines demarcate inertial range $50 < r/\eta < 400$, considered as the range within 5% deviation from 4/5. The claim is that *the longitudinal and transverse velocity increments scale the same*, Reproduced from Fig. 2 Iyer et al. (2017)

i.e. the third-order moment $S_3 \equiv \langle(\Delta u)^3\rangle$ is universal, i.e. it does not depend on the details of the turbulence production, but is determined solely by the mean energy dissipation rate alone.[1]

The problem of approach to (8.2) is mostly formulated generally in terms of the magnitude of the Reynolds number, e.g. how finite it can be. However, this rises a variety of issues, e.g. the impact of the finite Reynolds number effect on small scale turbulence as exposed by Antonia et al. (2017) and references therein.

Nevertheless, there exists an experimental confirmation that at Reynolds numbers up to $Re_\lambda \approx 10^4$ so that (8.2) does hold in a broad range of scales of about three decades at $\mathrm{Re}_\lambda \approx 10^4$, Kholmyansky and Tsinober (2008), Gulitskii et al. (2007a, b, c) see Fig. 8.1a. Also noteworthy recent confirmation of (8.2) is found in Iyer et al. (2017) a DNS of NSE on an 8192^3 periodic box with $\mathrm{Re}_\lambda = 1300$, Fig. 8.1b.

Thus one gets the impression from the term *"inertial range" (IR)* that the 4/5 law is a purely inertial relation at large Re and that the range of its validity defines the extension of the conventionally defined inertial range (CDIR) at high Reynolds numbers. However, this would be true if the term $6\nu d S_2/dr$ in (8.1) represented all the contributions from viscosity, i.e. if the $S_3 \equiv \langle(\Delta u)^3\rangle$ in (8.2), or more precisely just the velocity increments $\Delta u \equiv [\mathbf{u}(\mathbf{x}+\mathbf{r}) - \mathbf{u}(\mathbf{x})]\mathbf{r}/r$, would ***not*** contain non-negligible contributions from dissipative events. In other words, *in reality what is termed IR is* ***assumed*** *to be purely inertial and having no contribution from viscosity for* $\eta \ll r \ll L$. Thus the question is about the impact of viscosity (dissipative events) on the

[1]This latter is due to Kolmogorov (1941a). We emphasize that the correction he proposed in 1962 concerns the impact of the large scales on the scaling of structure functions in CDIR.

A typical statement is found in Frisch (1995, p. 81): *A consequence of Kolmogorov's formula* (6.5) (i.e the 4/5 law - see the (8.2) above) *is the existence of a range of* ℓ*'s over which* $\langle\epsilon\rangle(\ell)$ *is independent of* ℓ.

Fig. 8.2 Histograms of the increments of the longitudinal velocity component for the full data and the same data in which the strong dissipative events were removed: left - $r/\eta = 400$ and threshold $q = 3$, right - $r/\eta = 400$ with different thresholds; field experiment, $Re_\lambda \approx 10^4$; - the lower edge of the inertial range is about $r/\eta = 40$ An event $\Delta u \equiv [u(x+r) - u(x)]r/r$ is qualified as a strong dissipative (SDE) if at least at one of its ends $(x, x+r)$ the instantaneous dissipation $\epsilon > q\,\langle\epsilon\rangle$ with $q > 1$. Note that the PDFs with removed strong dissipative events (dark blue ones) are not close to the Gaussian curve. An important feature is that dissipative events (DE - "points" of order Kolmogorov scale η) at the ends $(x, x+r)$ of SDE's reside individually in the dissipative range, literally live within the CDIR in turbulence at high Reynolds numbers, i.e. nonlocality is a broader issue and does not necessarily involve scale separation such as in long range interactions - intermittency is an important feature of the common (!) nonlocal life of LS and SS. right - the same for various values of q and $[\Delta u_1(400)/RMSu_1]^3$. Kholmyansky and Tsinober (2009)

CDIR at high Reynolds numbers. The question is not as meaningless as it may seem. Indeed, computing Δu one encounters also large and even very large instantaneous dissipation at the ends $(\mathbf{x}, \mathbf{x}+\mathbf{r})$ - we denote them as DE's, in experiments they are "points" of order Kolmogorov scale η. In other words, the second Kolmogorov (1941a) hypothesis involves a strong assumption that the dissipative events such that at least at one of their ends $(\mathbf{x}, \mathbf{x}+\mathbf{r})$ the instantaneous dissipation $\epsilon > q\,\langle\epsilon\rangle$ with $q > 1$ do not matter for the statistics of velocity increments in CDIR, as long as r is within the *"inertial range"*. Otherwise the *"inertial range"* would be out of existence as a concept, which appears to be the case. Note that the above question stems from the two-point nature of the statistics used, so that it is more than legitimate to ask similar questions on quantities other than just velocity increments such as two-point correlations among others.

To (dis)prove this one needs access to instantaneous dissipation at large Reynolds numbers. Indeed, looking at Fig. 8.2 it can be seen that there exist a substantial number of dissipative (!) events (DE) living in the conventionally defined dissipative range (CDDR) contributing essentially to the PDF of velocity increments in the conventionally defined inertial range (CDIR) at high Reynolds numbers, $Re_\lambda \approx 10^4$.

The key feature is that this contribution is largest to the tails of the PDF of velocity increments. Thus the CDIR is an ill-defined concept. In particular, the neglected viscous term in the Kolmogorov 4/5 law (8.1) does not contain all the viscous contributions. Those present (but hidden) in the structure function S_3 itself remain and keep

Fig. 8.3 **a**- Contributions of the strong dissipative events in the CDIR to the third-order structure function as a function of the threshold q for various separations r; blue circles – $r/\eta = 4$, green squares – $r/\eta = 40$, brown triangles – $r/\eta = 400$, red crosses – $r/\eta = 4000$; in the insert: scaling exponents of the third-order structure function as a function of the threshold $Re_\lambda \approx 10^4$; **b** - PDFs of the third-order structure function at $r/\eta = 400$, Kholmyansky and Tsinober (2009)

the 4/5 law precisely: without the dissipative events just mentioned the 4/5 law does not hold! In this sense the 4/5 law is not a pure inertial law even at $Re_\lambda \approx 10^4$. Indeed, strong dissipative events do contribute to the 4/5 law, see Fig. 8.3, and removing them leads to an increase of the scaling exponent above unity, see insert in Fig. 8.3a. It is noteworthy that the contribution of the dissipative events in the 4/5 law in the form (8.2) is not small in spite of considerable cancellation between the negative and positive events.

Thus - contrary to common view - the 4/5 law is not a pure inertial relation at large Re. The important implication of more general nature is that nonlinear interactions (NI) are not synonymous to purely inertial ones. They (NI) consist of purely inertial ones with an essential contribution from the viscous and cross-interactions. In the case of the third order functions and energy fluxes these interactions are constrained by the 4/5 law. *The consequence is that the purely inertial and dissipative events are adjusting to keep some quantities such as the total dissipation or the energy flux approximately constant at large Reynolds numbers*. The constraint responsible for this adjustment is just the 4/5 law or more generally the NSE.[2]

A special digression is about the 4/5 law in case of hyperviscosity at high Re_λ. As shown in Fig. 8.4 it is valid in the form (8.2) within almost two decades at Reynold number $Re_\lambda \approx 1744$, see Barjona and da Silva (2017).

[2]Noteworthy is the pure kinematic relation involving the third order structure function, Hosokawa (2007), Kholmyansky and Tsinober (2008), Germano (2012) $\left\langle (\Delta u)^3 \right\rangle = 3 \left\langle u_{sum}^2 \Delta u \right\rangle$, with $2u_{sum} = u(x+r) + u(x)$, which together with the 4/5 law results in a relation equivalent to the 4/5 law $\left\langle u_{sum}^2 \Delta u \right\rangle = \langle \epsilon \rangle r/30$. Thus the 4/5 law provides a clear indication of absence of statistical independence between the quantities residing at large and small scales.

Of interest is the issue concerning the (an)isotropy of the CDIR and in particular the impact of the SDE's. The information is scarse. On a qualitative level the tendency to anisotropyas manifested by the relations of the second order velocity structure functions is somewhat weaker as compared to the 4/5 law, Gulitskii et al. (2007a, b, c), Iyer et al. (2017).

Fig. 8.4 Third-order longitudinal velocity structure function normalized by the viscous dissipation rate, for Newtonian and hyperviscous simulations. The horizontal line is at 4/5. Reproduced from Fig. 1b Barjona and da Silva (2017)

Here again there is a "hidden" dependence on viscosity in the 4/5 law due to the contribution from the viscous events to the third order function as above in the Newtonian case, i.e. here the 4/5 law is also not a pure inertial relation at large Re, and consequently the concept of inertial range is ill-posed. Moreover, one does not have to be misled by the above result, since there are clear indications about not "hidden" and not unexpected important differences between the Newtonian and hyperviscous cases in flow structure(s) etc., see e.g. Spyksma et al. (2012), Frisch et al. (2008) and references therein. An important aspect is that the independence of some (!) parameter of viscosity at large Reynolds numbers does not mean that viscosity is unimportant. It means only that rather than being unimportant the effect of viscosity is Reynolds number independent. Thus, speaking about asymptotic (large Re) behavior a realistic option is that some (**not all**) quantities $\Longrightarrow const$ as they do in observations, but this does not mean that they have to become independent on viscosity, i.e. generally the limit $\nu \Longrightarrow 0$ is not viscosity independent. We return to this issue below, for more on this issue see also Sect. 10.2 in Tsinober (2009).

As concerns the structure functions of higher order $p > 3$ there is no simple constraint as the 4/5 law for S_3, see, e.g. references in McComb (2014) and Antonia et al. (2017). The consequence is that the above mentioned dissipative events in the "inertial range" are responsible for what is called "anomalous scaling" for S_p, $p > 3$. This is clearly seen from Fig. 8.5. Thus the anomalous scaling is not an attribute of the conventionally-defined inertial range (CDIR), and thereby the latter is not a well-defined concept, just like "cascade" and the conventionally defined dissipative range, CDDR.

A noteworthy conjecture concerns the Lagrangian setting in which the sweeping effects are mostly (but not totally! See Chap. 4) removed so that the contribution of the strong dissipative events (SDE) in the CDIR is even larger than in the Eulerian setting. Thus one is tempted to bet that this is the reason for the absence of the

Fig. 8.5 Scaling exponents of structure functions at $Re_\lambda \approx 10^4$ for the longitudinal velocity component corresponding to the full data and the same data in which the strong dissipative events with various thresholds q were removed. With $q = 3$ the higher order structure functions ($p > 3$) exhibit Kolmogorov scaling $p/3$. Kholmyansky and Tsinober (2009)

expected scaling *a la* Kolmogorov 41, it is due to Landau (1944), see Obukhov (1962), even at the level of the second order Lagrangian structure function. In other words removal of the SDE from appropriate Lagrangian data set as above in the Eulerian setting would bring the expected scaling, which is not observed so far. This is (to our view).due to reduced sweeping and the consequent increase of the role of dissipative events in the "inertial range", see e.g. Falkovich et al. (2012) and Sawford and Yeung (2015).

It is natural to expect a similar phenomenon of the non-negligible contribution of dissipative events to quantities in the CDIR on a more general level than just structure functions and even just extreme/strong events. This is because of the general impact of nonlocality.

For instance, not all quantities entering equations such as RANS or LES for resolved scales in the "inertial range" are indeed purely inertial. For example, it appears that strong dissipative events make a nonnegligible contribution to the SGS stresses and SGS energy flux $\Pi(x;r) = -\tau_{ik}[s_{ik}]$ where $\tau_{ik} = [u_i u_k] - [u_i][u_k]$ are the SGS stresses and the filtered quantities are denoted as [...], see Fig. 8.6. Indeed, this is observed at a qualitative level by the necessity of using time series and a one dimensional standard Gaussian filter of width r. There is an essential contribution of the dissipative events to the SGS energy flux in the conventionally-defined inertial range with a considerable dependence on the threshold q,which is increased with the decrease of q. The main point is that the Reynolds stresses, subgrid stresses, etc. are not inertial range quantities, because the contribution of the viscous effects is not limited by corresponding the viscous terms, e.g., to the Laplacian in the filtered equations as in the case of 4/5 law.

Another example of the contribution of DE within the CDIR is observed in the behavior of other key quantities such as the enstrophy and strain production $\omega_i \omega_j s_{ij}$, $s_{ij} s_{jk} s_{ki}$. Though the origin of both quantities and corresponding processes is purely

Fig. 8.6 An example showing the contribution of strong dissipative events to the subgrid scale (SGS) energy flux $\Pi(x;r) = -\tau_{ik}[s_{ik}]$ at $Re_\lambda = 10^4$ for the longitudinal velocity component corresponding to the full data and the same data in which the strong dissipative events (when at least at one point x or $x+r$ the instantaneous dissipation $\epsilon > q\,\langle\epsilon\rangle$) with various thresholds q were removed

Fig. 8.7 **a** Contributions of strong dissipative events (when at point x the dissipation $\epsilon > q\,\langle\epsilon\rangle$) to the enstrophy production as a function of the threshold q. **b** Histograms of the enstrophy production for the same data with removed strong dissipative events for the threshold $q = 3$. Similar results are observed for strain production, see Fig. 4 in Borisenkov et al. (2011), also (2015)

inertial, it appears that both quantities contain a substantial contribution from the dissipative events. This is clearly seen from Fig. 8.7 showing the contribution of DE and examples of the PDFs of $\omega_i\omega_j s_{ij}$ and $s_{ij}s_{jk}s_{ki}$ for the whole data and the sets with removed dissipative events.

This is a direct demonstration of the nonlocal impact of the dissipative scales on the whole flow field through the chain $-s_{ij}s_{jk}s_{kj}$, $\omega_i\omega_j s_{ij}$ to ω^2, s^2 and the velocity field as described above. This includes what is termed the back reaction of dissipation on the whole flow field.

This impact of viscosity is also seen from the equations for strain and vorticity with the production terms $-s_{ij}s_{jk}s_{kj}$, $\omega_i\omega_j s_{ij}$ and is consistent with the Tennekes and Lumley balance, see Chap. 7 above. As mentioned an important aspect is that this

balance appears to be valid in different meanings (not only in the mean) as follows. The first feature that the TL balance holds at Re_λ as low as ≈60. Second, it holds pointwise in time, i.e. the integrals over the flow domain of the enstrophy production and of its viscous destruction are approximately balanced at any time moment, $\int \omega_i \omega_j s_{ij} dV \approx -\nu \int \omega_i \nabla \omega_i dV$. The features concerning the TL balance appear to be true for temporally modulated turbulent flows and for flows with hyperviscosity of different orders, $h = 2, 4, 8$ with $h = 1$ corresponding to Newtonian fluid.

Before proceeding further a short summary of the above is as follows:

A. The important aspects of the evidence presented above is that the Kolmogorov and sub-Kolmogorov scales (dissipative events - DE), defined as conventionally dissipative range (CDDR), are directly and bidirectionally coupled with the conventionally-defined inertial range (CDIR). These DE literally live within the CDIR in turbulence at high Reynolds numbers, so that the corresponding DE's make the largest contribution to the tails of the PDF of velocity increments in the CDIR, see Fig. 8.2. Hence several non-trivial consequences including those of paradigmatic nature: *(i)* both the CDIR and CDDR (and thereby "cascade") are ill defined concepts, e.g. it is the presence of strong dissipative events which is responsible for what is called "anomalous scaling" in the CDIR, see Fig. 8.5, i.e. its 'anomalous scaling' is not simply an attribute of the inertial range as such is not in existence[3] *(ii)* the 4/5 law is a not pure inertial relation at large Re. Similarly filtered equations for the inertial range quantities are not purely inertial range relations either. In both there is an essential contribution of dissipative events from the conventionally dissipative range. The same is true of such processes as enstrophy and strain production and other key nonlinear processes, these are not purely inertial processes having an essential contribution of dissipative events from the conventionally defined dissipative range.

B. In other words the recent experimental evidence with direct access to the field of velocity derivatives at large Reynolds numbers does not support the hypothesis on the existence of an inertial range with some scaling symmetry at large Reynolds numbers. Therefore viscosity/dissipation are far more important than just a passive sink of energy because of kinematic nonlocal relations, Eqs. (7.3), (7.4), between velocity and strain along with the self-amplification of the latter. A similar state of affairs exists with the issue of universality and the like. Many beliefs in the various aspects of universality (quite understandably especially needed in theoretical treatments) appear to be not real - the evidence points to the contrary: the flow, generally, depends on the nature of forcing, inflow, initial and boundary conditions and a variety of issues in flow control(lability) both in engineering and mathematical contexts. After all NSE, BCs and ICs define the flow field in its entirety.

The above calls for a discussion and reexamination of the issues mentioned, such as asymptotics at large Reynolds numbers, symmetries and universality especially in relation to the nonlocal properties of turbulent flows among others.

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[3]This is precisely the question by Kraichnan (1974): *How a theoretical attack on the inertial-range* (IR) problem should proceed is far from clear, This question seems to be irrelevant as there is no such an object (IR) in existence.

8.2 Reynolds Number Dependence and Behavior of Turbulent Flows at Large Reynolds Numbers

Whatever the tools, it is crucial that the theory be able to describe what happens at finite Reynolds numbers, both to understand the limit of infinite Reynolds number and to interpret correctly the data from existing experiments and simulations Kraichnan (1991). For a recent review see Antonia et al. (2017) and references therein.

Many of the interesting mathematical problems about Navier–Stokes and Euler equations originate in the attempt to understand questions related to turbulence. One of the basic tenets of turbulence is the non-vanishing of energy dissipation in the limit of high Reynolds numbers. Constantin (2016).

Observations show that there are two kinds of properties of turbulent flows. Some properties of turbulent flows become Reynolds number independent as the Reynolds number becomes large enough. Others remain Reynolds number dependent and the existing modest evidence[4] indicates that they may never saturate as $Re \to \infty$.

The outstanding quantity of the first kind is dissipation with its anomaly of (presumably) being finite as $Re \to \infty$. We remind that the most likely physical mechanism underlying the dissipation anomaly is the self-amplification of strain, see above. There is no dissipation anomaly in two-dimensional flows due to the absence of the mechanism. The first kind of properties is represented by the drag of bluff bodies. A circular disc is one of the cleanest example as it's drag coefficient is independent of Reynolds number beyond $Re \sim 10^3$. Another example is exhibited by the independence of dissipation of Reynolds number in a variety of flows from basic configurations to diverse applications in engineering, geophysical flows and others all of which are neither homogeneous nor isotropic. Things like 2/3, 4/5 and 4/15 laws, $k^{-5/3}$ spectrum and some others seem to belong to the same category.

However, it should be stressed that the meaning of Reynolds number independence (which is not the same as being insensitive to the nature of dissipation at large Re) requires caution, since, in fact, there is a "hidden" dependence on viscous effects as with the 4/5 law even at $Re_\lambda \sim 4 \cdot 10^4$ as demonstrated above**.** In other words, the independence of some parameter of viscosity at large Reynolds numbers does not mean that viscosity is unimportant. It means only that the (cumulative) effect of viscosity is Reynolds number independent. Moreover, though the dissipation is

[4]There is a general difficulty in the experimental context associated with the direct large/small-scale coupling, see Chap. 7. As the Reynolds number increases the characteristic scale between (!) the regions of strong small-scale activity ('intermittency') is increasing, whereas the characteristic scale of these regions themselves is decreasing. On the other hand when looking at higher- order structure functions $S^p \sim r^{\zeta p}$ (*assuming* some scaling) one is forced to use very long time records to achieve statistical convergence. This means that very large scales (which are anisotropic) are involved in the process. In other words, as the Reynolds number and the order of the structure function increase so (most likely) does the 'deviation' from $\zeta_p \tilde{} p/3$. This means that experimentally (!) it may be not realistic to get a reasonable and reliable estimate of the exponents ζ_p if such exist. This becomes less realistic as the order of the structure function and the Reynolds number are increasing. All the above provided that the experimental errors are small. Here again the larger the order of the structure function the larger is the influence of the errors.

known empirically to tend presumably to saturate to a nonzero limit as $\nu \to 0$ (or at least essentially non-vanishing) this however, does not mean that there exist a limit as $\nu \to 0$ in the sense (or any other sense) that all other flow characteristics do also saturate.

As a simple illustration let us look at the consequence of the mean dissipation $\langle \epsilon \rangle$ being constant/finite for the field of velocity derivatives at large Re. Since at large Re $\langle \epsilon \rangle = 2\nu \langle s^2 \rangle \approx \nu \langle \omega^2 \rangle$ this means that at large Reynolds numbers both $\langle s^2 \rangle, \langle \omega^2 \rangle \approx \nu^{-1}$, i.e. the field of velocity derivatives is not only Reynolds number dependent, but also becomes singular in the limit $\nu \to 0$ ($Re \to \infty$).[5] Due to the intermittent nature of the field of velocity derivatives one can expect that the maximal values of s^2, ω^2 (or $\max(|\partial u_i/\partial x_k|)$) increase even faster with the Reynolds number. This possible unboundedness of the field of velocity derivatives as $Re \to \infty$ has an implication that the Newtonian approximation can break down as $\nu \to 0$, since the linear stress/strain relation is only the first term in the gradient expansion. So far, however, there seems to be no evidence that the Navier- Stokes equations are inadequate for describing turbulent flows, i.e. breakdown of the continuum approximation, which is also an indication of absence of breakdown of the NSE due to possible formation of singularities in finite time; but still it is safer to keep in mind that any equations are not Nature. In this context it is instructive to recall a quotation from Goldstein (1972) concerning the success of NSE equations for the laminar flows of viscous fluids, but even in this case, *it is, in fact, surprising that the assumption of linearity in the relation between τ_{ij} and s_{ij} as usually employed in continuum theory,...works as well, and over as large a range, as it does. Unless we are prepared simply to accept this gratefully, without further curiosity, it seems clear that a deeper explanation must be sought.* Among the reasons that the possible violation is not so easy to detect is that, even if it happens, it will occur at rather large Reynolds numbers and small regions due to the strong intermittency of the field of velocity derivatives.

Let us turn to the Re-dependence at large Reynold numbers. There exists considerable evidence for Reynolds number dependence of various properties in different turbulent flows; for a partial list of recent references, see Tsinober (2009). The first example is the flatness factor of the streamwise velocity derivative $\partial u_1/\partial x_1$ is increasing from $3 \div 4$ at $Re_\lambda \sim 10$ to about 40 at $Re_\lambda \sim 4 \cdot 10^4$, without showing any trend for saturation, see Fig. 6 in Sreenivasan and Antonia (1997), Gulitskii et al. (2007a) and references therein. There is no understanding of the reasons for such a strong Reynolds number dependence at large values of the Reynolds number. Another example is about the Reynolds number dependence of the relation between the solenoidal and irrotational 'components' of the nonlinearity as represented by $(\mathbf{u} \cdot \nabla)\mathbf{u}$ and $\omega \times \mathbf{u}$, Fig. 8.8. There is a clear tendency of enhancement of solenoidal-

[5]The differences in the behavior of systems with different dissipation at finite Reynolds numbers points to a possibility that the hypothetical limiting solution will depend on the kind of dissipation we have at finite Reynolds number. For example, assume that for a hyperviscous case the mean dissipation $\epsilon \to const$ (or just nonvanishing) as some viscosity ν_h goes to zero, then velocity derivatives (both vorticity and strain) grow on the average as $\nu^{-1/2h}$, which compared to the Newtonian case $h = 2$ is pretty slow, if say, $h = 8$ as used in many simulations.

ity of the nonlinearity as the Reynolds number increases. Here too it is not clear what will happen when the Reynolds number will become very large.

The third example is the behavior of acceleration, see Fig. 8.9. Though the evidence is not conclusive, the indication is that even at large Reynold numbers there is no saturation of acceleration variance.

In Fig. 8.9 a number of points are shown obtained by Gulitskii et al. (2007b). The main feature is that there seems to be no saturation in the Re-dependence of the acceleration variance normalized on $\varepsilon^{3/2}\nu^{-1/2}$. This means that the scaling proposed by Yaglom (1949), see Monin and Yaglom (1975, p. 369) is not 'perfect' and the acceleration variance is larger than that proposed by Yaglom. The trend seen in Fig. 8.9 may be contaminated by the imperfections of the method. The issue seems to be open and requires further far more precise measurements.

Another aspect of concern is that $a^2 \sim \varepsilon^{3/2}\nu^{-1/2} \sim u_0^{9/4}L^{-3/4}\nu^{1/4}$, i.e. the acceleration variance along with viscosity, ν, depends explicitly on the large scale characteristics, u_0, of the flow contrary to the claim that *the turbulent acceleration is determined largely by the very small-scale motions* $l \lesssim \eta$, Monin and Yaglom (1971). Indeed, the observations, see Fig. 7.6 right, show that the conditional statistics of

Fig. 8.8 Reynolds-number dependence of the ratio of the variances of the irrotational and solenoidal parts of the nonlinear term $(\mathbf{u}\cdot\nabla)\mathbf{u}$ and $\omega \times \mathbf{u}$ in a DNS simulation of quasi-isotropic turbulence, Tsinober et al. (2001)

Fig. 8.9 Normalized acceleration variance, $a_0 = (1/3)a_k a_k^{3/2}\nu^{-1/2}$ versus Re_λ, from Gylfason et al. (2004) with added experimental data from field experiment, Gulitskii et al. (2007b) and from the PTV experiments, Lüthi et al. (2005)

a^2 on u_0 show a significant statistical dependence, see also Biferale et al. (2004) and Lüthi et al. (2005) and references therein. The bottom line is that *fluid particle acceleration variance does not* (seem to) *obey K41 scaling at any Reynolds number* Hill (2002).

Finally, as discussed above, the so called 'anomalous scaling' in the "inertial range" is due to viscous/diffusive effects with two options. One is just the finite Reynolds number effect, though the latest observations are at $Re_\lambda \sim 10^4$. The other option is the influence of viscous/diffusive effects in the tails (i.e. strong events) of corresponding PDFs and is expected to be present at any Reynolds number. The evidence seems to favor the latter option. In this case there remains the question whether the "anomalous scaling" is Reynolds number independent.

These examples, along with other results, show that the issue of the asymptotic 'ultimate' regime/state of turbulent flows at very large Reynolds numbers remains open. Unfortunately, progress in mathematical treatment of the problem is very small if any, especially as related to basic physical aspects, for recent example see Constantin (2016) and Sects. 10.2, 10.3 in Tsinober (2009).

8.3 Symmetries

The starting point is the symmetries of the NSE and Euler

The Euler and the Navier Stokes equations are invariant under the following transformations, see e.g. Frisch (1995):

Translations in space (homogeneity) and time,

Full group of rotation including rotations and reflections (isotropy),

Galilean transformation $u(x,t) \Rightarrow u(x-Ut,t)+U, \quad U = const,$

The Euler equation is in addition invariant under

time reversal $t \Rightarrow -t, \quad u \Rightarrow -u, \quad p \Rightarrow p.$

Scaling transformation $r \Rightarrow \lambda r; \quad t \Rightarrow \lambda^{1-h} t; \quad u \Rightarrow \lambda^h u; \quad p \Rightarrow \lambda^{2h} p, \quad \lambda > 0$ for any h for Euler equations.

The Navier–Stokes equations obey the scaling transformation for $h = -1$ only. However, it is not that uncommon belief that it *"may be justified at very high Reynolds number...that there are infinitely many scaling groups, labeled by their scaling exponent h , which can be any real number"*, i.e. *in the inviscid limit, the Navier–Stokes equation is invariant under infinitely many scaling groups, labeled by an arbitrary real scaling exponent h* Frisch (1995, pp. 18, 144), just like in the case of Euler equation. However, it is more than problematic why one can ignore the singular nature of the limit $Re \to \infty$ ($\nu \to 0$). It is not at all clear why one should forget that at any finite however large Re there is no such freedom as there is for a pure Euler case, especially when using experimental data at pretty moderate Reynolds numbers both for their "explanation" by the multifractal formalism and simultaneously its "confirmation" by the same data.

One of the issues of general nature is the belief//hypothesis that in "*turbulence at very high Reynolds numbers all or some possible symmetries are restored in a*

statistical sense and that *for this it is necessary that flow should not be subject to any constraint, such as a strong large-scale shear, which would prevent it from 'accepting' all possible symmetries* and that *in order to achieve maximum symmetry it is advantageous not to have any boundaries. We could thus assume that the fluid fills all of the space* $\mathbb{R}^3$. *The unboundedness of the space does, however, lead to some mathematical difficulties. We shall therefore often assume periodic boundary conditions in the space variable r* (in a box of scale L) and recovering *the case of a fluid in the unbounded space* $\mathbb{R}^3$ *by letting* $L \to \infty$, Frisch (1995, p. 11, 14).

In this context the first question is how relevant such an idealized turbulent flow is to any real one even at the qualitative level. The second question is about how real, even approximately, is the afore mentioned hypothetical(!) restoring of the symmetries in the statistical sense. The experimental and DNS evidence for this hypothesis is scarce, being very limited and at best marginal.

It is for the above reasons that Kolmogorov (1941a) proposed a hypothesis postulating that all the symmetries (including homogeneity and isotropy) of the Navier–Stokes equations are restored in the statistical sense in **any** turbulent flow for large enough Reynolds number *in sufficiently small regions G of the four-dimensional space* (x_1, x_2, x_3, t) *(i.e. time-space) not lying close to the boundaries of the flow or its other special regions*[6] – except for the one involving scaling. In order to handle the latter Kolmogorov introduced the concept of inertial range (IR) with the assumption that the statistical properties of this conventionally defined IR (CDIR) are independent of viscosity, thus assuming the scale invariance in the IR with the mean dissipation $\langle \epsilon \rangle$ being the only governing parameter.

There are numerous interpretations and ascribings of many things not belonging to Kolmogorov. The relevant one is the assumption of the localness of interactions (Frisch 1995, p. 104) - we use the term locality. The above involves some decomposition just like the suggestion by Kraichnan (1974) about the 'proper inertial-range quantity: the local rate of energy transfer' with all the problems in proper definitions of this quantity and use of Fourier transform. One more issue deserving special mentioning is the claim that the Euler equations are valid in the inertial range. For example (among numerous) Landau and Lifshits (1987), Migdal (1995), e.g. *In the inertial range we can neglect the viscosity and forcing and study the Eulerian dynamics of an ideal incompressible fluid*. Even if the inertial range did exist (which it does not) after excluding some range of scales such a statement by itself does not seem to make any sense - after all Euler is a differential equation. Finally, Kolmogorov used exclusively dimensional analysis and similarity arguments and did not make any claims about the validity of Euler in the inertial range.

The consensus view is that in real systems the symmetries are broken due to instabilities, forcing (excitation), inflow, initial and boundary conditions , but that following Kolmogorov the symmetries are expected to be restored locally in statistical sense at high Reynolds numbers. However, this expectation ignores the role of the

[6]Frisch (1995) presents this in the form of his hypothesis H1 (p. 74), but omits to mention that it is due to Kolmogorov: there is no presentation of the hypothesis of local isotropy in his book. A similar omitting is pretty ubiquitous, among the latest example being Falkovich (2009).

nonlocal nature of turbulence. *The consequence due to the effects of nonlocality is that, one cannot generally apply results for globally homogeneous, isotropic turbulent flows to flows which are approximately homogeneous in a bounded region of flows and otherwise are non-homogeneous/non-isotropic. This is because the nonlocality makes local homogeneity, isotropy, etc. impossible unless the whole (infinite extent) flow is such, which is trivially impossible. The "quasi-homogeneous" behavior of some quantities in bounded regions is misleading due to several factors, the main being the nonlocal nature of turbulence. These are typically quantities and relations exhibiting "quasi-Gaussian" manifestations.* On these latter see Sect. 6.8.2, Chap. 6 in Tsinober (2009).

As mentioned the attraction of Kolmogorov's hypotheses is that they concern the local properties of any turbulence, not necessarily homogeneous or isotropic or decaying or stationary, provided the Reynolds number is large enough. The consistency of these hypotheses has been debated for quite a while both from the theoretical point of view, Gkioulekas (2007), Hill (2006) and references therein, and also experimentally, but as with other similar issues without much progress from the fundamental point. To reiterate *one of the main problems is the inherent property of nonlocality questioning the validity of the above hypotheses on local homogeneity/isotropy in bounded flow domains surrounded by nonhomogeneous/nonisotropic flow regions,* see the example on the flow in plane channel in Sect. 7.3 and references therein.

A similar controversial situation is with spatially developing flows claimed to possess the property of self-similarity in analogy with von Kármán (1937) and von Karman and Howarth (1938) as an attempt to "solve" their famous (KH) equation, containing both second and third order correlations. This required a hypothesis to close the KH equation which is known as the so-called self-preservation (or self-similarity) hypothesis. Such a claim is essentially an assumption of locality both in time and space: the turbulent motion at some point in time and space is assumed to be defined by its immediate proximity.

A special example is about reflectional symmetry, e.g. involving helicity defined as the integral of the scalar product of velocity and vorticity, $H = \int \mathbf{u} \cdot \omega dx$, see Tsinober (2004). The hypothesis of local isotropy (K41a) includes restoring of all the symmetries in small scales. Thus one expects restoring of reflection-invariance at small scales. However, to maintain finite "helicity dissipation" to balance the finite helicity input in a statistically stationary turbulence helically forced at large scales the tendency to restore reflection symmetry at small scales can not be realized. This is because helicity dissipation is associated with broken reflection symmetry at small scales, since helicity dissipation, $D_H = -\nu H_s$ is just proportional to the superhelicity $H_s = \int \omega \cdot curl\omega dx$, showing the lack of reflection symmetry of the small scales. The important point is that helicity dissipation is vanishing if reflectional symmetry holds in small scales. Moreover, if the helicity dissipation should remain finite as the Reynolds number increases this lack of reflectional symmetry should increase since the dissipation of helicity ($D_H = -\nu H_s$) is proportional to viscosity, see Galanti and Tsinober (2006). A noteworthy phenomenon is the breaking of reflectional symmetry in a turbulent flow with initially vanishing pointwise helicity density $\mathbf{u} \cdot \omega \equiv 0$, Shtilman et al. (1988) - a result that can be interpreted in terms of the statistical

instability of turbulent flow with reflectional symmetry to disturbances breaking this symmetry, see also a non-trivial experiment in Kholmyansky et al. (2001).

To repeat, the role of nonlocality is the same in the issue concerning the scale invariance in the conventionally defined inertial range (CDIR). The experimental results at high Reynolds numbers show unequivocally that such an object is ill defined and that the CDIR in reality contains dissipative events which among other things are responsible for what is called anomalous scaling in the CDIR.

The bottom line is that there is no answer to the question by Kraichnan (1974): *How a theoretical attack on the inertial-range problem should proceed is far from clear,* because this question relates to a nonexistent object. A similar statement is true as concerns the 'theories' such as multifractal formalism, Frisch (1995), and some others attempting to 'explain' intermittency and 'anomalous scaling' in such a nonexistent object, for more see Sects. 5.3.1 in Chap. 5, Chaps. 6 and 9 in Tsinober (2009).

On analogies involving symmetries and related issues A general aspect is that it is not enough for two systems to share the same basic symmetries, conservation laws and some other general properties, or to have the same or similar basic properties. However, it is a common practice and there are a multitude of models, e.g. using results from the so called "passive turbulence" (i.e. a passive scalar in a random velocity field - not necessarily physical) to draw conclusions about genuine turbulence obeying the NSE. However, this is not really the case, as pointed out by Kraichnan (1974) in a counter example of a '*dynamical equation is exhibited which has the same essential invariances, symmetries, dimensionality and equilibrium statistical ensembles as the Navier–Stokes equations, but which has radically different inertial-range behavior.*' The issue is much broader. Using only the above properties many essential properties are missed, such as a variety of dynamically relevant geometrical aspects. The first example, concerns the interaction of vorticity and strain and enstrophy production. Here the geometrical relations between the vorticity and the eigenframe of the rate of strain tenso rare of the utmost importance; see below Chap. 6 and Sect. 6.4 in Tsinober (2009).

As mentioned in Sect. 5.1.1 Chap. 5 the issue extends into broad misinterpretations of a variety of analogies such as between the genuine (e.g. NSE) turbulence and passive "turbulence", i.e. evolution of passive objects in random (or just not too simple) velocity fields. The differences are more than essential, though there are numerous claims for *the well-established phenomenological parallels between the statistical description of mixing and fluid turbulence itself*, Shraiman and Siggia (2000). Of special interest here is the anomalous scaling in genuine versus passive turbulence, e.g. a recent statement by Eyink and Frisch (2011, p. 362): "*Kraichnan's model of a passive scalar advected by a white-in-time Gaussian random velocity has become a paradigm for turbulence intermittency and anomalous scaling*. The authors mean the genuine NSE turbulence. We just repeat that the analogy between genuine and passive turbulence is illusive and mostly misleading, for more on this see above Sect. 6.1.1 Interpretation.

Far more can be found on misuse and misinterpretations of analogies in Chap. 9 in Tsinober (2009) and references therein.

On periodic boundary conditions There is considerable evidence that the statistical properties of some turbulent flows with the same geometry at very modest Reynolds numbers are invariant of the boundary and even initial conditions (BC and IC). For example, typical DNS computations of NSE of turbulent flows (e.g., in a circular pipe and a plane channel, in a cubic box, etc.) involve extensive use of periodic BC. The results of these computations agree well with those obtained in laboratory experiments, in which the BC have nothing to do with periodicity. Indeed, the correlation coefficient between two values of any quantity at the opposite ends of such boundaries (i.e., the points separated at maximal distance in the flow domain) is precisely equal to unity and close to unity for the points in the proximity of such boundaries. However, is in any real flow the correlation coefficient becomes very small for points separated by a distance on the order of (and larger than) the integral scale of turbulent flows.) and in which the IC were totally different from those in DNS. No explanation of this kind of invariance is known so far, but it is natural to expect that it is related to some kind of hidden symmetry(ies) of the NSE. If such exist, this may be the reason for the similarity of results obtained via DNS of NSE in, e.g., periodic boxes by various forcing (different deterministic, random/stochastic, etc.).

The above property, is however, not universal and there are many examples of long memory of turbulent flows which do 'remember', e.g., the inflow and initial conditions, sensitive to finiteness of the physical domain, etc.

As mentioned the reasons for this behavior may involve nonlocal properties in conjunction with the changing environment so that this "memory" is an attribute of a transient phenomenon in spatially developing flows due to the continuously changing conditions preventing an approach to some universal state/behavior.

8.4 Universality

The issue of universality is one of several continuously debated controversies in the problem of turbulence. This includes the meaning of the term 'universality' which always refers to behavior at large Reynolds numbers and related to symmetry in one way or another. For example, one issue involves the invariance of Reynolds number of (some) properties of a particular turbulent flow at large enough Reynolds numbers. Another issue is concerned with the universality of (mostly scaling) properties of small-scale turbulence assumed to be the same in any flow, which has remained for more than fifty years one of the most active fields of inquiry. Derivation of the scaling properties of fully developed turbulent flows directly from the Navier–Stokes equations analytically is one of the most popular elusive goals of theoretical research. Since the Kolmogorov papers (1941a, b), there still exists almost religious belief in

some universal properties of small scale turbulence.[7] This belief was strengthened by achievements in dynamical chaos, such as the discovery of some universal numbers by Feigenbaum (1997), etc.

The expectation of universality in turbulence stems from the nonlinear nature of turbulence. However, this expectation to a large extent did not come true due to much neglected set of properties of an essentially nonlocal nature. This includes practically all cases where quantitative universality was expected. In other words, the problems with universality are due to the competition between nonlinearity and nonlocality, the latter is also aided by linear processes, such as in the proximity of walls, Kim (2012) and references therein. However, as it stands now, the main problems are with quantitative universality, i.e. universality of numbers and the necessity of distinguishing between the quantitative and the qualitative universality, i.e. involving some more general universal features and processes.

8.4.1 Quantitative Universality

> *It might be thought that the possibility exists in principle of obtaining a universal formula, applicable to any turbulent flow, which should give Brr and Btt for all distances r that are small compared with l. In fact, however, there can be no such formula, as we see from the following argument. The instantaneous value of* $(v_{2i} - v_{1i}.)(v_{2k} - v_{lk})$ *might in principle be expressed as a universal function of the energy dissipation* ε *at the instant considered. When we average these expressions, however, an important part will be played by the manner of variation of* ε *over times of the order of the periods of the large eddies (with size - l), and this variation is different for different flows. The result of the averaging therefore cannot be universal.* Landau and Lifshitz (1987, p. 140), due to Landau (1944) in Russian. They add: *The question whether fluctuations of* ε should be reflected in the form of the correlation functions in the inertial range can scarcely be resolved with certainty until we have a consistent theory of turbulence.

With the exception of the 2/3(5/3) and the 4/5 laws there appeared to exist no quantitative universality so far: the first doubt came from the famous remark by Landau in the first Russian edition of Fluid Mechanics by Landau (1944) about the fluctuations of the energy dissipation rate. These were followed by various 'universal' corrections, which did not appear to be universal either. These corrections were followed by the (multi) fractal approach using either the so-called $D(h)$ or $f(\alpha)$ formalisms, in which the *functions* $D(h)$ and/or $f(\alpha)$ are assumed to be universal. However, they do not seem to be universal either. There is quite solid evidence accumulated during the last fifty years against the most beautiful hypothesis on the restoring of the symmetries in the statistical sense of the Navier–Stokes equations locally in time and space, i.e. local isotropy together with scale invariance. People therefore started to look for some universality in the anisotropic properties of turbulent flows, see ref-

[7] That far that *one of the principal incentives for writing this book was a desire to summarize the development of the idea of a universal local structure in any turbulent flow for sufficiently large Reynolds number*, Monin and Yaglom (1971, p. 21). This is 1600 pages total.

erences in Biferale and Procaccia (2005). This involves the SO(3) decomposition of tensorial objects again assuming universal scaling behavior in r of each component of the decomposition. Consequently there is no "simple" scaling of, say, structure functions in r, but rather the different terms of the SO(3) decomposition each with its own scaling exponent assumed to be universal. All the attraction of simple scaling as in Kolmogorov 41 has thus gone.

The assumption of universality has no serious justification and is more a kind of a belief much weaker than the belief in the inertial range. Moreover, it is not clear at all why each "sector" of the irreducible representation is expected to have its own universal scaling exponent independently of the physical/dynamical nature/underlying mechanisms of anisotropy such as mean shear, strain, rotation, stratification (both stable and unstable), magnetic field, etc. The expectation of universality is especially problematic in the case of strong anisotropy (Q2D) in all the above cases. There is a claim that the amplitudes of the various contributions are nonuniversal and that it is possible to fit the experimental data by keeping fixed the scaling properties and adjusting only the prefactors. Also noteworthy is that in determination of anisotropic scaling exponents one encounters the same kind of difficulties as those known from previous experience. This however, does not prove much regarding universality and may well be at best the "right result not necessarily for the right reason". One more difficulty may arise due to the nonuniqueness of the SO(3) decomposition in the sense that there exists more than one possibility to choose its basis in cases when the SO(3) decomposition is applied to tensorial objects. There are also similar claims on universality related to passive objects. This kind of a claim is quite surprising, as passive objects are governed by linear equations and their its statistics and scaling exponents are expected to be sensitive to the statistics of the velocity field.

The nonuniversality of the small scale structure of turbulence is mostly due to nonlocality along with the difference in the mechanisms of large-scale production. The latter depend on the particularities of a given flow and thus are not universal. The large scales, especially their edges seem to be responsible for the contamination of the small scales. This 'contamination' is unavoidable even in homogeneous and isotropic turbulence, since there are many ways to produce such a flow, e.g., many ways to produce the large scales. The situation is complicated by the back reaction of the small scales on the large ones, see also below Sect. 9.1.4 on direct and bidirectional interaction/coupling between large and small scales.

On the special status of scaling exponents of the second and third order structure functions The empirically observed Reynolds number independent behavior of some global characteristics of turbulent flows at large Re, such as the total dissipation, the drag of bluff bodies and resistance in many other configurations, comprises one of the quantitative universal properties of turbulence at large Re. This is distinct from some possibly universal (mostly scaling) properties of small-scale turbulence. The exceptions in some sense are the scaling exponents of the second and third-order structure functions.

It is widely thought that the Kolmogorov similarity hypotheses imply that the mean dissipation, remains finite/non-vanishing as $Re \to \infty$, whereas they are based

on an assumption that this is the case. The issue is whether the properly normalized mean dissipation (usually by U^3/L^{-1}as suggested by Taylor 1935) tends really to a finite limit as $Re \to \infty$ ($\nu \to 0$), or is Re dependent even at very large Reynolds numbers. There is much speculation about this subject, while experimental and recent evidence from DNS favoring the former is solid but still limited, although in a variety of fluid engineering practices this fact ha been recognized long in a great variety of flow configurations since the end the 19th century. *Since the finite limit of the mean dissipation at large Re defines a unique scaling exponent in the* 2/3 *law and since the* 4/5 *law is a consequence of the Navier–Stokes equations, the two scaling exponents,* $\zeta_2 = 2/3$ *and* $\zeta_3 = 1$*, possess a special status provided that the inertial range is a well defined concept (which is not) and the mean dissipation is indeed Reynolds number independent as* $Re \to \infty$. It is noteworthy that rigorous upper bounds of the long time averages of dissipation are independent of the Reynolds number at large Re, Doering (2009) and references therein, and thereby are consistent with the experimental results. This, however, does not fully resolve the problem since there are no results on the lower bounds, except trivial values corresponding to the laminar flows. There are also experimental results on the energy spectra at large Reynolds numbers with the exponent steeper than $-k^{-5/3}$ (see references and new results in Tsinober 2009) and there is a need for different Reynolds numbers in the experimental confirmations of the 4/5 law at large Reynolds number, Kholmyansky and Tsinober (2009). Thus it makes sense to keep in mind the possible alternatives, see e.g. Grossman (1995), Long (2003) and references in Abe and Antonia (2011).

In other words, it seems that the dream of quantitative universality of turbulence, i.e. the universality of numbers, may never come true.[8] The main reason is the nonlocality leading not only to the breaking of the symmetries embodied in the Navier–Stokes equations, but also preventing them from being restored in the statistical sense at high Reynolds numbers, along with the ill posed concept of inertial range as reiterated below in the next subsection on qualitative universality.

8.4.2 Qualitative Universality

Though the quantitative universality of turbulence, i.e., universality of numbers, may not exist, a qualitative universality does seem to exist. The concept of qualitative universality is not just a fuzzy idea. First, the major qualitative features of turbulent flows as described in Chap. 1 are universal for all turbulent flows arising in qualitatively different ways and circumstances and generally characterize turbulent flows as a whole. It is these qualitative features which do not depend on the diverse processes/ways by which flows become turbulent. Second, these qualitative features possess a number of quantitative attributes/features which are more specific for turbulent flows and are of special interest. Third, as mentioned it is natural to use the major universal

[8]For other negative statements about universality see, for example, Saffman (1978) and Hunt and Carruthers (1990).

qualitative manifestations of turbulent flows as a basis and as a first step to 'define' what turbulence is.

In other words, apart from the general qualitative properties of turbulence as apparent randomness, enhanced effective diffusivity and dissipation, rotational nature, and others as discussed above, (e.g. in Chaps. 1 and 6), - the more specific qualitative universal properties are of particular interest along with the quantitative attributes.

We attend below to a number of the most important issues on or related to qualitative universality.

Turbulent flows possess (empirically) stable statistical properties (SSP), not just averages but almost all statistical properties. In case of statistically stationary flows the existence of SSP seems to be an indication of the existence of attractors in mathematical terms. But matters are more complicated as many statistical properties of time-dependent (in the statistical sense) turbulent flows possessing no attractor but stable SSP are quite similar (at least qualitatively) to those of statistically stationary flows; that is, providing as long as the Reynolds number of the former is not too small at any particular time moment of interest. This can be qualified as qualitative temporal universality/memory.

Velocity derivatives, Self amplification, Tennekes and Lumley balance Most important is the process of self-amplification of velocity derivatives, both strain and vorticity which turns turbulence into a strongly dissipative and rotational phenomenon. These two concomitant processes posses a quantitative universal aspect as reflected in the Tennekes an Lumley balance and a similar relation for strain production, see above Chap. 7. It should be stressed again that there is an essential and qualitative difference between the process of self-amplification of strain and other similar processes. It is a specific feature of the dynamics of turbulence having no counterpart in the behavior of passive objects. In contrast, the process of self-amplification of vorticity, along with essential differences, has a number of common features with analogous processes in passive vectors; in both the main factor is their interaction with strain, whereas the production of strain is a self-production provided that the flow is rotational. It is the the process of strain self-amplification which is responsible for the dissipation anomaly (DA) of three-dimensional turbulence. The qualitative universality of the field of velocity derivatives is to due the independence of their self-amplification of "mediators".

The amplification of derivatives in other systems as passive objects is a property which can be seen as qualitatively universal not only for any random fluid flow, be it real or artificial, such as the Gaussian velocity field, but of any fluid flows that are Lagrangian chaotic, many of which are simple laminar in the Eulerian setting.

On universal aspects of turbulence structure In dynamical systems, one looks for structure in the *phase space,* Shlesinger (2000), Zaslavsky (1999), since it is relatively 'easy' due to the low dimensional nature of the problems involved. In turbulence nothing is known about its properties in the corresponding infinite or

very high dimensional phase space.[9] Therefore, it is common to look for structure in the *physical space* in the hope that the structure(s) of turbulence - as observed in *physical space* - is (are) the manifestation of the generic structural properties of mathematical objects *in phase space*, which are called attractors and which are in some sense invariant. In other words, the structure(s) is (are) assumed to be 'built in' to the turbulence independently of its (their) origin - hence universality. However, as mentioned, the expectation for universal numbers seems to be unjustified. It is more natural to expect universal qualitative statistical features in the physical space rather than universal numbers. Indeed, some of these features have already been observed, which are common for very different - essentially all known - turbulent flows. These are not only the general qualitative features of turbulent flows as described in Chap. 1, but rather specific ones.

We bring three examples with features which are essentially the same for all known incompressible flows such as grid turbulent flow, periodic flow in a computational box, turbulent boundary layer and channel flow, mixing layer and also compressible flows. Such features can be seen as universal statistical manifestations of the structure of turbulent flows.

The first example, is the so-called 'tearing drop' feature observed in the invariant map of the second invariant, $Q = \frac{1}{4}(\omega^2 - 2s_{ik}s_{ik})$, versus the third invariant $R = -\frac{1}{3}(s_{ik}s_{km}s_{mi} + \frac{3}{4}\omega_i\omega_k s_{ik})$ of the velocity gradient tensor $\partial u_i/\partial x_k$, such as shown in Fig. 6.1. This feature appears to be essentially the same for all known turbulent flows. We draw attention to the 'tail' of the teardrop which is mainly located in the quadrant $Q < 0,\ R > 0$, in which most of the turbulent activity happens in a variety of ways. The important point is that this is the region dominated by strain as compared with enstrophy, $2s_{ik}s_{ik} > \omega^2$, and by *production* of strain as compared with production of enstrophy, $-s_{ik}s_{km}s_{mi} > \frac{3}{4}\omega_i\omega_k s_{ik}$. This is in full conformity with the behavior of nonlinearities in these regions, see Sect. 6.5 and Chap. 10.1.3 in Tsinober (2009).

The second example is related to the depression of nonlinearity. We mention here one aspect of this problem, which seems universal in the sense that it is true for different flows (including shear) and different Reynolds numbers, though the evidence is still quite limited. Namely, all key nonlinearities appear to be much stronger in the strain dominated regions rather than in regions with concentrated vorticity. This is in contrast to the common expectation that, for example, the vorticity amplification process will be strongest where the vorticity already happens to be large. The regions with concentrated vorticity are in approximate equilibrium in the sense that the rate of enstrophy production is in approximate balance with the viscous destruction in these regions even at low Reynolds numbers. Therefore, their life time is considerably longer than the life time of the regions dominated by strain. These are in strong disequilibrium in the sense that the rate of enstrophy production is much larger than its destruction by viscosity in these regions. This is reflected in the $R - Q$ diagram as the strongest activity in the fourth quadrant in which Q and R are negative, i.e. $2s^2 > \omega^2$ and $-s_{ik}s_{km}s_{mi} > \frac{3}{4}\omega_i\omega_k s_{ik}$.

[9] Hopf (1948) conjectured that the underlying attractor is finite dimensional due to presence of viscosity.

It is noteworthy that the issue of depression of nonlinearity was initiated via comparison of nonlinearities in real turbulent flows with their Gaussian counterparts with the results that turbulent flows tend to deplete nonlinearity significantly. The problem is that such a comparison is too narrow as it is only meaningful for even moments. For a Gaussian field all odd moments such as, e.g. strain and enstrophy production, vanish identically. In other words, measured by odd moments the real nonlinearity is infinitely larger, see Tsinober (2009, p. 154).

The third example is related to geometrical statistics for example those of enstrophy production as a process of interaction of vorticity and strain. These are various alignments between vorticity and the eigenbasis of the rate of strain tensor, and between vorticity and the vortex stretching vector. It appears that these and many other similar properties are the same for all known flows and, moreover, for a broad range of Reynolds numbers. For example, the character of the above alignments is essentially the same at $Re_\lambda \sim 10^2$ and $Re_\lambda \sim 10^4$.

The same similarity was observed recently for a variety of alignments and other properties associated with fluid particle accelerations, Gulitskii et al. (2007b) and for passive scalars, Gulitskii et al. (2007c), such as alignments of passive scalar gradient, $\mathbf{G}$ with the eigenframe λ_i of the rate of strain tensor, conditional averages on ω^2 and s^2 of the production $-G_i G_j s_{ij}$, tilting of $\mathbf{G}$ and some others.

A more general aspect is that the universality of the structure of the field of velocity derivatives is due to the universality of the process of their self-amplification. This is an important example showing that it makes sense to speak about **universality of processes.**

There is a far more difficult issue of universality (or not) of finite objects in turbulence, which is discussed in the next chapter.

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Chapter 9
Intermittency and Structure(s) of and/in Turbulence

Abstract Intermittency specifically in genuine fluid turbulence is associated mostly with some aspects of its spatiotemporal structure and its relation to the essential dynamical aspects. Hence, the close relation between the origin(s) and meaning of intermittency and structure of turbulence on one hand and of intermittency, structure and the dynamics, on the other hand. Just like there is no general agreement on the origin and meaning of the former, there is no consensus regarding the origin(s) and or meaning of turbulence structure(s). The present state of matters is that both issues are pretty speculative, an example of an 'ephemeral' collection of such is given in this chapter. We must admit at this stage that structure(s) is(are) just an inherent property of turbulence. Structureless turbulence is meaningless. There is no turbulence without structure(s). Every part (just as the whole) of the turbulent field—including the so-called 'structureless background'—possesses structure. Structureless turbulence (or any of its part) contradicts both the experimental evidence and the Navier–Stokes equations. The claims for 'structureless background' are a reflection of our inability to 'see' the more intricate aspects of turbulence structure: intricacy, complexity and 'randomness' are not synonymous with absence of structure. What is definite is that turbulent flows have lots of structure(s). The term structure(s) is used here deliberately in order to emphasize the duality (or even multiplicity) of the meaning of the underlying problem. The first is about how turbulence 'looks'. The second implies the existence of some entities. Objective treatment of both requires use of some statistical methods. It is thought that these methods alone may be insufficient to cope with the problem, but no satisfactory solution has been found. One (but not the only) reason—as mentioned—is that it is not so clear what one is looking for: the objects still seem to be elusive. For example, a non-negligible set of people in the community doubt that the concept of coherent structure is much different from the Emperors's New Clothes. An example of the acute difficulties described in this chapter is associated with the high dimensionality of what is called structures. Simple single parameter thresholding is inappropriate in drawing statistics due to the painful question of how really "similar" all these structure are if the individual members of such an ensemble are defined by one parameter only. An acute issue is about the spatiotemporal identity of what are called "structures", which is not the same what is denoted by the term "structure" of turbulence. The view that turbulence structure(s) is(are) simple in some sense and that turbulence can be represented as a collection

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of simple objects only seems to be a nice illusion which, unfortunately, has little to do with reality. It seems somewhat wishfully naive to expect that such a complicated high-dimensional phenomenon like turbulence can merely be described in terms of collections of only such 'simple' and weakly interacting objects. *On the contrary, intermittency is an important feature of the common (!) nonlocal life of large and small scales and small scales both dynamically and kinematically.*

The nature and characterization of the structure(s) of turbulent flows are among the most controversial issues in turbulence research with extreme views on many aspects of the problem - in words of Richard Feynman (1963, p. 41–12), *holding strong opinions either way*. For example, it is common in the vast literature on turbulence to consider the terms *statistical*, *structural* (and *deterministic*) as incompatible or even contradictory, see for example Dwoyer et al. (1985), Lumley (1989). However, there are common points as well. For instance, it is mostly agreed that turbulence definitely possesses structure(s) (whatever this means) and that intermittency, which is addressed in the following subsection, is intimately related to *some* aspects of the structure(s) of turbulence.

It has become clear that it is a misconception to contrapose the *statistical,* the *structural* and the *deterministic* and that they represent different facets/aspects of the same problem, so that there is no real gap between structure(s), statistics (but not in the sense of absence of laws) and determinism. Similarly, it seems impossible to separate the structure(s) from the so called 'random structureless background' or the '*random processes from the nonrandom processes*', Dryden (1948) due to strong interaction and nonlocality, both between individual structures, and between structures and the 'background'. In other words, there is no turbulence *without* structure, every part of the turbulent field just like the whole possess structure.[1] Structureless turbulence contradicts both the experimental evidence and the Navier-Stokes equations. It is noteworthy that the statement that turbulence has structure is in a sense trivial: *to say that turbulent flow is 'completely random' would define turbulence out of existence*, Tritton (1988), p. 295.

One of the basic properties underlying both aspects, i.e. intermittency and structure(s) manifest the essentially non-Gaussian nature of turbulence, which follows from NSE, e.g. Novikov (1967, 1974), Lumley (1970). There are numerous experimental observations on non-Gaussianity of turbulence, see e.g. Sect. 6.8 in Tsinober (2009) on non-Gaussian nature of turbulence, e.g. the outstanding non-zero odd moments: the strain and enstrophy production $-\langle s_{ij}s_{jk}s_{ki}\rangle$, $\langle \omega_i\omega_j s_{ij}\rangle$. It must be stressed that (i) *non-Gaussianity and intermittency/structure are not synonymous as not any non-Gaussian field is turbulent and (ii) non-Gaussianity is an important statistical manifestation of intermittency/structure.* We point out that Gaussian velocity fields are dynamically impotent and lack dynamically relevant structure(s).

In this context the claim that *Kolomogorov work on the fine-scale properties ignores any structure which may be present in the flow,* Frisch (1995, p. 182) con-

[1] There are proposals to *scan out* the structure(s). In fact there is no way to do so, since *structure* is everywhere. Even the so-called 'simple' structures, worms, sheets, etc. are 'renormalized' by the background.

tradicts the 4/5 law as one of the outstanding manifestations of the non Gaussianity of turbulent flows. Aslo note the very title of the Kolmogorov (1991a) paper.

An interesting aspect that non-Gausianity, and time irreversibility (i.e. dissipative nature) are concomitant features of turbulence, Betchov (1974, 1976) see above the Sect. 7.5 on both, also Sect. 6.8.2 in Tsinober (2009). Hence the importance of parameters related to the non-Gaussian nature of turbulence and not only in various contexts of intermittency and structure(s).

9.1 Intermittency

At any instant the production of small scales is... occurring vigorously in some places and only weakly in the others Tritton (1988).

Intermittency is a phenomenon where Nature spends little time, but acts vigorously, Betchov (1993).

Typical distribution of scalar and vector fields is one in which there appear characteristic structures accompanied by high peaks or spikes with large intensity and small duration or spatial extent. The intervals between the spikes are characterized by small intensity and large extent (Zeldovich et al. 1988).

Intermittency is an important feature of the common (!) nonlocal life of large and small scales both dynamically and kinematically.

The term intermittency is used in two distinct (but not independent) aspects of turbulent flows. The first one is the so-called external intermittency. It is associated with what is called here partly turbulent flows, specifically with the strongly irregular and convoluted structure and random movement of the 'boundary' between the turbulent and nonturbulent fluid.

The second aspect is the so-called small scale, internal or intrinsic intermittency and is associated with spotty temporal and spatial patterns of the small scale structure(s) in the interior of turbulent flows.

9.1.1 The External Intermittency and Entrainment

This kind of intermittency was studied first by Townsend (1948) and involves the so called entrainment. This is one of the most basic processes of the transition from laminar to turbulent state with the coexistence of both states in one flow. It is associated with the coexistence of laminar and turbulent flow regions. An observer located in the proximity of either side of the 'mean' boundary between these regions observes intermittently laminar and turbulent flow in the form of a signal similar to that as in Fig. 21.4 in Tritton (1988); this clearly demonstrating the external intermittency in the wake past a circular cylinder. Here again we encounter the question about what turbulence is. Looking at such signals one clearly sees what is turbulent and what is

laminar. But the question is how one can say whether a small part of flow is turbulent. In other words if turbulence is to be identified by statistical means, then what is the meaning of 'turbulent' locally? This involves taking difficult decisions about what is turbulent using some conditional criterion, see discussion and references in Kuznetsov et al. (1992).

The main mechanism by which nonturbulent fluid becomes turbulent locally as it crosses the interface is due to the viscous diffusion of vorticity across the interface. As this process is associated with small scales it is thought to be the reason why the interface appears sharp compared to the scale of the whole flow.

However, at large Reynolds numbers, the entrainment rate and the propagation velocity of the interface relative to the fluid are known to be independent of viscosity. Therefore *the slow process of diffusion into the ambient fluid must be accelerated by interaction with velocity fields of eddies of all sizes, from viscous eddies to the energy-containing eddies so that the overall rate of entrainment is set by large-scale parameters of the flow* (Townsend 1976). That is *although the spreading is brought about by small eddies* [viscosity] *its rate is governed by the larger eddies. The total area of the interface, over which the spreading is occurring at any instant, is determined by these larger eddies*, Tritton (1988). This is analogous to the dissipation independence of viscosity in turbulent flows at large Reynolds numbers. In other words, small scales do the 'work', but the amount of work is fixed by the large scales in such a way that the outcome is independent of viscosity.[2] This shows that independence of some parameter of viscosity at large Reynolds numbers does not mean that viscosity is unimportant. It means *only* that the (cumulative) effect of viscosity is Reynolds number independent, for more see Tsinober (2009), Wolf et al. (2012, 2013) and references therein.

9.1.2 The Small Scale, Internal or Intrinsic Intermittency

The second aspect is the so-called small scale, internal or intrinsic intermittency. It is usually associated with some tendency to spatial and temporal localization of the 'fine' or small scale structure(s) of turbulent flows with large probability of taking values of some variable (dissipation) both very large and very small compared to its standard deviation. An important feature is that regions of exceptionally high values make disproportionately large contributions (sometimes even dominant) to the integral properties even though occupying a small fraction of the flow. Consequently, regions with "voids" (low intensity) occupy a "disproportionately" large fraction of the flow (thus being statistically dominant, but contribute far less to the dynamics. For example, in field experiments with $Re_\lambda \approx 10^4$ the dissipative events with dissipation exceeding the mean 12 times (i.e. $\epsilon < 12\,\langle\epsilon\,\rangle$) contribute to $\langle\epsilon\rangle$ about 20% while taking about 1% of the total volume, see Fig. 9.1 The instantaneous values of dissipation exceed $10^4\,\langle\epsilon\rangle$.

[2] All this doas not mean absence of bidirectional nonlocal effects between large and small scales.

Fig. 9.1 Percent of the strong dissipative events as defined below and their contribution to the total dissipation (b) as a function of the threshold q for various separations r. An event $\Delta\mathbf{u} = [\mathbf{u}(\mathbf{x}+\mathbf{r}) - \mathbf{u}(\mathbf{x})]\mathbf{r}/r$ is qualified as strong dissipative if at one of its ends $(x+r)$, x the instantaneous dissipation $\epsilon > q\,\langle\epsilon\rangle$ with $q > 1$. The value of separation r is not relevant as concerns the contribution of the dissipative events to the total dissipation, Kholmyansky and Tsinober (2009)

Similar behavior is observed for other variables exhibiting intermittency, e.g. enstrophy, and enstrophy and strain production, see Tsinober (2009), Sawford and Yeung (2015), Yeung et al. (2015).

Our concern here is with the intermittency of this second kind. It is noteworthy that in a broad sense intermittency is a ubiquitous phenomenon occurring in a great variety of qualitatively different systems, see Zel'dovich et al. (1988) for a lively exposition of a wide number of different systems exhibiting intermittency, and also Vassilicos (2001). The main common features of them all is (space/time) irregularity and localization (both spatial and temporal) of their 'fine' structure. However, this is not enough to define intermittency. For example, almost any nonlinear function or almost any nonlinear functional of a random Gaussian field is intermittent in the above sense, though random Gaussian fields by definition lack any intermittency and are dynamically impotent.

We do not make a special distinction between the intermittency in the inertial and dissipative ranges as neither are well defined.

The intermittent nature of the small scale structure of turbulent flows was foreseen by Taylor (1938b): *...the view frequently put forward by the author that the dissipation of energy is due chiefly to the formation of very small regions where vorticity is very high.* However, note that dissipation is high in regions where strain – not vorticity – is high see above Chap. 7. Similarly, the enstrophy production, $\omega_i\omega_i s_{ij}$, is small where strain production, $s_{ij}s_{jk}s_{ki}$, is large and vice versa, see Fig. 7.5. *Such behavior is not unexpected since the viscous term in the NSE contains the highest-order derivative while high Reynolds number turbulence involves the limit* $\nu \to 0$. *This limit is a singular perturbation problem and localized regions in which gradients are large should be expected to form*, Orszag (1977). Along with nonlocality this means that

the consequence of intermittency is not just a small "correction" in the properties of turbulence with the possible exception of the quasi-gaussian ones.

The phenomenon of small scale intermittency was observed by Batchelor and Townsend (1949) in experiments with turbulent grid flows and in a wake past a circular cylinder.

Batchelor and Townsend (1949) wrote:

> *The basic observation which requires explanation is that activation of large wave-numbers is very unevenly distributed in space. These space variations in activation can be described as fluctuations in the spectrum at large wave-number... As the wave-number is increased the fluctuations seem to tend to an approximate on-off, or intermittent variation. Whatever the reason for the occurrence of these fluctuations, they appear to be intrinsic to the equilibrium range of wave-numbers. All the evidence is consistent with the inference that the fluctuations are small in the region of smallest wave-numbers of equilibrium range and become increasingly large at larger wave-numbers ... the mean separation of the visible activated regions is comparable with the integral scale of the turbulence, i.e. with the size of the energy-containing eddies.*

Batchelor and Townsend (1949, pp. 252–3) obtained some evidence that the deviation from Gaussianity is stronger as the Reynolds number is increased. However, they did not appreciate this effect and claimed that the flatness factors seems to vary a little with Reynolds number, although this factor changed from 5 to 7 for the third order derivative; see their figure 5 for the flatness factor of velocity derivatives of different orders and different Reynolds numbers. This was confirmed by a number of subsequent experiments such as by Kuo and Corrsin (1971), for an updated overview of the subsequent results see, e.g. Sreenivasan and Antonia (1997). An example of time records of the streamwise velocity component, and their derivatives obtained in a field experiment at $Re_\lambda = 10^4$ is shown in Fig. 1.17 in Tsinober (2009). The increasingly intermittent behavior of the signal with the derivative order is seen quite clearly. Also shown are records for the enstrophy ω^2, total strain $2s^2 \equiv 2s_{ij}s_{ij}$ and their surrogate $(\partial u_1/\partial x_1)^2$, and enstrophy production $\omega_i\omega_j s_{ij}$, $s_{ij}s_{jk}s_{ki}$ and their surrogate $(\partial u_1/\partial x_1)^3$. The experiment was performed in the atmospheric surface layer at a height 10m in approximately neutral (slightly unstable) conditions.

A qualitative summary is that small scale intermittency of turbulence is associated with its spotty/sporadically (spatio-temporal) *structure* which among other things is manifested as a *particular* kind of non-Gaussian behavior of turbulent flows. This deviation from Gaussianity, increases with both (1) increasing the Reynolds number and (2) decreasing the 'scale'. In other words, intermittency along with dynamical aspects involves two (not independent) aspects of turbulent flows - their structure/geometry and statistics**.** These two aspects are reflected in attempts to 'define' intermittency, see references in Tsinober (2009).

9.1.3 Measures/Manifestations of Intermittency

There are numerous quasi-Gaussian manifestations of turbulent flows, for some see above Sect. 7.5 Non-Gaussian nature of turbulence and Sect. 7.2.2 in Chap. 7, Tsinober (2009). Hence the importance of parameters related to the non-Gaussian nature of turbulence in various contexts of intermittency and structure(s). It is important that intermittency implies non-Gaussianity, but not necessarily vice versa – practically any parameter can at most indicate the degree of intermittency of a flow *already* known to be intermittent. A statistical measure such as flatness or some similar intermittency factors may deviate strongly from a Gaussian value without any intermittency in the flow field. The simplest example is the Gaussian field itself, which by definition lacks any intermittency. However, any nonlinear function (or functional) of a variable, which is Gaussian, is non-Gaussian. For instance enstrophy, dissipation, pressure, etc. of a Gaussian velocity field possess *exponential* tails and their flatness is quite different from 3. For example, for a Gaussian velocity field $F_G(\omega^2) = \langle\omega^4\rangle/\langle\omega^2\rangle^2 = 5/3$ and $F_G(s^2) = \langle s^4\rangle/\langle s^2\rangle^2 = 7/10$. This by no means indicates that, for a Gaussian velocity field, these quantities are intermittent as sometimes claimed. Moreover, the flatness of enstrophy is *larger* than that of total strain, $F_{\omega^2} - F_{s^2} = 29/30$. Similarly, the Reynolds stress $u_i u_j$ exhibits 'intermittency'. The main contribution to this intermittency comes from the fact that $u_i u_j$ is a product of two random variables both distributed close to Gaussian. For example, the PDF of the $u_1 u_2$ of the strongly intermittent signal obtained by Lu and Willmarth (1973) in a turbulent boundary layer is strongly non-Gaussian. However, the PDF of $u_1 u_2$ is approximated with high precision by assuming both u_1 and u_2 to be Gaussian with a correlation coefficient between them adjusted from the experiment -0.44 The above examples may serve as a warning that multiplicative models are able to produce intermittency for a purely nonintermittent field as is the Gaussian velocity field. A similar concern is what is called "Lagrangian" intermittency, e.g. "intermittency" of a purely Eulerian Gaussian field in its Lagrangian representation, see Sect. 3.4 above and 9.1.5 below. For recent examples see Wilczek and Meneveau (2014), Wilczek (2016) and references therein. The term "Lagrangian intermittency" is used also in the context of scaling exponents of Lagrangian structure functions of real turbulent flows, Sawford and Yeung (2015) and references therein, see below. See also Zel'dovich et al. (1988); Zaslavsky (1999) on interesting observations on this and related matters.

Odd moments Any odd moment of a Gaussian variable vanishes. Therefore, odd moments are very sensitive to deviations from Gaussianity, so that non-zero odd moments may be especially good indicators of intermittency. The build up of odd moments is a result of both the (kinematic) evolution of a passive field in any random velocity field and the *dynamics* of turbulence itself. In the latter case, non-vanishing odd moments are the most important, dynamically significant manifestations of non-Gaussianity, i.e. they reflect *directly* the dynamic aspects of intermittency. The most prominent odd moments are the third order structure function for longitudinal velocity increments $S_3^{||} = \left\langle \{[\mathbf{u}(\mathbf{x}+\mathbf{r}) - \mathbf{u}(\mathbf{x})] \cdot (\mathbf{r}/r)\}^3 \right\rangle$ entering the 4/5 law, the enstro-

phy production $\langle \omega_i \omega_k s_{ik} \rangle$ and the third order moment of the strain tensor $\langle s_{ij} s_{jk} s_{ki} \rangle$, which are of special importance due to their property of self-amplification. Note that all these and other odd moments vanish in a Gaussian velocity field. This points to the dynamical impotence of Gaussian velocity fields and lack of dynamically relevant structure(s).

Passive objects (scalars like temperature, vectors like gradient of passive scalar **G** and magnetic field **B**) in a random velocity field (real or artificially prescribed) are nonlinear functionals of the velocity field and forcing/excitation. Therefore, even when both the velocity field and forcing are Gaussian the field of a passive object is expected to be strongly non-Gaussian, so that $\langle G_i G_k s_{ik} \rangle$, $\langle B_i B_k s_{ik} \rangle$ do not vanish as is usually (but not always) the case, Majda and Kramer (1999). Such *kinematic* intermittency is observed in a great number of theoretical and some experimental works. The term 'kinematic' is used here in the sense that there is no relation to the dynamics of fluid motion, which does not enter in the problems in question, and the velocity field is prescribed and often assumed to be Gaussian.

We remphasize that the non-Gaussian nature of genuine turbulent flows and of passive objects is qualitatively different, as is intermittency in a great variety of physically different systems.

Scaling exponents and PDFs It is commonly believed that among the manifestations of the small scale intermittency in the commonly defined inertial range[3] is the experimentally observed deviation of the scaling exponents for structure functions $S_p^{\|} = \langle \{[\mathbf{u}(\mathbf{x}+\mathbf{r}) - \mathbf{u}(\mathbf{x})] \cdot (r/r)\}^p \rangle$ for $p > 3$ from the values implied by the Kolmogorov theory, i.e. anomalous scaling. This in turn is due to rare strong events. Namely, $S_p^{\|}(r) \propto r^{\zeta_p^{\|}}$, where $\zeta_p^{\|} = p/3 - \mu_p < p/3$ is a convex nonlinear function of p.

However, there are major problems with scaling as follows.

First, there exists no one-to-one relation between simple statistical manifestations and the underlying structure(s) of turbulence. Moreover, qualitatively different phenomena can and do possess the same set of scaling exponents, so that one needs more subtle statistical characterizations of turbulence structure(s) and intermittency. For example, until recently one of the common beliefs was that the observed vortex filaments/worms are mainly responsible for the phenomenon of intermittency understood as anomalous scaling. However, it appears that this is not the case, see the evidence and references given in Tsinober (1998a, b, 2009). More specifically, the problem with the intermittency in the conventionally defined inertial range (CDIR) and the related anomalous scaling is in the object (CDIR) itself as it is ill defined and does not exist in reality. Hence the problem with theories attempting explanations of intermittency in this nonexistent phenomenon, e.g. breakdown coefficients/multipliers

[3] A last example is the paper by Dowker and Ohkitani (2012), see also references therein in which the anomalous scaling is identified with intermittency as is done in multitude of papers.

One more example is in Seiwert et al. (2008) who studied *the decrease of intermittency in decaying rotating turbulence* via looking at the scaling of the longitudinal velocity structure functions, up to order $q = 8$. This decrease can be explained by suppression of strong dissipative events in the presence of rotation.

(Novikov 1971, 1990), multi-fractals (Frisch 1995) and the so-called 'hierarchical symmetry', She et al. (2017) and references therein.

There are no 'corrections' to the scaling exponent in 4/5 law – it is an exact consequence of NSE. However, as it (i) manifests the non-Gaussian nature of turbulence and (ii) the PDFs of the longitudinal velocity increments especially at small r have flaring tails, i.e. hanging far above the Gaussian PDF, the 4/5 law should be considered as related to intermittency. This shows that 'intermittency corrections' are not that reliable as indicators of intermittency, if at all. Pointing out that recent experiments at high Reynolds numbers showed that the 4/5 law is not a pure inertial relation (demonstrating that the CDIR is ill posed) since the PDFs of the velocity increments contain strong dissipative events with nonnegligible contributions to the structure functions $S_p^{\parallel}(r)$ increasing with the order and among them $S_3^{\parallel}(r)$.

The next example is represented by numerous models attempted to reproduce the anomalous scaling. A partial list of references is given in Sreenivasan and Antonia (1997) and Tsinober (1998b, 2009). These models followed the Kolmogorov (1962) refined similarity hypothesis (RSH) in which the mean dissipation $\langle\epsilon\rangle$ was replaced by 'local' dissipation ϵ_r averaged over a region of size r. The scaling exponents obtained in all of these models are in good agreement with the experimental and numerical evidence, e.g. these models exhibit the same scaling properties (and some other such as PDFs) as in real turbulence. It is noteworthy that many of these models are based on qualitatively different premises/assumptions and with few exceptions have no direct bearing on the Navier-Stokes equations. *Therefore the success of such models can hardly be evaluated on the basis of how well they agree with experiments. Phenomenology and models only will hardly be useful and convincing, since almost any dimensionally correct model, whether right or wrong, will lead to correct scaling without appealing to NSE and/or elaborate physics.* For example, there exist many theories which produce the $k^{-5/3}$ energy spectrum for qualitatively and/or physically different reasons. A recent example is *a suggestion that the spectrum of fully developed turbulence is determined by the equilibrium statistics of the Euler equations and that a full description of turbulence requires only a perturbation, small in some appropriate metric, of a Gibbsian equilibrium*, Chorin (1996). The most common justification for the preoccupation with such models is that they (at least some of them) share the same basic symmetries, conservation laws and some other general properties, etc. as the Navier-Stokes equations. The general belief is that this - along with the diversity of such systems (there are many having nothing to do with fluid dynamics, e.g. granular systems, financial markets, brain activity - is the reason for the above mentioned agreement. However, this is not really the case, e.g. in Kraichnan (1974) a counter example of a *'dynamical equation is exhibited which has the same essential invariances, symmetries, dimensionality and equilibrium statistical ensembles as the Navier-Stokes equations but which has radically different inertial-range behavior'*! The majority of models exhibit temporal chaos only. Therefore, such models (and most others) can hardly be associated with the intermittency of real fluid turbulence, which also involves essentially spatial chaos. Again, for the above reasons the agreement between such models and experiments (both laboratory and numerical) cannot be used for evaluation of the success of such models. There are

proposals to use more than one set of exponents including 'universality' proposals involving 'much more' scaling exponents, see e.g. Biferale and Procaccia (2005), Frisch (1995) and references therein.

Scaling laws alone are not necessarily theories. With all the attractiveness of scaling, turbulence phenomena are infinitely richer than their manifestation in scaling and related matters. Most of these manifestations are beyond the reach of phenomenology. Phenomenology is inherently unable to handle the structure of turbulence in general, and phase and geometrical relations in particular, to say nothing of dynamical features such as build up of *odd* moments, interaction of vorticity and strain resulting in positive net strain and enstrophy production/predominant vortex stretching. It seems that there is little promise for progress in understanding the basic physics of turbulence by dealing only with scaling and related matters, without looking into the structure and, where possible, basic mechanisms which are *specific* to turbulent flows. In fact, the main question of principle which should have been asked long ago is: Why on earth should we perform so many elaborate measurements of various scaling exponents without looking into the possible concomitant physics and/or without asking why and how more precise knowledge of such exponents, even assuming their existence, can aid our understanding of turbulent flows? This is not to say that one must abandon the issues of scaling. An example of affirmative answer is given in the Sect. 7.1 concerning how ill-posed the concept of inertial range is.

Second, as discussed in the previous section the very existence of scaling exponents in a statistical sense (e.g. for various structure functions or corresponding PDFs, etc., which is taken for granted) is a problem by itself!

A similar question arises in respect of multifractality which was designed to 'explain' the 'anomalous' scaling, since there is no direct experimental evidence on the multifractal structure of turbulent flows. So there is a possibility that multifractality in turbulence is an artifact see Frisch (1995, p. 190). Moreover, in reality multifractality in fact is a kind of description of *finite* Reynolds number effect at whatever large Re due to the ill posed concept of the inertial range as mentioned above.

The PDFs of an intermittent variable are quite useful as, e.g. they carry the information showing that extremely small and extremely large values are much more likely than for a Gaussian variable. However, they contain no information on the structure of the underlying weak and strong events, nor on the structure of the background field. Hence, the same PDFs can have qualitatively different underlying structure(s) of the flow, i.e. 'how the flow looks'. Similar PDFs of some quantities can correspond to qualitatively different structure(s) and quantitatively different values of Reynolds number, see references in Tsinober (1998a, b, 2009). For example, the qualitative difference in the behavior and properties of regions dominated by strain and those with large enstrophy cannot be captured by such means and other conventional measures of intermittency. Further, the PDFs, like scaling exponents, do not allow us to infer much about the underlying dynamics. This, however, is true of 'conventional' PDFs like those of velocity increments, but not of any PDFs such as those directly associated with geometrical flow properties.

Note that the largest deviation from Gaussianity occurs at small scales. In this sense, the field of velocity derivatives, $\partial u_i/\partial x_k$, is more intermittent than the field of velocity, u_i, itself. One of the possible reasons for this is in the different nature of nonlinearity at the level of velocity field, i.e. in the Navier-Stokes equations and, for example, in the equation for vorticity. Namely, the nonlinearity in the Navier-Stokes equations, $(\mathbf{u} \cdot \nabla)\mathbf{u} = \omega \times \mathbf{u} + \nabla(u^2/2) = \nabla(\alpha + u^2/2) + \nabla \times \beta$ contains a considerable irrotational part, $\nabla(\alpha + u^2/2)$. This irrotational part can be included in the pressure term, i.e. the solenoidal part of nonlinearity is reduced, see Fig. 8.8 There is no such reduction of nonlinearity on the level of vorticity and strain. Hence the difference, since the nonlinearity can be seen as one of the general reasons for the intermittency of genuine turbulence. This is both too general and not specific as many other qualitatively different systems exhibit intermittency.

Thus we turn to the discussion of more specific possible reasons/origins of intermittency.

9.1.4 On Possible Origins of Small Scale Intermittency

At the present the state of affairs the issue is pretty speculative, and an example of an 'ephemeral' collection of such is given below.

As one of manifestations of turbulence structure(s), intermittency has its origins in the structure of turbulence, see next section. Therefore we briefly address here the possible origins of intermittency. There are roughly two kinds of origins of intermittency: kinematic and dynamic.

Before proceeding we reiterate that non-Gaussianity and intermittency are not synonymous just as the origins of non-Gaussian statistics in various systems and genuine turbulence are even qualitatively generally quite different. Therefore, it is misleading to 'explain' such properties of genuine turbulence by analogy with the non-Gaussian behavior of, e.g. Burgers and/or restricted Euler equations. An important point is that these are integrable equations, and exhibit random behavior only under random forcing and or initial conditions, otherwise their solutions are not random and should be distinguished from problems involving genuine turbulence. Navier-Stokes equations at sufficiently large Reynolds number have the property of intrinsic mechanisms becoming complex without any external aid including strain and vorticity self-amplification. There is no guarantee that the outcome, e.g. such as structure(s) is the same from, e.g. natural "self-randomization" and with random forcing and even with different kinds of forcing. Moreover, there is evidence that the outcome is indeed different.

Direct and bidirectional interaction/coupling between large and small scales. As discussed, direct and bidirectional interaction/coupling between large and small scales is one of the elements of the nonlocality of turbulence. It is both of kinematic and dynamic nature. To repeat *Intermittency is an important feature of the common (!) nonlocal life of large and small scales both dynamically and kinematically.*

The first recognized manifestation of such interaction is that the small scales do not forget the anisotropy of the large ones. There are a variety of mechanisms producing and influencing the large scales: various external constraints like boundaries with different boundary conditions, including the periodic ones, initial conditions, forcing (as in DNS), mean shear/strain, centrifugal forces (rotation), buoyancy, magnetic field, external intermittency in partially turbulent flows, etc. Most of these factors usually act as organizing elements, favoring the formation of coherent structures of different kinds (quasi-two-dimensional, helical, hairpins, etc.). These, as a rule, large scale features depend on the particularities of a given flow that are not universal. Therefore the direct interaction between large and small scales leads to 'contamination' of small scales by the large ones, e.g. the edges of large scale structures are believed to be responsible for such 'contamination' in any kind of flow. This contamination is unavoidable even in homogeneous and isotropic turbulence, since there are many ways to produce such a flow, i.e. many ways to produce the large scales. It is the difference in the mechanisms of large scale production which 'contaminates' the small scales. Hence, non-universality.

The direct and bidirectional interaction/coupling of large and small scales, i.e. nonlocality, is a generic property of all turbulent flows and one of the main reasons for small scale intermittency, non universality, and quite modest manifestations of scaling. This dates back to the famous Landau remark stating that the *important part will be played by the manner of variation of* ε *over times of the order of the periods of large eddies (of size* ℓ*)*, Landau and Lifshits (1944, 1987, p. 140).

"Near" singularities, extreme events and alike It is not known for sure whether Navier-Stokes equations at large Reynolds numbers develop a genuine singularity in finite time, though there no evidence in favor of this, so the term "near" singularities is just another term for a subclass of a wider set of objects under the common name extreme/strong events not necessarily just dissipative. Nevertheless, it seems a reasonable speculation that the 'near' singularities *trigger topological change and large dissipation events; their presence is felt at the dissipation scales and is perhaps the source of small scale intermittency*, Constantin (1996).

In any case, the 'near' singular objects *may* be among the origins of intermittency of dynamical nature.[4] However, there is a problem with two-dimensional 'turbulence'. Namely, in this case everything is beautifully regular, but there is intermittency in the sense of the above definitions, with the exception of scaling exponents for velocity structure functions and corresponding quasi-Gaussian behavior. However, non-Gaussianity is strong at the level of velocity derivatives of a second order. Hence the possible formation of singularities in 3-D is not necessarily the underlying reason for intermittency in 3D turbulence. Another example relates to modified Navier-Stokes equations such as those using hyperviscosity replacing the Laplacian by a higher order operator $(-1)^{h+1}\nabla^{2h}$ with $h > 1$ with the underlying *assumption that this manipulation changes only the small scales, which is unlikely due to nonlocal effects* In this case too everything is beautifully regular too for $h > 5/4$, i.e.

[4] We mean singularities which appear at random in space and time and not in a strictly periodic (and fully coherent and mutually amplifying?) fashion as in DNS with periodic boundary conditions.

the solution remains regular for all times and any Reynolds number, Ladyzhenskaya (1975), Lions (1969), recent paper by Bardos et al. (2010) and Sect. 10.3 Turbulence versus mathematics and vice versa in Tsinober (2009). *Some* features of turbulence are reproduced well (such as $k^{-5/3}$ spectrum) including some features of intermittency, but its structure(s) appear quite different from those for true NSE.

Until recently strong dissipative events (SDEs) have been blamed for deviations from the K41 conventionally defined inertial range (CDIR) similarity scalings especially for higher order statistics. However, it appears (see above) that the concept of CDIR is ill defined due to the nonlocal impact of SDEs living in the conventionally defined dissipative range (CDDR) on the CDIR and that the removal of the SDEs from the statistics results in restoration of the K41 scaling of structure functions up to the eighth order. More generally, depending on their scale, energy etc. extreme events can dominate aspects of both the large and fine scale dynamics of turbulence. Interaction with the environment includes nonlocality and consequently the need to treat the whole system as a complete entity.

The theme is a part of a broader problem in nature and technology, in general, and in a diverse variety of fluid systems, in particular. The concern is the so called extreme events, which are in some sense localized in space and time and are in one sense or another intense. In probabilistic thinking these are defined as associated with the tail of a probability distribution of some quantity considered important or 'rare but influential'. The interesting question on what objects populate the tails of the PDFs alone is on its own insufficient as extreme events are not "single parameter" creatures related to structure(s). This must be balanced with the basic physics of extreme events (where insight is lacking), which include their own mechanisms, their their own interactions and interactions with the environment. The latter are far more intense than in regular situations thereby being of utmost importance for a great variety of applications. Insight into these issues is desperately needed in diverse fields, e.g. from pure/applied mathematics, theoretical physics, classical turbulence and engineering, geophysical and astrophysical turbulence. For references see Müller et al. (2007) and Yeung et al. (2015).

Multiplicative noise, intermittency of passive objects in random media. It has been known for about thirty years that passive objects (scalars, vectors) exhibit 'anomalous scaling' behavior and other strong manifestations of intermittency even in a pure Gaussian random velocity field, see references in Tsinober (2009). These are dynamically linear systems, but they are of the kind which involve the so-called multiplicative 'noise', i.e. the coefficients in the equations that depend on the velocity field. Therefore, statistically they are 'nonlinear', since the field of passive objects is a nonlinear functional of the velocity field. Therefore, passive objects exhibit strong deviations from Gaussianity. In such systems, intermittency results either from external pumping (forcing term on RHS of the equations), or in systems without external forcing from instability (self-excitation) of a passive object in a random velocity field under certain conditions.

The velocity field does not 'know' anything about the passive objects. In this sense, problems involving passive objects are kinematic in respect with the velocity

field in real fluid turbulence. They may reflect the contribution of kinematic nature in real turbulent flows. In view of the recent progress in this field it was claimed that *investigation of the statistics of the passive scalar field advected by random flow is interesting for the insight it offers into the origin of intermittency and anomalous scaling of turbulent fluctuations,* Pumir et al. (1997) for later references see Tsinober (2009). More precisely it offers an insight into the origin of intermittency and the "anomalous scaling" of fluctuations in random media generally and independently of the nature of the random motion, Zel'dovich et al. (1988). It gives some insight into the contributions of a kinematic nature, but does not offer much regarding the specific dynamical aspects of strong turbulence in fluids. Moreover, anomalous diffusion and scaling of passive objects occurs in purely laminar flows in an Eulerian setting (E-laminar flows) as a result of Lagrangian chaos (L-turbulent flows), i.e. intermittency of passive objects may even have nothing to do with the random nature of fluid motion in Eulerian setting, see Sect. 3.4 and the next section.

Therefore in real turbulent flows there are two contributions to the behavior of passive objects, kinematic and dynamic. It seems hopeless to separate them in any sense.

9.1.5 On the Eulerian Versus Lagrangian Intermittency

As mentioned the term "lagrangian intermittency" is used in several related meanings. The first is "intermittency" of Lagrangian fields without regarding their dynamics, e.g. the purely kinematic "intermittency" of a purely Eulerian Gaussian field or other chaotic artificial E-velocity fields in their Lagrangian representation, see Sect. 3.4 above. This extreme kind of purely kinematic Lagrangian "intermittency" is characteristic of the so-called L-turbulent flows which are E-laminar, i.e. laminar in the Euler representation, see Sect. 3.4 above.

Another use of the term "Lagrangian intermittency" is used in the context of the scaling of Lagrangian structure functions of real turbulent flows, Sawford and Yeung (2015) and references therein.

The main aspect here is that whereas the E-turbulence is a dynamic phenomenon this is not necessarily the case with the L-turbulence which may be a purely kinematic one as in Fig. 1.3 for a flow at small Reynolds numbers. In other words the Eulerian setting reveals the pure dynamic chaotic aspects of genuine turbulence as contrasted the "mixing" of the kinematical with the dynamic ones in the Lagrangian setting, i.e. in genuine turbulence the latter contains both which seem to be essentially inseparable. Consequently, studying Lagrangian statistics alone may not provide adequate information on the L-statistics of genuine turbulence as not necessarily containing its pure dynamic "stochasticity". The well known example is the difference between the Eulerian and Lagrangian microscales, etc. Tennekes (1975). The simplest almost purely kinematic aspect is that in Lagrangian representation the advection of small scales by the large ones is almost(!) absent, i.e., a Lagrangian setting in which the sweeping effects are mostly (but not totally! see Chap. 4) removed so that the con-

tribution of, e.g. the strong dissipative events (SDE) in the conventionally defined inertial range (CDIR) is even larger than in the Eulerian setting. Hence, the difference in the properties of intermittency.

Summarizing, intermittency specifically in genuine fluid turbulence is associated mostly with some aspects of its spatiotemporal structure, especially the spatial one. Hence, the close relation between the origin(s) and meaning of intermittency and structure of turbulence. Just as there is no general agreement on the origin and meaning of the former, there is no consensus regarding the origin(s) and meaning of turbulence structure(s). What is definite is that turbulent flows have many structure(s). The term structure(s) is used here deliberately to emphasize the duality (or even multiplicity) of the meaning of the underlying problem. The first is about how turbulence 'looks'. The second implies the existence of some entities. Objective treatment of both requires use of some statistical methods. It is thought that these methods alone may be insufficient to cope with the problem, but no satisfactory solution has so far been found. One (but not the only) reason - as mentioned - is that it is not clear what is looking for: the objects still seem to be elusive. For example, there is still a non-negligible part of the community that are in a great doubt that the concept of "coherent" structure is much different from the Emperor's New Clothes.

9.2 What is(are) Structure(s) of Turbulent Flows? What We See is Real. The Problem is Interpretation

What we see is real. The problem is interpretation as there is even a problem of defining of what is "seeing".

The issue is pretty speculative, and an example of an 'ephemeral' collection of such is given below. We must admit at this stage that structure(s) is(are) just an inherent property of turbulence. Structureless turbulence is meaningless.

The difficulties in definition the structure(s) of turbulence are of the same nature as the question what is turbulence itself. In order to 'see' or 'measure' the structure(s) of turbulence one encounters such difficult questions such as: what is a (say, dynamically relevant) structure? Structure of what? Which quantities possess structure in turbulence? What is the relation between structure(s) and 'scales' - unfortunately both ill defined? Can structure exist in 'structureless' (artificial) pure random Gaussian fields? Which ones? All these questions - as many other issues - are intimately related to the skill/art of asking the right and correctly posed questions. These impossibly difficult questions are not made easier by the terminological use and abuse of turbulencein a variety of ill defined terms (eddies, worms, sheets, tubes, pancakes, ribbons, vortex or vortical structures/filaments, vortons, 'eigensolutions', significant shear layers, etc.). Some would also include in this list those coherent structures frequently used as synonyms of components of some decompositions or similar "executions" of the real flow field. These are usually followed by studies (sometimes pretty sophisticated) of their interaction which don't necessarily reflect any physics,

at least as concerns physical space. One of the popular games of this kind is looking desperately for confirmation of the classical energy cascade picture.

The main starting point here is at the end of the previous section: just as there is no general agreement on the origin and meaning of intermittency, there is no consensus regarding the origin(s) and real meaning of turbulence structure(s). What is definite is that turbulent flows have lots of structure(s). The term structure(s) is used here deliberately to emphasize the duality (or even multiplicity) of the meaning of the underlying problem. The first is about how turbulence 'looks'. The second implies the existence of some entities. This is followed by more serious issues most of which are unfortunately not even properly posed.

The meaning of structure(s) depends largely on what is meant by turbulence itself, and especially the structure(s) of the particular field for which one is looking. For example, the velocity field may have no structure, but the passive tracer may well have a pretty nontrivial one. A simple laminar Eulerian velocity field (E-laminar) creates complicated Lagrangian field (L-turbulent). A purely Gaussian, i.e. 'structureless' velocity field creates structure in the field of passive objects. The structure(s) seen in the velocity field depend on the motion of the observer. Finally, what is called the "coherent structures" or "organized motion", which has been rediscovered many times, may be not directly related to the turbulent nature of the flow (such as mixing layer), but are rather a result of the large scale instability of the flow as 'whole' (zooming out).

9.2.1 On the Origins of Structure(s) of/in Turbulence

This question - in some sense - is a 'philosophical' one. But its importance is in direct relation to even more important questions about the origin of turbulence itself. Hence once again an 'ephemeral' collection of such possible reasons/causes of structure(s) in turbulence flows keeping in mind that structure(s) is (are) just an inherent property of turbulence. There is no turbulence without structure(s).

Instability As mentioned in Chap. 2, the most commonly accepted view on the origin of turbulence is flow instability. An additional factor is that instability is considered as one of the origins of structure(s) in/of turbulence. However, this latter view requires the assumption that turbulence has non-negligible 'memory' of or, alternatively, that the 'purely' turbulent flow regime, i.e. at large enough Reynolds numbers, has instability mechanisms similar to those existing in the process of transition from laminar to turbulent flow state. The problem is that referring to (in)stability requires one to define the state of flow (in)stability under consideration, which is not a simple matter in the case of a turbulent flow.

Note the observation made by Goldshtik and Shtern (1981): *The fact that the phenomenon of intermittency and structures are observed in the proximity of the outer boundary of turbulent flow or in close to the wall and in the small scale "tail" of turbulent flow flows, i.e when the characteristical Reynolds numbers are relatively*

not large, prompts an assumption that "structureness" is associated with mechanisms of turbulence origins. This may be an underlying reason of some similarity between some flow patterns ("structures") in transitional and developed turbulent flows. This idea appeared in a number of subsequent publications, e.g. Blackwelder (1983), Pullin et al. (2013) and references therein. It should be stressed that even if the above hypothesis is true there in no escape from nonlocal effects!

Emergence Another less known view holds that structure(s) emerge in large Reynolds number turbulence out of 'purely random structureless' background, e.g. via the so-called inverse cascades or negative eddy viscosity. Among the spectacular examples, are the 'geophysical vortices' in the atmosphere, and ocean, as well as astrophysical objects. Another example is the emergence of coherent entities, such as vortex filaments/worms and other structure(s), out of an initially random Gaussian velocity field via the NSE dynamics, for examples see references in Tsinober (2009).

Anderson (1972) emphasizes *the concept of 'broken symmetry', the ability of a large collection of simple objects to abandon its own symmetry as well as the symmetries of the forces governing it and to exhibit the 'emergent property' of a new symmetry*. One of the difficulties in turbulence research is that no objects simple enough have yet been found so far such that a collection of these objects would adequately represent turbulent flows. It is not clear how meaningful is the very question on the existence of such objects.

It 'just exists', or do flows become or are they are 'just' possessing structure?

> *To the flows observed in the long run after the influence of the initial conditions has died down there correspond certain solutions of the Navier- Stokes equations. These solutions constitute a certain manifold $\mathcal{M} = \mathcal{M}(\mu)$ (or $\mathcal{M} = \mathcal{M}(Re)$) in phase space invariant under phase flow* (Hopf (1948)).
>
> *Kolmogorov's scenario was based on the complexity of the dynamics along the attractor rather than its stability.* Arnold (1991), see also Keefe (1990a), Keefe et al. (1992)

This view is a reflection of one of the modern beliefs that the structure(s) of turbulence - as we observe in *physical space* - is (are) the manifestation of the generic structural properties of mathematical objects in *phase space*, these are called attractors and are invariant in some sense. In other words, the structure(s) are assumed to be 'built in' the turbulence independently of origin, hence the possible tendency to universality.[5] It is noteworthy that the assumed existence of an attractor makes sense for statistically stationary turbulent flows. However, for flows which are not such, e.g. decaying turbulent flows past a grid or a DNS simulated flow in box the attractor is trivial. Nevertheless, these flows possess many properties which are essentially the same as their statistically stationary counterparts provided that their Reynolds numbers are not too small ($Re_\lambda \geq 10^2$).

The above refers to the dynamical aspects of real turbulent flows. We mention again here also the

[5] In the strange attractor theory, the experimental measurements are viewed as projections of these attractors onto low dimension that correspond to these measurements.

Emergence of structures in passive objects in random media

In such flows the velocity field and the external forcing are prescribed. Whatever their nature - even Gaussian - structure is emerging in the field of passive objects, Zel'dovich et al. (1988), Ott (1999) and references therein.

9.2.2 How Does the Structure of Turbulence 'Look'? Is This Question Meaningful?

For a long time the first and only impression/answer to this question was obtained by employing visualization techniques. First in experiments using mainly passive markers and later using the DNS simulations looking mainly at objects bounded by isosurfaces of some quantity, such as enstrophy, ω^2.

The first important result was that even turbulence which is 'homogeneous' and 'isotropic' has structure(s), i.e. contains a variety of strongly localized events. The primary evidence is related to spatial localization of subregions with large enstrophy, i.e. intense vorticity, which are organized in long, thin tubes-filaments-worms. Some evidence was obtained about regions with large strain, $s_{ij}s_{ij}$, i.e. dissipation, being sheet-like objects with very sharp edges (razors/flakes), see references in Tsinober (2009) for both.

The relatively simple appearance of the observed 'structures' is due ta such simple techniques as looking at isosurfaces of some quantity, e.g. enstrophy ω^2, with thresholding using conditional sampling techniques. This is how the first evidence of concentrated vorticity/filaments/worms was obtained.

This prompted a rather popular view that turbulence structure(s) is (are) simple in some sense and that essential aspects of turbulence structure and dynamics may be adequately represented by a random distribution of simple (weakly interacting) objects.

In particular, it is still commonly believed that most of the structure of turbulence is associated with and is due to various strongly localized intense events/structures, e.g. mostly regions of concentrated vorticity so that *turbulent flow is dominated by vortex tubes of small cross-section and bounded eccentricity*, Chorin (1994, p. 95), for other quotes and references see Tsinober (1998a, 2009). Further, that these events are mainly responsible for the phenomenon of intermittency. It is demonstrated in Tsinober (1998a, 2009) that such views are inadequate. It appears that - though important - these structures are not the most dynamically important ones and are the consequence of the dynamics of turbulence rather than its dominating factor. Namely, regions other than those involving concentrated vorticity such as: i – 'structureless' background, ii – regions of strong vorticity/strain (self) interaction and largest enstrophy and strain production dominated by large strain rather than large enstrophy, and iii – regions with negative enstrophy production are all dynamically significant and in some important respects more significant than those with concentrated vorticity, strongly non-Gaussian, and possess structure. Due to the strong nonlocality of

turbulence in physical space all the regions are in continuous interaction and are strongly coupled. A similar statement can be made regarding the so called streamwise vortices observed in many turbulent flow. Moreover, as described in Chap. 7 the anomalous scaling is due to strong dissipative events, i.e. large strain s^2, so that the conventionally defined inertial range is an ill defined concept and falling it out of use.

To emphasize, the above conclusions are the outcome of the use of quantitative manifestations of turbulence structure, which just like intermittency are in the first place of statistical nature independently of how specifically the individual structures look and whether they exist at all.

Though the isosurfacing/thresholding approach is useful and 'easy', it is inherently limited and reflects at best the simplest aspects of the problem, for a recent example see Cardesa et al. (2017) and references therein. Even for characterization of *some* aspects of the *local* (i.e. in a sense 'point'-wise) structure of the flow field in the frame following a fluid particle requires *at least two* parameters.[6] Therefore attempts to *adequately* characterize finite scale structure(s) by only one parameter are unlikely to be successful.

In a challenging paper by Yeung et al. (2015) a study of the small-scale properties of dissipation s^2 and ensrophy ω^2 was made using thresholding with the conclusion that *extreme magnitudes of energy dissipation rate and enstrophy occur simultaneously in space and remain nearly colocated during their evolution*. This is correct, but as any thresholding is limited as concerns the physical properties of the events in question. For example, looking at the properties of their self-amplification $s_{ij}s_{jk}s_{ki}$ and $\omega_i\omega_j s_{ij}$ one expects that their extreme magnitudes will not occur simultaneously in space and time. The hint for this is seen from the joint PDF of these quantities in top-right Fig. 7.5. A more general aspect is that the s^2 and the ω^2 threshold regions are not simple, they are not a 'single parameter creatures' and themselves possess structure associated, e.g. with $s_{ij}s_{jk}s_{ki}$ and $\omega_i\omega_j s_{ij}$, geometrical properties as alignments, etc.

Another example, is by Cardesa et al. (2017), Jimenez et al. (2017), Jiménez (2018)—employing kinetic energy as a parameter defining finite object(s). The main reason that these finite objects are not "single parameter creatures" is nonlocality as manifested by the strong coupling (not "cascade") between large and small scales (e.g. kinetic energy and dissipation), see Sect. 7.3 and Fig. 7.6, and also Sect. 7.4.

The one parameter approaches are not made any better (but rather more misleading) by adding to thresholding and isosurfacing some decomposition. One can study

[6]These are $Q = 1/4(\omega^2 - 2s^2)$ and $R = -1/3(s_{ij}s_{jk}s_{ki} + 3/4\omega_i\omega_j s_{ij})$. Here Q is the second — and R is the third invariant of the velocity gradient tensor $\partial u_i/\partial x_j$. The first invariant is vanishing due to incompressibility.

A similar problem arises when attempting to characterize structure(s) of turbulent flows using two-point information but based only on a single velocity component, e.g. longitudinal structure functions $S_n^{||}(r)$, since such an approach does not 'know' (almost) anything about the two other velocity components.

Similarly, there are many attempts to handle 'structures' even with just steamwise velocity component.

some geometrical issues of the isosurfaces in the filtered fields and even the "interaction" between such "structures" belonging to the fields corresponding to different filter bands with the remaining acute problem is how all this is related to the whole flow field in the real physical space. More generally, the problem is related to pattern recognition and requires defining a conditional sampling scheme involving more than two parameters. This scheme is in turn based on what a particular investigator thinks are the most important physical processes, features, etc. This in turn opens a Pandora box of possibilities and contains an inherent element of subjectivity and arbitrariness, since the physics of turbulence is not well understood. In this sense, the circle is closed: in order to objectively define and educe some structure, one needs clear understanding of the physics of turbulence, which, it is in turn believed, can be achieved via study of turbulence structure(s).

There are other serious problems in the observations of individual structures obtained via isosurfacing or thresholding and the like.

First, the "boundaries" of flow regions isolated by such methods cannot in any sense be qualified as "natural" and serve only as a technical means. One cannot take such an approach for granted as being reliable for getting the "natural" boundaries of these structures and serving simultaneously as definitions of those "structures". Both can be qualified as wishful thinking at best. On the other hand, there exist a number of attempts to define what is, e.g. a coherent structure, a vortex, etc., see references in Malm et al. (2012) and also Monin (1991), Townsend (1987), Bonnet (1996) and Holmes et al. (1966).

Second, these structures are just single time snapshots in space having no identity beyond the particular time moments of their (infinitesimal) life, so that one cannot observe their evolution over time. The latter difficulty is nontrivial, because one ventures to deal with *finite* objects. Namely, even having defined such a finite relatively simple object at some time moment one looses it shortly threafter even if there is a possibility to follow this object as in case of purely Lagrangian objects. This is illustrated in Fig. 9.2. It is for these reasons people produce statistics out of collections of "similar objects" obtained from snapshots at the same and different time moments via isosurfacing and thresholding and other tricks to justify these "surgeries", etc. But the painful question is how really "similar" are all these if they are typically defined by one parameter only? It is almost obvious that such "statistical" processing is killing most of the essential features of the real "structure" and leaves the question of relevance, say the dynamical one, of these "structures" at best open.

The above points to acute problems in defining instantaneous structures not to mention studying them. Even though it is commonly assumed that there exist instantaneous "structures" which are in some sense "key" objects (from some point of view) and that these unknown objects even govern the dynamics of the flow. Claims of this kind are pretty frequent, but without much - if any - explanation/justification whatever.

As concerns the individual structure it was already mentioned that the regions of concentrated vorticity are of limited dynamical relevance. First, these regions are characterized by approximate balance between enstrophy production $\omega_i \omega_k s_{ik}$ and its viscous destruction in a way similar to that of the Tennekes and Lumley balance,

Fig. 9.2 Evolution of a tetrahedron with edge of $\approx 4\eta$ at $t = 0$ using the data base of Johns Hopkins University, Li et al. (2008), for a 1024^4 space-time history of a direct numerical simulation of isotropic turbulent flow in incompressible fluid in 3D. Courtesy Beat Luethi. It is seen that a simple Lagrangian tetrahedron, i.e. consisting of fluid particles, becomes not so simple after just one Kolmogorov time scale $\tau_\eta$and turns into a non-trivial object in the time of a few Kolmogorov time scales

see Tsinober (2009). Second, they belong to the category of flow patterns with predominant alignment of vorticity and the strain eigenvector λ_2 corresponding to the intermediate strain eigenvalue, Λ_2. However, the major contribution to the enstrophy production comes from the regions with the ω, λ_1 alignment (corresponding to the largest strain eigenvalue, Λ_1) and in which there is no approximate balance between enstrophy production $\omega_i\omega_k s_{ik}$ and its viscous destruction with strong dominance of $\omega_i\omega_k s_{ik}$. Moreover, the regions with the ω, λ_1 alignment comprise a large part of those where the vorticity/strain interaction is strongest, see Chap. 7 above and Chap. 6 in Tsinober (2009).

Recently there is some trend toward reviving and ascribing some excessive importance to thin shear layers, Hunt et al. (2014, 2015) and references therein. However, they are not more relevant than the "worms" as they belong to the same category with predominant alignment of vorticity and the strain eigenvector corresponding to Λ_2, the intermediate strain eigenvalue Λ_2,[7] whereas the most dynamically active are flow patterns with predominant ω, λ_1 alignment, e.g. as concerns enstrophy produc-

[7]Indeed, e.g. Ishihara et al. (2011) state on observations of *thin shear layers consisting of a cluster of* ***strong vortex tubes*** *with typical diameter of order 10η, where η is the Kolmogorov length scale.*

tion and other essential nonlinear processes, see Chap. 7 above, Tsinober (2009) and references therein.

A final note is that although the patterns with predominant alignment of vorticity and the strain eigenvector corresponding to the intermediate strain eigenvalue, Λ_2 (worms, shear layers and more involved patterns) are statistically dominant they are not the most dynamically relevant. In other words, statistical dominance is not synonymous to dynamical relevance. The qualification of "shear layer" (as several previous "key" objects, e.g. worms) belongs to the category of oversimplified concepts and vague terminology. More preferable are well defined strain and vorticity. The oversimplification (thin layers!) is seen clearly from the above as neglecting important issues of geometrical nature among others, e.g. the aspects of alignments of vorticity and the eigenframe of the rate of strain tensor.

9.2.3 Structure Versus Statistics

The 'not objective enough' nature of a variety of conditional sampling procedures resulted in a whole 'zoo' of 'structures' in different turbulent flows, which some believe to be significant in some sense, but many do not. The zoo seems to have a tendency to have grown at least exponentially with the introduction of multiscale approaches. One cannot help referring to the question by Kadanoff (1986) when the mulifractal "formalism" was just born: *Where is the physics?* Among the reasons for such scepticism is some evidence that attempts at an adequate representation of such a complicated phenomenon as turbulence as only a collection of simple objects/structures are unlikely to succeed. As mentioned, until recently it was believed that concentrated vorticity/filaments is the dominating structure in turbulent flows in the sense that most of the structure of turbulence is associated with and is due to regions of concentrated vorticity. It appears that - although important - these structures are not the most dynamically important ones and are the consequence of the dynamics of turbulence rather than being its dominating factor. A similar statement is true in respect of the recently revived shear layer.

Nevertheless, some 'objectiveness' can be achieved using quantities appearing in the NSE and/or the equations which are exact consequences of NSE.

The question about what the structure(s) of turbulence mean(s) can be answered via a statement of impotence: speaking about 'structure(s)' in turbulence the implication is that there exists something 'structureless', e.g. Gaussian random field as a representative of full/complete disorder. A Gaussian field is appropriate/natural to represent the absence of structure in the statistical sense. Hence all non-Gaussian manifestations of turbulent flows can be seen as some statistical signature of turbulence structure(s). This does not imply that an exactly Gaussian field does not necessarily possess any spatial or temporal structures, see, e.g. fig. 3 in She et al. (1990) – any individual realization of a Gaussian field does have structures. However, an exactly Gaussian field does not possess dynamically relevant structure(s), it is dynamically impotent.

So the next most difficult question concerns the relevance/significance of some particular aspect of non-Gaussianity for a specific problem. It seems that here one enters the *subjective* realm: the criteria of significance (which is a physics! matter) are decided by the researchers. However, the following examples show that the objective choice of the structure sensitive statistics is dictated by the general dynamical aspects of the problem.

For instance, the build up of *odd* moments is an important *specific* manifestation of the structure of turbulence along with being the manifestation of its nonlinearity. The two most important examples are the third order velocity structure function $S_3(r) = \langle\{[\mathbf{u}(\mathbf{x}+\mathbf{r}) - \mathbf{u}(\mathbf{x})] \cdot \mathbf{r}/r\}^3\rangle$ and the mean enstrophy $\langle\omega_i\omega_k s_{ik}\rangle$ and strain $-\left\langle s_{ij}s_{jk}s_{ki}\right\rangle$. The first one is associated with the $-4/5$ Kolmogorov law $S_3(r) = -4/5\langle\epsilon\rangle r$ (Kolmogorov 1941b), which is the first strong indication of the presence of structure in the inertial range showing that both non-Gaussianity and the structure of turbulence are directly related to it's dissipative nature. It is remarkable that the title of this paper by Kolmogorov is *Dissipation of energy in the locally isotropic turbulence.* The $-4/5$ Kolmogorov law clearly overrules the claims that '*Kolmogorov's work on the fine-scale properties ignores any structure which may be present in the flow*', Frisch (1995, p. 182) and that it is associated with near-Gaussian statistics, see references in Tsinober (2009) among many others. Concerning the near Gaussian statsitics it is correct that single point statistics is known to be quite close to the Gaussian one. However, the conclusion that velocity fluctuations are really almost Gaussian is a misconception, not to mention the field of velocity derivatives. This is already seen when one looks at two-point velocity statistics. For instance, in such a case the odd moments are significantly different from zero, e.g. Frenkiel et al. (1979).

The essentially positive value of the mean enstrophy production $\langle\omega_i\omega_k s_{ik}\rangle$ discovered by Taylor (1938a) is the first indication of the presence of structure in the small scales, where turbulence is particularly strongly non-Gaussian and intermittent. The above two examples show that both the essential turbulence dynamics and its structure are associated with those aspects of it's non-Gaussianity exhibited in the build up of odd moments, which among other things means phase and geometrical coherency, i.e. structure. Hence, the importance of odd moments as indicators of intermittency. It is to be noted that the non-Gaussianity found experimentally both in large and small scales is exhibited not only in the nonzero odd moments, but also in strong deviations of even moments from their Gaussian values. Therefore both the large and small scales differ essentially from Gaussian indicating that both possess structure.

However, an important point is that probability criteria are insufficient, since *one can find in statistical data irrelevant structures with high probability,* Lumley (1981). In other words the structure(s) should be relevant/significant in some sense. For example, it should be *dynamically* relevant for velocity field, and related quantities such as vorticity and strain. This does not mean that the kinematical aspects of turbulence structure(s) are of no importance. For example, *anisotropy* is a typical *kinematic statistical* characteristic of turbulent flows of utmost dynamical significance/impact which can hardly be applied to *individual* structures, e.g. a turbulent flow consisting mostly of 'anisotropic' individual structures can be statistically isotropic. Among

the first statistical treatments of turbulence structure is, of course, the first paper by Kolmogorov (1991a), the very title of which is *The local structure of turbulence in incompressible viscous fluid for very large Reynolds numbers.*

The advantage of such an approach is that it allows one to get insights into the *structure* of turbulence without the necessity of knowing much (if anything) about the actual appearance of it's *structures*, since any such question of this kind may well be just meaningless. This is especially important in view of numerous problems/ambiguities in definitions of *individual* structures in turbulent flows, their identification and statistical characterization as well as their incorporation in 'theories'. The main reason is that there exist intrinsic problems both of defining what the *relevant* structures are, see Bonnet (1996) for references and a review of existing techniques (which all are based on statistics anyhow, e.g. of defining extracting/educing) and characterizing the so-called coherent structures. For a number of reasons, it is very difficult, if not impossible, to quantify the information on the *instantaneous* structures of turbulent flows into dynamically relevant/significant form. The observed individual structures strongly depend on the method of observation/extraction, but more importantly none of them are simple, neither are they weakly interacting between themselves or with the background which in fact is not structureless as many assumed. Indeed, *you can find structures, essentially arbitrary, which have equal probability to the ones we have latched onto over the years: bursts, streaks, etc....If structures are defined as those objects which can be extracted by conditional sampling criteria, then they are everywhere one looks in turbulence*, Keefe (1990b). For instance, looking at a snapshot of the enstrophy levels of a purely Gaussian velocity field in She et al. (1990) one can see a number of filaments - the dynamically irrelevant ones - like those observed in real turbulent flows, i.e. pure Gaussian velocity field has some structure(s) too.

9.2.4 What Kinds of Statistics and Which Kinds of Quantities are Most Appropriate to Characterize at Least Some Aspects for Turbulence Structure

Returning to isosurfacing and thresholding it should be mentioned that these allow the handle regions of (rather than "structures") with some properties of interest, for latest example see Malm et al. (2012). These authors used a measure of vorticity stretching related to the magnitude of the vortex stretching vector $W_i \equiv \omega_i s_{ij}$, which emphasizes that the main criteria should be of dynamical relevance, and sensitiveness to the non-Gaussian properties of turbulence, so that one can speak about statistics weakly sensitive to structure and structure sensitive statistics.

Examples of statistics weakly sensitive to structure(s) The first examples of this kind are energy spectra in which the phase (and geometric) information is lost. Hence their weak sensitivity to the structure of turbulence. This insensitivity, in particular, is exhibited in the scaling exponents when/if such exist. For example, the famous

$-5/3$ exponent can be obtained for a great variety of *qualitatively* different real systems - not necessarily fluid dynamical - and theoretical models, for a partial list of references see Tsinober (2009). One can also construct a set of purely Gaussian velocity fields, i.e. lacking any dynamically relevant structure(s), with any desired length of the $-5/3$ 'inertial' range, Elliott and Majda (1995). An extreme example is a single sharp change in velocity represented in Fourier space has an energy spectrum $\propto k^{-6/3}$ which is not so easy to distinguish from $k^{-5/3}$! Vice versa the spectral slope can change, but the structure remains essentially the same '*yet retaining all the phase information*', Armi and Flament (1987). Moreover, not only '*the spectral slope alone is inadequate to differentiate between theories*', *alone* it does not correspond to any particular structure(s) in turbulence or their absence : *there is no one-to-one relation between scaling exponents and structure(s) of turbulence*. This is true not only of exponents related to Fourier decomposition with its ambiguity, Tennekes (1976), but of many other scaling exponents including those obtained in some wavelet space, SO(3) decomposition and in the physical space – a much overstressed aspect of turbulent flows. Likewise, similar PDFs of *some* quantities can correspond to qualitatively different structure(s) and quantitatively different values of Reynolds number. The emphasis is on *some* quantities like pressure or some other usually (but not necessarily) even order quantities in velocities or their derivatives, since the PDFs of *other* appropriately chosen quantities are sensitive to structure (see below).

As mentioned turbulence possesses a number of quasi-gaussian manifestations. The corresponding statistics belongs to those that the weakly sensitive to structure(s).

Structure sensitive statistics It is noteworthy that – as shown by Hill (1997) – the 4/5 Kolmogorov law is more sensitive to flow anisotropy, i.e. the third-order statistics (again odd moments), than the second-order statistics. Similarly, the structure functions of higher odd orders $S_p^{\|}(r) = \langle(\Delta u_{\|})^p\rangle$ are essentially different from zero, see references in Betchov (1976), Sreenivasan and Antonia (1997), Tsinober (1998b, 2009)

Odd moments and related PDFs. This is an example of how structure sensitive statistics can help in looking for the right reasons of measured spectra in the lower mesoscale range, Lindborg (1999). The procedure involves using the third order structure functions which are generally positive in the two dimensional case contrary to the three-dimensional case. Calculations were based on wind data from airplane flights, reported in the MOZAIC data set. It is argued that the k^{-3}-range is due to two-dimensional turbulence and can be interpreted as an enstrophy inertial range, while the $k^{-5/3}$-range is probably not due to two-dimensional turbulence and should not be interpreted as a two-dimensional energy inertial range. There is a competing hypothesis that the large scale $-5/3$ range is the spectrum of weakly non-linear internal gravity waves with a forward energy cascade, Zandt (1982). A third claim is that the spectral slope in the enstrophy range is more shallow than -3 and is close to $-7/3$, Tsinober (1995). This range and related anomalous diffusion is explained in terms of the phenomenon of spontaneous breaking of statistical isotropy (rotational and/or reflectional) symmetry - locally and/or globally.

Another example is the demonstration that the small scale structure of a homogeneous turbulent shear flow is essentially *anisotropic* at Reynolds number up to $Re_\lambda \approx 1000$, Shen and Warhaft (2000), see also Ferchichi and Tavoularis (2000). In order to detect this anisotropy the authors measured the velocity structure functions of third and higher odd orders of both longitudinal and transverse velocity components and corresponding moments of velocity derivatives. In particular, they found a skewness of order 1 of the derivative of the longitudinal velocity in the direction of the mean gradient, which should be very small (or ideally vanish) for a locally isotropic flow. Similar results were obtained in DNS, see references in Tsinober (2009). We should recall that analogous 'misbehavior' of large Reynolds-number turbulence regarding the skewness of temperature fluctuations in the atmospheric boundary layer has been known since the late sixties, Stewart (1969), Gibson et al. (1970), Gibson (1977).

Odd moments such as strain and enstrophy production are obviously of primary importance.

Geometrical statistics This example shows how conditional sampling based on *geometrical* statistics can help to get insight into the nature of various regions of turbulent flow, e.g. those associated with strong/weak vorticity, strain, various alignments, and other aspects of dynamical importance. The first general aspect is the qualitative difference in the behavior and properties of regions with large enstrophy from strain dominated regions, which is also one of the manifestations of intermittency. Various alignments comprise important simple geometrical characteristics and manifestation of the dynamics and structure of turbulence. For example, there is a distinct qualitative difference between the PDFs of $cos(\omega, \lambda_i)$ for a real turbulent flow and a random Gaussian velocity field. In the last case, all these PDFs are precisely flat. An example of special dynamical importance is the strict alignment between vorticity, ω, and the vortex stretching vector $W_i \equiv \omega_j s_{ij}$, since the enstrophy production is just their scalar product, $\omega_i \omega_j s_{ij} = \omega \cdot \mathbf{W}$. In real turbulent flows, the PDF of $\cos(\omega, \mathbf{W})$ is strongly positively asymmetric whereas it is symmetric for a random Gaussian field. It remains essentially positively skewed for any part of the turbulent field, e.g. in the 'weak background' involving whatever definition based on enstrophy, strain, any other relevant quantity or combination thereof. Thus, contrary to common beliefs, the so called 'background' is not structureless, dynamically not inactive and essentially non-Gaussian, just as the whole flow field or any part of it. The structure of the apparently random 'background' seems to be rather complicated. The previous qualitative observations (mostly from DNS) about the '*little apparent structure in the low intensity component*' or the '*bulk of the volume*' with '*no particular visible structure*' should be interpreted as meaning that no *simple visible* structure has yet been observed in the bulk of the volume in the flow. It is a reflection of our inability to 'see' more intricate aspects of turbulence structure: intricacy and 'randomness' are not synonyms for absence of structure.

Pressure hessian Some quantities like pressure or other usually (but not necessarily) even order quantities in velocity or their derivatives are less sensitive to structure. The example below present an opposite case.

Of special interest is the pressure hessian $\Pi_{ij} \equiv \frac{\partial^2 p}{\partial x_i \partial x_j}$. Among the general reasons for such an interest is that the pressure hessian is intimately related to the nonlocality of turbulence in physical space, see references in Tsinober (1998a, b), Tsinober (2009).

One of the quantities in the present context directly associated with the pressure hessian is the scalar invariant quantity $\omega_i \omega_j \Pi_{ij}$. It is responsible for the nonlocal effects in the rate of change of enstrophy production $\omega_i \omega_k s_{ik}$. What is special about this quantity, which is of even order in velocity, is that for a Gaussian velocity field $\left\langle \omega_i \omega_k \Pi_{ij} \right\rangle_G \equiv 0$, whereas in a real flow it is essentially positive and $\left\langle \omega_i \omega_k \Pi_{ij} \right\rangle \sim \frac{1}{3} \left\langle W^2 \right\rangle$, where $W_i \equiv \omega_k s_{ik}$ is the vortex stretching vector. Thus interaction between the pressure hessian and the vorticity is one of the essential features of turbulence structure associated with its nonlocality. It is noteworthy that a similar useful quantity involving strain is non-vanishing for a Gaussian velocity field, $\langle s_{ik} s_{kj} \Pi_{ij} \rangle_G = -\frac{1}{20} \langle \omega^2 \rangle_G^2$,

On passive objects and Lagrangian coherent structures Above we discussed the dynamical aspects of the problem. The issues of structure(s) in various 'kinematic' issues, like the transport of passive objects (scalars, vectors, passive particles including just the fluid particles, etc.), in which Gaussian or other prescribed velocity fields are used rather successfully, can be treated in a similar way as the one described in this section.

As mentioned in a Lagrangian setting the dissipative effects are more "influencing" due to strong removal of sweeping effects. Hence stronger deviations from Gaussian statistics in the Lagrangian setting as compared to the Eulerian one just because "inbetween" there is a relation turning even a pure Gaussian velocity field in the Eulerian setting into strongly non-Gaussian one. Hence the so called Lagrangian coherent structures (LCS's) even in pure laminar in the Eulerian setting and pure Gaussian Eulerian velocity field. In this sense the LCSs are purely kinematic objects just as the structures in the passive objects evolving in a purely Gaussian velocity field due to the non-linear and non-integrable relation between the Eulerian and Lagrangian fields, for more see above Sect. 3.4, Tsinober (2009, p. 300) and Pouransari et al. (2010)

9.2.5 Structure(s) Versus Scales and Decompositions

It is natural to ask how meaningful it is to speak of different scales in the context of 'structure(s)' and in what sense, especially when looking at the 'instantaneous' structure(s) in or of turbulence. The known structures indeed possess quite different scales. Vortex filaments/worms – have at least two essentially different scales, their length can be of the order of the integral scale, whereas their cross-section is of the order of Kolmogorov scale. Similarly, the ramp-cliff fronts in the passive scalar fields have a thickness much smaller than the two other scales. This fact is consistent with the observation by Batchelor and Townsend (1949), that *the mean separation of the*

visible activated regions is comparable with the integral scale of the turbulence, i.e. with the size of the energy-containing eddies.

It is believed that appropriately chosen decompositions may represent structure(s) of turbulence, e.g. Holmes et al. (1966, 1997). Here again several notes are in order. First, this position depends strongly on what is meant by structure(s). Second, such a possibility is realistic when the flow is dominated by (usually large scale) structures, when many, or practically any reasonable decompositions will do. And third, structure(s) (and related issues such as geometry) emerging in the 'simplest' case of turbulent flows, in a box with periodic boundary conditions, is(are) are inaccessible via Fourier decomposition, the most natural one in this case.

One of the popular 'decompositions' is into 'coherent structures' and random/dissipative 'background'.[8] This latter is generally considered as structureless and as a kind of passive sink of energy. Neither is true: the background is not passive at all, it is strongly coupled with the 'coherent structures', and possesses its 'own' structure(s).

There is no turbulence without structure(s). Every part (just as the whole) of the turbulent field – including the so-called 'structureless background' – possess structure. Structureless turbulence (or any of its part) contradicts both the experimental evidence and the Navier-Stokes equations. The qualitative observations on the little apparent structure in the low intensity component or the bulk of the volume with no particular visible structure should be interpreted as indicating that no simple visible structure has yet been observed in the bulk of the volume in the flow. It is a reflection of our inability to 'see' the more intricate aspects of turbulence structure: intricacy and 'randomness' are not synonyms for absence of structure.

Another kind of decomposition is represented by a latest example attempting to take into account the undeniable structure of the above mentioned "structureless background" by dividing the flow in two characteristic regions: the mentioned above "thin shear layers" occupying a small part of the volume of and the quasi-homogeneous rest, Hunt et al. (2014); Hunt JCR et al. (2015) and references therein. The assumption of "thin" is necessary in order to employ a kind of RDT approach. Another recent example is by Leung et al. (2012). Apart from the problematic nature of such decompositions from fundamental point of view, e.g. the claims that the components of some decomposition represent physically meaningful "structures", there are many problems of conceptual and technical nature with what is called 'coherent structures', "thin shear layers", vortex structures, filaments etc. These start from the very beginning of their definition (in fact non-existent or at best vague) and endi with their role in fluid flows both in Eulerian and Lagrangian setting. It is for this

[8] An example of a typical statement is represented by the following: *The emergence of collective modes in the form of coherent structures in turbulence amidst the randomness is an intriguing feature, somewhat reminiscent of the mix between the regular "islands" and the "chaotic sea" observed in chaotic, low-dimensional dynamical systems. The coherent structures themselves approximately form a deterministic, low-dimensional dynamical system. However, it seems impossible to eliminate all but finite number degrees of freedom in a turbulent flow–the modes not included form an essential, dissipative background, often referred as an eddy viscosity, that must be included in the description.,* NewtonandAref (2003b).

reason that *At this stage, this alternative approach (i.e. the 'structural') has not led to a generally applicable quantitative model, neither – for better or worse – has it a major impact on the statistical approaches. Consequently the deterministic viewpoint is neither emphasized nor systematically presented* (Pope 2000). This does not mean that there exists "*generally applicable quantitative model"* based on statistical approaches. It seems that so far Liepmann was correct (but a bit over-optimistic) in his prediction: *Clearly, the exploration of the concept of coherent structure is still on the rise. Turbulence is and will remain the most difficult problem of fluid mechanics, and the past experience suggests that the subsequent fall of interest in the coherent structures is more than likely. The resulting net gain in understanding of turbulence may be less than our expectations of today but will certainly be positive* (Liepmann 1979). Unfortunately, the *resulting net gain so far in the understanding of turbulence* is far less than was expected in 1979. Nevertheless, although essentially there is no acceptable definition of "coherent structure" the boldest part of the community wonders about "quantification" and even "the dynamical equations for coherent structures to predict their evolution", see e.g. Holmes et al. (1997). Apart from sensible definition for this finite object one also needs a definition of the "incoherent components" not to mention the tools to handle their interaction. On top of this there is the not just technical question on what exactly constitutes a coherent structure. It is also the right place to point out that the objects termed "coherent structures" as well as other terms, still elude definition and may appear to be not much different from the Emperor's New Clothes..

The reason for the above statement is as follows.

In dynamical systems, one looks for structure in the *phase space,* Shlesinger (2000), Zaslavsky (1999), since it is relatively 'easy' due to the low dimensional nature of the problems involved. In turbulence nothing is known about its properties in the corresponding very high dimensional phase space.[9] Therefore, it is common to look for structure in the *physical space* with the hope that the structure(s) of turbulence - as we observe it in *physical space* - is (are) the manifestation of the generic structural properties of mathematical objects *in phase space*, which are called attractors and which are invariant in some sense. In other words, the structure(s) is (are) assumed to be 'built in' in the turbulence independently of its (their) origin. The problem is that due to very high dimension and complex behavior of turbulent flows and structure of the underlying attractors *one may never be able to realistically determine the fine-scale structure and dynamic details of attractors of even moderate dimension...The theoretical tools that characterize attractors of moderate or large dimensions in terms of the modest amounts of information gleaned from trajectories* [i.e., particular solutions]...*do not exist...they are more likely to be* ***probabilistic than geometric in nature***, Guckenheimer (1986). Therefore, it is indeed unlikely that one can succeed in hunting individual structures of finite dimensions using low-dimensional tools not to mention isosurfacing based on one parameter since the evolution of finite objects is not low dimensional. The remaining question is what

[9]Hopf (1948) conjectured that the underlying attractor is finite dimensional due to presence of viscosity.

does one see in reality being named as "structure", "coherent structure", etc. This question deserves far more serious attention beyond weakly founded speculations.

At present the dynamical systems community advocates an alternative approach to turbulence, based on recently found simple invariant solutions and connecting orbits in Navier-Stokes flows, Cvitanovič and Gibson (2010), Kawahara et al. (2012). This requires handling of ODEs systems with a large number of degrees of freedom for pretty moderate Reynolds numbers, typically larger 10^5 for $Re \sim 10^2$, which hardly can be qualified as low-dimensional. This means that even here one cannot avoid statistics. However, although the system of these equations seems not to be far easier to solve than the full time-dependent three-dimensional Navier–Stokes equations there is an important hope and even promise to "get into" what von Neumann wrote (1949): *nothing less than a thorough understanding of the* [global behavior of the] *system of all their solutions would seem to be adequate to elucidate the phenomenon of turbulence... There is probably no such thing as a most favored or most relevant, turbulent solution. Instead, the turbulent solutions represent an ensemble of statistical properties, which they share, and which alone constitute the essential and physically reproducible traits of turbulence*. The results obtained so far seem to be significant for a theoretical description of transition to turbulence. However, the claim that the same is true of "also fully turbulent flow" seems to be a bit premature even for low Reynolds numbers $\leq 10^3$ as long as one talks about things like "resembling the spatially coherent objects found in the near-wall region of true turbulent flows", having "the potential to represent coherent structures", "simple invariant solutions could represent turbulence dynamics, whereas the simple solutions themselves would represent coherent structures embedded in a turbulent state", reminiscent of things observed in DNS as "regeneration cycle in the buffer layer", etc. All this for low Reynolds numbers with considerably lower capability than DNS of NSE. The bottom line is that one is tempted to ask the question by Cvitanovič and Gibson (2010) *Should this be called 'turbulence?'*. Resemblance is far less than necessary and definitely not sufficient.

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Part IV
Epilogue

As mentioned at the very beginning, there is far more to say about the difficulties rather than achievements when it comes to the basic aspects of the problem. We reiterate some main points with somewhat different accents which are followed by some articulate remarks of more general nature.

In contrast to enormous progress in a variety of applications, the state of matters in basic research in turbulence is pretty modest and not much different from that defined by von Neumann (1949) as a "deadlock," especially as concerns the essence, i.e., the absence of theory—both mathematical and physical—based on first principles and inadequate tools to handle both the problem and the phenomenon of turbulence. The basic properties of turbulence of fundamental nature and *the essential mathematical complications of the subject were only disclosed by actual experience with the physical counterparts of these equations*, von Neumann (1949). He stated that *under these conditions, there might be some hope to "break the deadlock" by extensive, but well-planned computational efforts*. However, the latter has so far enabled great contributions to what has been achieved by *actual experience with the physical counterparts of these equations*. In other words, today, as before, observations (which now include both physical and DNS) remain the major exploratory tool in elucidating the properties of turbulence as a physical phenomenon.

This state of affairs can to a large extent be qualified as long-lasting and continuing paradigmatic crisis. It is noteworthy that thirty years after Neumann Liepmann gave a brief outline of a similar situation in 1979 in a seven-page paper titled "The rise and fall of ideas in turbulence." Concerning at least the basic aspects, the state of matters at present is not much different except for an enormous increase in the amount of publications claiming "theory" in one sense or another. Regarding the essence, all ideas/theories, etc., proposed so far have failed falling into the category of misconceptions and/or ill-defined concepts, etc., Chaps. 5, 6, 9, and 10 in Tsinober (2009). Some, though outdated, are still in use. All these are the consequence of the profound difficulties of the subject and which "travel" from one publication to another without much change. Below we recollect several examples of these with some emphasis on those pretending to a paradigmatic status. The bottom line is that there is no paradigmatic basis for fundamental research in turbulence. For this reason, we start the chapter on the status with the question why turbulence is so impossibly difficult. With its imprint on the problem in general and this chapter in particular, it

is interconnected with rest of this part. We continue to reiterate with an overview of the status using main/selected issues of a general nature and other general aspects not already touched upon.

This part ends with an Appendix on Essential Quotations as the outstanding theoretical thinking and efforts of great people in the field are of special importance. This is because and in spite of the absence of a theory of turbulence based on first principles so that the efforts should continue *until we have a consistent theory of turbulence*, Landau and Lifshits (1987), although as of today this seems to be an unrealizable dream; see Appendix on Essential Quotations.

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Chapter 10
On the Status

Abstract The cental issue here is the long-lasting and continuing paradigmatic crisis in fundamental turbulent research especially in theoretical/mathematical but also experimental (both observational and numerical) aspects. This is the general reason of special importance for discussion of the conceptual and problematic aspects, physical phenomena, observations, misconceptions and unresolved issues rather than conventional formalistic aspects, models and similar. We start from the question on why the turbulence and the turbulence problem is so impossibly difficult with a number of specific aspects as seen from the subtitle below. These involve the major qualitative properties of turbulent flows and reiteration of problematic main/selected issues of a general nature, on one hand and theory versus experiments and/or theoreticians versus experimentalists and vice versa with some emphasis on what comes next. The following chapter is a colection of Essential Quotations in view of the importance of the outstanding theoretical thinking and efforts of great people in the field.

10.1 On Why Turbulence Is So Impossibly Difficult?

This is an overview of the issue concerning the reasons for the absence of theory of turbulence based on first principles. Or perhaps, there is *no such thing as* 'turbulence'...*no 'real turbulence problem'*... Saffman (1978).

Finally, we should not altogether neglect the possibility that there is no such thing as 'turbulence'. That is to say, it is not meaningful to talk of the properties of a turbulent flow independently of the physical situation in which it arises. In searching for a theory of turbulence, perhaps we are looking for a chimera.

Turbulent phenomena of many types exist, and each one of practical importance can be analysed or described to any required degree of detail by the expenditure of sufficient effort. So perhaps there is no 'real turbulence problem', but a large number of turbulent flows and our problem is the self imposed and possibly impossible task of fitting many phenomena into the Procrustean bed of a Universal turbulence theory (Saffman 1978).

One of the things that I always found troubling in the study of the problem of turbulence is that I am not quite sure what the theoretical turbulence problem actually

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is. One reason I think we have so much difficulty in solving it, is that we are not really sure what it is... I just cannot think of anything where a genuine prediction for the dynamics of turbulent flow has been confirmed by an experiment. So we have a big vast empty field (Saffman 1991).

Saffman is not alone, e.g. Martin (1976), see Sect. 11.1.1 On Absence of Genuine Theory with quotations from great scientists. Saffman is definitely right in claiming the rather obvious absence of basic theory. But he is doing this too literally and thereby predicting a less than bright future for further research. At the same time he is doing an injustice to the outstanding theoretical thinking and efforts of great people in the field. Furthermore, (i) not everybody in the field was sure about the need for *a Universal turbulence theory*; (ii) except of quantitative universality, i.e. universality of numbers there is *qualitative* universality which is of the utmost importance as it concerns the fundamental physical processes as universal attributes of turbulence.[1] An immediate example is the process of self-amplification of strain and vorticity along with other and most importantly unknown processes. The former was mostly unnoticed by the theoretical community as the correct cause of the dissipation anomaly in turbulence rather than the widespread fictitious "energy cascade" etc.

On the constructive side theoretical thinking is of the utmost importance and it is vital for experimentalists, both physical and numerical. We point out that the observational aspect is extremely not trivial in such a highly-dimensional system as turbulence. It is intimately related to the skill or art of asking the right and correctly-posed questions: one has to have at least some idea what to look at and for what reasons, i.e. one must have some idea about what the problems of turbulence are. Thus the "experimentalists" should possess a serious theoretical background. The role of experiments in turbulence goes far beyond the view of those who think of experimentalists as just a superior kind of professional fixers knowing how to turn nuts and bolts into a confirmation of other people's 'theories' which paradoxically do not yet exist. In other words, it is necessary to look for the reasons as to why turbulence is indeed so impossibly difficult. We proposed the following list consisting of several interconnected categories: objective, subjective, "technical" and alike. The all contribute in one way or another to the impossibly difficult nature of the turbulence problem.

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[1] Concerning the definition of turbulence theory. There is considerable 'turbulence' in the numerous attempts to define what turbulence is indicating that such attempts at the present stage are futile.

In contrast to mathematical *theories* the definition of the main object of study precedes the results. In physics it is vice versa. Usually it happens when one first studies a new phenomenon and only at a later stage, after understanding it sufficiently, classifying it, etc. a most reasonable definition is found. Turbulence is not a new field, but this time has yet to come and it may not come soon if at all – to quote A.N. Kolmogorov (1985): *I soon understood that there was little hope of developing a pure, closed theory, and because of absence of such a theory the investigation must be based on hypotheses obtained on processing experimental data*, see Tikhomirov (1991, p. 487). Even if such a definition would be possible it is likely to come after the basic mechanisms of turbulence as a physical phenomenon are well understood. Moreover, in the case of turbulence it seems too optimistic to look for a definition of a quantitative nature.

10.1.1 Objective

These reflect the main universal inherent qualitative properties of turbulent flows:

- Nonintegrability - not solvable 'by hands', chaotic behavior
- Nonlinearity - not just "purely inertial". At the level of derivatives it is responsible for the essentially dissipative (dissipation anomaly) and rotational nature of turbulence via the purely understood mechanisms of *self-amplification* of strain ($-s_{ij}s_{jk}s_{ki}, \langle -s_{ij}s_{jk}s_{ki}\rangle > 0$) and vorticity ($\omega_i\omega_j s_{ij}, \langle \omega_i\omega_j s_{ij}\rangle > 0$).
- Nonlocality - the turbulent motion at some point in time and space is **not** defined by its immediate proximity. The 'general idea' that the Navier–Stokes equations are local PDEs in physical space is generally incorrect. There is a need to study turbulence as an undecomposable whole. An example of particular concern *is the inherent property of nonlocality questioning the validity of the K41 hypotheses on local homogeneity/isotropy in bounded flow domains surrounded by nonhomogeneous/nonisotropic flow regions*.[2]
- Non-Gaussianity and irreversibility,
- No low-dimensional description - turbulence is a highly dimensional system
- No small parameters

The above plus some additional 'minor' aspects comprise the essence of theoretician's frustration, see Moffatt (2002) on Batchelor's frustration on turbulence[3]: *... that sense of frustration that afflicted Batchelor (and many others) from 1960 onward...These frustrations came to the surface at the now legendary meeting held in Marseille (1961) to mark the opening of the former Institut de Mécanique Statis-*

[2]It is a common view in consensus that in real systems the symmetries are broken due to instabilities, forcing (excitation), inflow, initial and boundary conditions, but that following Kolmogorov the symmetries are expected to be restored locally in statistical sense at high Reynolds numbers. However, this expectation ignores the role of nonlocal nature of turbulence. The consequence due to effects of nonlocality is that, generally, one cannot apply results for globally homogeneous, isotropic turbulent flows to flows which are approximately homogeneous in a bounded region of flows and otherwise are non-homogeneous/non-isotropic, because the nonlocality makes local homogeneity, isotropy, etc. impossible unless the whole (infinite extent) flow is such, which is trivially impossible. Thus the "quasi-homogeneous" behavior of some quantities in bounded regions (*surrounded by nonhomogeneous/nonisotropic flow regions)* is misleading due the nonlocal nature of turbulence see the example on the flow in plane channel in Sect. 7.3.

As mentioned the attraction of Kolmogorov hypotheses is that they concern the local properties of *any* turbulence, not necessarily homogeneous or isotropic or decaying or stationary, provided the Reynolds number is large enough. The consistency of these hypotheses is debated for quite a while both from the theoretical point of view, Gkioulekas (2007), Hill (2006) and references therein, and also experimentally, but as with other similar issues without much progress from the fundamental point.

[3]*Formal mathematical investigations have produced remarkably little value... A number of general procedures for calculation of various dynamical aspects of homogeneous turbulence have been devised, but none of them impresses me as being likely either to advance our understanding of turbulence or to achieve results on which we can place reliance...The universal similarity theory of the small-scale components of the motion stands out in this rather grey picture as a valuable contribution...* Batchelor (1962).

tique de la Turbulence (Favre 1962), as well as numerous non-trivial difficulties for experimentalists, both physical and numerical.

10.1.2 Subjective

Before proceeding it should be emphasized that among the consequences of the absence of a theory is that turbulence research is loaded with misconceptions and ill-defined concepts, misinterpretations, outdated "paradigms", a multitude of problematic analogies along with other issues based on human perception, dogmas, hypotheses and the like – quite a bit of what is called "understanding" in turbulence in the absence of any real alternative(s). In short, all ideas/theories, etc. proposed so far have failed falling into the category of misconceptions and/or ill-defined concepts, see Chaps. 5, 6, 9 and 10 in Tsinober (2009). Consequently, all of the above fall under the category 'subjective'.

Some popular examples are given below.

Several Interrelated Misconceptions and/or ill- Defined Concepts in One Package

• Turbulence is an essentially inviscid, inertial phenomenon and is uninfluenced by the precise nature of the viscous mechanism, viscosity is irrelevant in the 'inertial range' and dissipation is clearly seen as a passive process in the sense that it proceeds at a rate dictated by the inviscid inertial behavior of the large eddies.

Contrary to the above turbulence is an essentially strongly dissipative (lots of dissipation) and rotational (plenty of vorticity) phenomenon; the "guilty" process for these properties is the self amplification of velocity derivatives (both strain and vorticity) by the nonlinearity of NSE. This process is also responsible for the dissipation anomaly rather than the fictitious 'cascade' and/or vortex stretching. This is because viscosity along with nonlocality matters very much in the (ill-posed, thus nonexistent) conventionally defined 'inertial range' including the 4/5 law at large Reynolds numbers! It is for this reason the question by Kraichnan (1974): *How a theoretical attack on the inertial-range problem should proceed is far from clear*, is irrelevant because this question relates to a nonexistent object. Inertial range is an ill defined concept.

Misconceptions and Misinterpretations

• Spatial fluxes represent 'cascade' in physical space.

• Turbulence can be described adequately by equations 'simpler' than the Navier–Stokes equations, e.g. by a low dimensional system.

• When Re is large vorticity is virtually frozen into the fluid.

• Kolmogorov picture is structureless and quasi-Gaussian.

• Large scales and small scales are decoupled.

• Large scales obey the Euler equations.

• Turbulence possesses a random (quasi)-Gaussian background

• Large spatial scales of motion convect very small scales without directly causing significant internal distortion of the small scales.

Problematic Analogies

• Between active (velocity, vorticity) and passive (material elements, scalar, vector) fields. The famous analogy between vortex and material lines, between vorticity and magnetic field are among the earliest incorrect ones, for more see Chap. 9 Analogies, Misconceptions and Ill-defined Concepts and also 5, 6, and 10 in Tsinober (2009).

The difference is seen most distinctly in case of a purely Gaussian velocity field. This is because such a field is dynamically impotent. Indeed, in such a field, all the odd moments vanish. This invalidates the Kolmogorov 4/5 law, turns the Karman–Howarth equation into a linear one, and prevents the self-production of velocity derivatives, i.e. production of enstrophy and strain (i.e. dissipation): in a Gaussian velocity field $\langle \omega_i \omega_j s_{ij} \rangle \equiv 0$, $\langle s_{ij} s_{jk} s_{ki} \rangle \equiv 0$, which are known empirically to be positively skewed, see Fig. 7.5 above. Gaussian fields lack vorticity-strain alignments and essential structure(s), to mention some. Another important aspect is that a Gaussian field lacks intermittency and structure by definition. In contrast in cases of passive fields the third moments are not vanishing, e.g. the Yaglom 4/3 law analogous to the Kolmogorov 4/5 law remains valid and the passive fields are in possession of their own very much different intermittency and structure(s).

10.1.3 "Technical" and Related

First, a reminding by von Neumann (1949) about inadequate tools to handle both the problem and the phenomenon of turbulence: ... *the essential mathematical complications of the subject were only disclosed by actual experience with the physical counterparts of these equations* (i.e. experimentally). . . *The entire experience with the subject indicates that the purely analytical approach is beset with difficulties, which at this moment are still prohibitive. The reason for this is probably as was indicated above...not having succeeded at anything like deep mathematical penetration in any part of the subject, we are still quite disoriented as to the relevant factors, and as to the proper analytical machinery to be used.* Note the 'bias' of a mathematician/theoretician: equations first, but cf. the statement by Kolmogorov (1985) as concerns the role of experiments!

The issue with inadequate tools remains acute too with the difference that - to quote Siggia (1994) - *In contrast to this experimental cornucopia, theory can offer only a few crumbs*.

The bottom line is put forward as the first item in the list

• Inadequate tools - theoretical/analytical and experimental/numerical - to handle both the problem and the phenomenon of turbulence.

The first concern here is the use of statistical methods and possible alternatives. There is an essential difference between the "enforced" necessity to employ statistical

methods in view of the absence of other methods and tools and the impossibility in principle(?!) of studying turbulence via other approaches, Monin and Yaglom (1971). Insisting on statistical theory as the only possibility in the theory of turbulence also leads to the necessity of low-dimensional description: *It is both reasonable and realistic to expect that the removal of small scale and high-frequency components of the dynamics of a flow be described in terms of modified molecular transport coefficients like an eddy viscosity*, Orszag et al. (1993). This removal, however, includes quantities, i.e. *small scale and high-frequency components*, containing a great deal of fundamental physics of the whole flow field such as the inherent rotational and dissipative nature of turbulence among others and their direct and bidirectional coupling with the large scales. Thus, relying solely on statistical methods (with all due respect) one is inevitably losing/missing some essential aspects of the basic physics of turbulence such as its inherently dissipative and rotational nature.

The insistence on statistical methods is especially discouraging all attempts to get into more than just "en masse" approaches, although there are also some statistical methods for handling extreme events, large deviations and the like. This is a particularly alarming issue due to problems with the judicious use of employing statistical approaches (among others) which use limited/partial information since physical quantities and the relations between them (such as physical laws and alike) are **not** synonymous to their statistical "images" and human interpretations. We return below to the issue of using limited/partial information. Such a standpoint means that not much can be expected concerning the essence of turbulence using only statistical methods. There are a number of other issues concerning the judicious use of statistical methods (as described in Chaps. 5 and 6) we mention here one of special importance on the relation between statistical predominance and dynamical significance with the former being **not** the most significant dynamically as compared with the latter.

In reality other approaches are being used in some special cases as described in the text. At present the dynamical systems community advocates an alternative approach to turbulence, see e.g. Cvitanović (2013). There is some hope and possibly promise to "get into" what von Neumann wrote about in 1949 possibly at moderate Reynolds numbers.

One more issue is

• Ambiguities of language with imperfect/inadequate terminology, which is symptomatic of lack of the field maturity. One of the most frequent is the use of terms like 'scale' in a multitude of meanings along with 'similar' 'eddy' 'structure' and alike without giving any definition(s).[4] There is another category of even 'less defined' terms such as "relevant" and "significant" and other buzzwords. Use of an ambigu-

[4] *What is meant by 'scale' is hard to define precisely*. Chorin (1994).

Fully turbulent flows consist of a wide range of scales, which are classified somewhat loosely as either large or small scales. Sreenivasan and Antonia (1997).

An eddy eludes a precise definition, but it is conceived to be turbulent motion, localized within a region of size l. Pope (2000).

...to a large extent we have failed. The key point is that we have not yet agreed on what we mean by an eddy. Davidson (2004).

ous language in science generally and even more in fields with profound difficulties such as turbulence encourages the proliferation of serious misconceptions, ill defined concepts and misinterpretations.

10.2 Reiteration of Problematic Main/Selected Issues of General Nature

• Energy cascade (versus decompositions), inertial range and its intermittency and 'anomalous scaling', 'hierarchical structure' of turbulence and their numerous representations and descriptions, rather than explanation, by things like breakdown coefficients/multipliers Novikov (1971, 1990) or equivalently multi-fractals, Frisch (1995) and the so-called 'hierarchical symmetry' She et al. (2017). The main problems are due to misinterpretations of observations such as in the case of the famous poem by Richardson (1922) p. 66. The point is that this observation was made by *analogy* looking at the structure of clouds, i.e., condensed water vapor, at the interface between laminar and turbulent flows in their bulk, which today is known as not necessarily reflecting the structure of the underlying velocity field. The analogy between genuine and passive turbulence is illusive and mostly misleading, see above Fig. 1.3. Just like many subsequent and more general problems in using experimental data; an almost infinite belief in some hypotheses as in the case of the existence of inertial range and, more generally, an excessive belief in the locality properties of turbulence; the view that turbulent flows are hierarchical, which underlies the rather fictitious concept of cascade, which although convenient, is more a reflection of the unavoidable (due to the nonlinear nature of the problem) hierarchical structure of models of turbulence and/or decompositions rather than reality; similarly the view that turbulence structure(s) is(are) simple in some sense and that turbulence can be represented as a collection of simple objects only seems to be a nice illusion which, unfortunately, has little to do with reality. It seems somewhat wishfully naive to expect that such a complicated phenomenon as turbulence can be merely described in terms of collections of only such 'simple' and weakly interacting objects. We reiterate that structure - whatever this means - can be very simple and complex in the same flow depending on the field of interest, see Fig. 1.3. In particular, flow visualizations used for studying the structure of dynamical fields (velocity, vorticity, etc.) of turbulent flows may be quite misleading, making the question "what do we see?" extremely nontrivial. The commonly used isosurfacing/thresholding cannot be expected to correspond to the generic aspects of turbulence structure(s) and objects in question many of which still wait to be found and properly defined. There is no turbulence without structure(s). Every part (just as the whole) of the turbulent field – including the so-called 'structureless background' – possess structure. Structureless turbulence (or any of its part) contradicts both the experimental evidence and the Navier–Stokes equations. The qualitative observations on the *little apparent structure in the low intensity component* or the *bulk of the volume* with *no particular visible*

structure should be interpreted as indicating that no *simple visible* structure has yet been observed in the bulk of the volume in the flow. It is a reflection of our inability to 'see' the more intricate aspects of turbulence structure: intricacy and 'randomness' are not synonyms for the absence of structure.

• Origins of vorticity amplification, prevalence of vortex stretching over compressing and high rate of dissipation in turbulent motion, Taylor (1938). The main problems are due to a misconception that vorticity amplification (which is a dynamical process) is due to the same mechanism as stretching of material lines (which is a kinematical process) as with other passive objects, see Chap. 9 Tsinober (2009); misinterpretation of the prevalence of vortex stretching - *the dissipation anomaly (enhanced dissipation) is due to strain self production, rather than fictitious energy cascade, and prevalence of compression in this process. It is the strain self production which plays the role of an engine producing the whole field of velocity derivatives, both itself and the vorticity. It is of special importance on a paradigmatic level that it is the strain production which is responsible for the finite overall dissipation at (presumably) any however large Reynolds numbers.* An important aspect is that the field of strain is efficient in the above two missions only with the aid of vorticity, i.e. only if the flow is rotational. There is a conceptual and qualitative difference between the nonlinear interaction between vorticity and strain, e.g. $\omega_i\omega_j s_{ij}$ and the self-amplification of the field of strain i.e., $s_{ij}s_{jk}s_{ki}$, which is a specific feature of the dynamics of turbulence having no counterpart (more precisely analogous – not more) in the behavior of passive and also active objects. This process, i.e., $-s_{ij}s_{jk}s_{ki}$ is local in contrast to $\omega_i\omega_j s_{ij}$, as the field of vorticity and strain are related nonlocally.

• Modeling, the problem of closure, eddy viscosity, low dimensional description and "structural approach", LES and similar and even "the dynamical equations of coherent structures".[5]

This is a set of problems comprising the everlasting dream to be able to, e.g. obtain the mean properties and large scale properties of turbulence without solving directly the NSE. No wonder *that most of the theoretical work on the dynamics of turbulence has been devoted (and still is) devoted to ways of overcoming the difficulties associated with the closure problem*, Monin and Yaglom (1971, p. 9). These difficulties have not been overcome and it does not seem that this will happen in the near future if at all. Moreover, not everybody in the community will agree to qualify this work as really theoretical or basic. Nevertheless the common and even massive practice is to employ some "parameterization", e.g. representing *the unresolved scales in climate models by imagining an ensemble of sub-grid processes in approximate secular equilibrium with the resolved flow*, Palmer (2005), see also Palmer and Williams (2008). The use of such approaches – all being a kind of low dimensional description - has been successful and even adequate in the narrow sense as a (semi) empirical tool. However, from the basic point of view the dream about the low-D description seems to be ephemeral – there cannot be an adequate low-D description of turbulence. This would be a major oversimplification abusing the

[5] Just a reminder: *The equations, in fact, preclude the assumption of any ad hoc model, although this is often done in the absence of a better idea*, Lumley (1970).

small scales as carrying lots of the essential physics of turbulence as an essentially dissipative and rotational phenomenon, the complex interaction of large/resolved and small/unresolved scales including the direct and bi-directional, i.e. non-locality, which is an inherent property of turbulence either.

Indeed, looking at the equations for the small/unresolved scales it is straigtforward to realize that the small/unresolved scales depend on the large/resolved scales via nonlinear space and history-dependent functionals, i.e. essentially non-local both spatially and temporally. So it is unlikely – and there is accumulating evidence for this – that relations between them (such as "energy flux", but not only) would be approximately local in contradiction to K41a hypotheses and surprisingly numerous efforts to support their validity.

Among the strong arguments for this is that the low-D description is killing an important part of turbulence physics residing in the "small scales" and in particular those associated with the dissipative and rotational properties of turbulence - the two properties of distinct paradigmatic nature and importance. Looking for the right reasons the unresolved scales cannot be represented by *simple formulae relating them to rest of the flow* due to the inherent non-local properties of turbulence among other reasons. In other words, in the low-D approaches the rotational and dissipative properties do not seem to be considered as belonging to the essential dynamics due to the very nature of low-D approaches: *The basic question* (which usually is not asked) *concerning statistical description is whether such complex behavior permits a closed representation that is simple enough to be tractable and insightful but powerful enough to be faithful to the essential dynamics.* Kraichnan and Chen (1989). Thus from a fundamental point of view a fluid flow which is adequately represented by a low-dimensional system is not turbulent – a kind of definition of 'non-turbulence'.

10.3 Theory Versus Experiments and/or Theoreticians Versus Experimentalists and Vice Versa - What Is Next

Before getting to this issue we briefly recall several points directly relevant for the sequel.

Concerning genuine theory it is hard to imagine any clear optimistic expectations. Indeed, along with the absence of genuine theory there is no consensus on what the problem(s) of turbulence is(are) and what would constitute its(their) solution; there is no consensus on what the main difficulties are and why turbulence is so impossibly difficult: almost every aspect of turbulence is controversial, which by itself is one of the greatest difficulties. Neither is there agreement on what constitutes understanding, this can only be accomplished by a genuine theory: just as no sophisticated experiment (laboratory or DNS[6]) can by itself bring understanding, neither can modeling of whatever sophistication, see references and quotations in the

[6] *Progress in numerical calculation brings not only great good but also awkward questions about the role of the human mind... The problem of formulating rules and extracting ideas from vast masses*

Appendix. Judging by previous experience there is no basis for optimism concerning all known theoretical approaches, and definitely including statistical methods due to their inherent limitations.

The above is not to claim the absence of theory(ies). On the contrary, there are plenty, many with qualitatively different and even contradictory premises but all agreeing well with **some** experimental data *(interpretational abuse)* and even claiming rigor - people attempting to find it are in danger of a non-trivial waste of time - but not necessarily for the right reasons and not based on first principles with few exceptions having no direct bearing on the Navier–Stokes equations. On the contrary, as mentioned there are plenty.

There is a need for a genuine physical theory with some non-trivial understanding of the physics taking into account the nonlocal properties of turbulence. Any theory based on some "locality", "small parameters", etc., has little chance to succeed if any. It seems that no small parameters exist, except of the genuinely small parameter introduced by Saffman (1978), which he called information density, ϵ_I, and defined as the ratio, S/N, in the literature, with $S =$ signal (*understanding*), and $N =$ noise (*mountains of publications*).

10.3.1 Theoreticians Versus Experimentalists and Vice Versa

In view of the general theoretical failure, i.e. the absence of theory based on first principles there are a number of issues of special concern about the relation(s) of what today is called "theory" in turbulence research and observations/experiments. These include the use of factual information concerning fundamental aspects and as it stands now the experimental 'confirmation' of a 'theory' has become meaningless. The above concern also includes the issue of the non-trivial resources (not just money) needed for serious progress. Special care is needed as there is no theory to guide the observations. Moreover, it is not unusual that the hard data (both in physical observations and numerical computations) is considered as kind of inferior compared to 'models' so that the former are 'tested' against the latter and not vice versa as it is done even in fields being in possession of rigorous theories. The approach in reality should be reversed.

There exist a great variety of observations in a great variety of conditions. The problem is that the theoretical community itself did not express much interest in most. This is because almost (if not all) of these observations did not match the requirements of a variety of excessively idealized concepts and "simplifications" such as homogeneity and anisotropy both local and global, scaling and other symmetries, universality, periodic BC's, very large Reynolds numbers, etc.

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of computational or experimental results remains a matter for our brains, our minds (Zeldovich 1979).

There are essential differences between physical and numerical experiments. If underresolved the former still provide correct information which is a problem with the latter.

It is becoming clear that such a matching is impossible as the idealizations are far more different than conditions corresponding to all observations. In other words, calculations are done in ignorance of or ignoring realistic experimental results.Thus the turbulence community is in danger of staying divided forever between experimentalists who observe what cannot be explained and theoreticians who try to explain what can not be observed, and there will be more attempts to replace explanation with mere description. The existing theoretical "frame" of idealizations is too narrow mainly due to problems associated with nonlocal effects. Insistence on such a "frame" results in the misinterpretation of experimental data and irrelevant models such as in the case of "anomalous scaling" and intermittency in the "inertial range" - a nonexistent object. In other words, due to nonlocal effects - with numerous manifestations - one cannot generally assume that approximately homogeneous (isotropic) regions in a bounded region in flows, which are otherwise non-homogeneous, have the same properties as those that are globally homogeneous, although it is done pretty frequently. This is also true of local homogeneity and isotropy.

The issue of interpretation and validation refers not just to "theories", which anyway do not exist, but in the first place to the factual information. The main point here is that the right results should be interpreted and related to the right reasons at least concerning fundamental studies.[7] One of the problems is misinterpretation, which is "aided" by the extremely complex nature of the problem. Another acute problem is that most of the theories claim explanations of some specific aspects of observations, while in fact being mere descriptions of these same particular observations.

The correspondence with experimental results may and unfortunately mostly does occur for the wrong reasons. *It is clear that if a result can be derived by dimensional analysis alone . . . then it can be derived by almost any theory, right or wrong, which is dimensionally-correct and uses the right variables,* Bradshaw (1994), i.e. this correspondence is at best a necessary condition. One of the problems is misinterpretation, which is "aided" by the extremely complex nature and highly dimensional of the problem , e.g. there are up to 10^{29} degrees of freedom in the atmosphere.

Most of the evidence was obtained at moderate Reynolds numbers for finite nontrivial non-isotropic and non-homogeneous systems, e.g. jets which consist of coexisting turbulent and non-turbulent regions. Theoreticians claimed an 'explanation' based on "infinite" objects/boxes assuming some properties such as isotropy and homogeneity. Today they say that the effects of a finite box are of special interest. Hence the insistence on various kinds of locality. *It is well known that theoretical physics is at present almost helpless in dealing with the problem of strong interactions... and that it is necessary to consider "distributed", non-local, interactions… Unfortunately, the non-local nature of the interaction renders completely useless the technique of the present existing theory.* Landau (1960).

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[7] In applications this may be not as crucial since *even wrong theories may help in designing machines*, (Feynmann, R. 1996) Lectures on Computation, Addison-Wesley.

Gradient transport ideas (which have been around since the beginning) are understood to be wrong in principle, yet they are used daily with moderate success by industry. Understanding how this can be (it is thoroughly explained by Tennekes and Lumley 1972*, p. 57) sheds light on turbulence,* Kraichnan (1976).

In the absence of a true theory based on first principles and in view of the high dimensional nature of the problem the issue of the confirmation/validation of 'theories' in turbulence is far more serious than just checking the 'validity' of some formulae or any other theoretical matter via comparison with limited experimental evidence, especially because turbulence is a highly dimensional problem. Use of some partial information is one of the most acute and general problems as the main part of the problem which is qualified as "interpretational abuse".

10.3.2 Interpretational Abuse

Many examples in attempting interpretation, validation, etc. fall under the category of interpretational abuse of one kind or another. A pretty frequent one is due to the use of limited experimental information evidence for different reasons. An outstanding example (for more see Chap. 6 above) of this kind is found in Monin and Yaglom (1971, p. 21) :... *we have avoided introducing experimental results which have no theoretical explanation and which do not serve as a basis for some definite theoretical deductions, even if these data are in themselves very interesting or practically important.* This is because *one of the principal incentives for writing this book was a desire to summarize the development of the idea of a universal local structure in any turbulent flow for sufficiently large Reynolds number*. **On the contrary, this is the most important situation where theoreticians are needed most desperately**. In other words, they looked for confirmation of something very important as everybody does. Unfortunately, just like stated by Liepmann (1979), digging a bit deeper into the observational evidence proved that what Monin and Yaglom were seeking appeared as problematic as all previous and subsequent ideas, i.e. the role of experiments in turbulence appears to go far beyond the view of those who think of experimentalists as a superior kind of professional fixers knowing how to turn nuts and bolts into a confirmation of other people's 'theories'.

The above approach is to a large degree what Saffmann not unjustly called "postdiction" and is not very useful especially in a situation where from the basic point of view there is almost nothing to be confirmed. *Correlations after experiments done is bloody bad. Only prediction is science* (Hoyle 1957). Moreover, quoting (Saffman 1978), *It must of course be kept in mind that the achievements or intrinsic value of a theory is not decided by democratic means. The majority view in science is not necessarily the right one.* This stands is drastic contradiction with the modern 'culture' of evaluation such as 'ratings' and similar bureaucratic 'inventions', which has contributed significantly to the alarming overabundance and continuing major over-production of publications. It is indeed amazing that, e.g. *the one mechanism for turbulence dissipation at high Reynolds numbers that has dominated turbulence textbooks and research activity over the past seven decades is indeed the Richardson–Kolmogorov cascade*.

An important example of different nature on interpretational abuse is the Kolmogorov 4/5 law at large Reynolds numbers

$$S_3 = -(4/5)\,\langle\epsilon\rangle\, r + 6\nu d S_2/dr, \tag{8.1}$$

with the third-order moment $S_3 \equiv \left\langle(\Delta u)^3\right\rangle$ being universal at large Reynolds numbers due to smallness of the term $6\nu d S_2/dr$, i.e. determined solely by the mean energy dissipation rate $\langle\epsilon\rangle$. Therefore (8.1) is considered as purely inertial relation (as independent of viscosity) and IS therefore used as a popular argument for the existence of the "inertial range" (IR). However, this would be true if the term $6\nu d S_2/dr$ in (8.1) would represent all the contributions from viscosity, i.e. if the $S_3 \equiv \left\langle(\Delta u)^3\right\rangle$ or more precisely just velocity increments $\Delta u \equiv [\mathbf{u}(\mathbf{x}+\mathbf{r}) - \mathbf{u}(\mathbf{x})]\mathbf{r}/r$, would ***not*** contain non-negligible contributions from dissipative events! In other words, *in reality what is termed IR is* ***assumed*** *to be purely inertial and having no contribution from viscosity for* $\eta \ll r \ll L$. Thus the question is about the impact of viscosity (dissipative events) on the conventionally defined IR (CDIR) at high Reynolds numbers. In other words, the second Kolmogorov (1941a) hypothesis involves a strong assumption that the dissipative events such that at least at one of their ends $(\mathbf{x}, \mathbf{x}+\mathbf{r})$ the instantaneous dissipation $\epsilon > q\,\langle\epsilon\rangle$ with $q > 1$ do not matter for the statistics of velocity increments in CDIR, as long as r is within the *"inertial range"*. Otherwise the *"inertial range"* would be out of existence as a concept, which seems to be the case. Note that the above question stems from the two-point nature of the statistics used, so that it is more than legitimate to ask similar questions of other quantities than just velocity increments, such as two-point correlations among many others.

Experiments at large Reynolds numbers with access to instantaneous dissipation at large Reynolds numbers showed that indeed, see Figs. 8.1 and 8.2, there exists a substantial number of dissipative (!) events living in the conventionally defined dissipative range contributing essentially to the PDF of velocity increments in the CDIR at Reynolds numbers $Re_\lambda \approx 10^4$.

Thus - contrary to the common view - the $4/5$ law is not a pure inertial relation at large Re. The important implication of a more general nature is that nonlinear interactions (NI) are not synonymous with purely inertial ones. They (NI) consist of purely inertial ones with an essential contribution from the viscous and cross-interactions. In the case of the third order functions and energy fluxes these interactions are constrained by the 4/5 law. *The consequence is that the purely inertial and dissipative events are adjusting to keep some quantities such as the total dissipation or the energy flux approximately constant at large Reynolds numbers*. The constraint responsible for this adjustment is just the 4/5 law or more generally the NSE. The bottom line is that the conventionally defined inertial range is an ill defined concept in spite the validity of the 4/5 law because of a non-negligible "hidden" dependence on viscosity due to the contribution from the viscous events to the $S_3 \equiv \left\langle(\Delta u)^3\right\rangle$ in the CDIR.

The third example of interpretational abuse concerns the misinterpretations of analogies such as between the genuine (e.g. NSE) turbulence and passive "turbulence", i.e. evolution of passive objects in random (or just not too simple) velocity fields.

The differences are more than essential, though there are numerous claims for *the well-established phenomenological parallels between the statistical description of mixing and fluid turbulence itself* , Shraiman and Siggia (2000) which are a con-

sequence of the multiple repetition of this claim without almost any factual basis. Among the earliest examples is the famous verse by Richardson (1922) p. 66 related to the cascade picture of turbulent flows: ... *we find that convectional motions are hindered by the formation of small eddies resembling those due to dynamical instability. Thus C.K.M. Douglas writing of observations from aeroplanes remarks: "The upward currents of large cumuli give rise to much turbulence within, below, and around the clouds, and the structure of the clouds is often very complex". One gets a similar impression when making a drawing of a rising cumulus from a fixed point; the details change before the sketch is completed. We realize thus that: big whirls have little whirls that feed on their velocity, and little whirls have lesser whirls and so on to viscosity—in the molecular sense.*

The point is that this observation was made by looking at the structure of clouds, i.e., condensed water vapor, at the interface between laminar and turbulent flows in their bulk, which today is known not necessarily reflecting the structure of the underlying velocity field. The analogy between genuine and passive turbulence is illusive and mostly misleading, see above Fig. 1.1, Chap. 1 and Tsinober (2009) Sects. 9.3, 9.4, 9.5.4 and references therein. Of special interest in this context is a recent statement by Eyink and Frisch (2011) p. 362: *Kraichnan's model of a passive scalar advected by a white-in-time Gaussian random velocity has become a paradigm for turbulence intermittency and anomalous scaling (the authors mean the genuine NSE turbulence)... The theory of passive scalar intermittency has not yet led to a similar successful theory of intermittency in Navier–Stokes turbulence. However, the Kraichnan model has raised the scientific level of discourse in the field by providing a nontrivial example of a multifractal field generated by turbulence dynamics. It is no longer debatable that anomalous scaling is possible for Navier Stokes.*

Here there are three points of a problematic nature. First, the Kraichnan model it is not a paradigm for turbulence intermittency and anomalous scaling – it is not even a paradigm for intermittency and anomalous scaling for passive objects due to use of an unphysical velocity field. Second, it is not an a example of a multifractal model, but rather an example of anomalous scaling which is definitely not synonymous.[8] And third, it is definitely not debatable that anomalous scaling is possible for Navier Stokes, even more the anomalous scaling **is in** the Navier Stokes as it is observed in experiments for quite a while in real systems which are described by NSE. However, the real issue is about the reasons/interpretation or even better underlying mechanisms. The very problem is reflected by the term "anomalous scaling" as it is observed in what is called the "inertial range" which in reality is an non-existent object being not well defined due to severe contamination of the conventionally defined inertial range by strong dissipative events at whatever large Reynolds numbers,

[8]In this context it is of interest to quote Goto and Kraichnan himself (2004): *Multifractal models of turbulence have not been derived from the NS equation but they are supported by theoretical arguments and their parameters can be tuned to agree well with a variety of experimental measurements... Multifractal cascade models raise the general issue of distinction between what is descriptive of physical behavior and what can be used for analysis of data... Multifractal models may or may not express well the cascade physics at large but finite Reynolds numbers.*

see Chap. 8. Far more can be found on misuse and misinterpretations of analogies in Chap. 9, Tsinober (2009). For additional examples see also Sect. 6.1 above.

One more aspect is about the attitude of modelers: *Whenever they fail in their predictions, scientists tend to blame the poor accuracy of the observations, the lack of computer power and the inadequate parametrization in their numerical models, rather than their own lack of skill in computing the accuracy that can be obtained with present resources* Tennekes (1993).

The bottom line is that a narrow mathematical, theoretical, observational, etc. standpoint is outstandingly and especially harmful for basic research in turbulence. The exception seems to be the physical standpoint understood in as broad sense as one can imagine.

10.4 If Not Theory - What to Do and What Are the Questions

Ya.B. Zeldovich used to say that *Use of similarity and dimensional analysis in turbulence is more an art than science.* It seems that this is true also of most questions in basic aspects of turbulence.

P.G. Saffman wrote a most interesting paper in 1978 with an introduction which among other things was devoted *to the general philosophical question of the aims, purpose and motivation of the theoretical study of turbulence.* Today theoreticians are in the position of being in possession of valuable advice and the like, but without the ability of implementing Samman's advice on the subject of being studied.

A.N. Kolmogorov wrote in 1985 *I soon understood that there was little hope of developing a pure, closed theory, and because of absence of such a theory the investigation must be based on hypotheses obtained on processing experimental data.* A bit later in 1978 he added ... *the observational material is so large, that it allows to foresee rather subtle mathematical results, which would be very interesting to prove.*

V. Yudovich wrote in 2003 *Sometimes experiments provide us with so beautiful and clear results that it is a shame on theorists that they cannot interpret them.*

There is far more all having difficulties in formulating the "right" questions. Hence the problems as mentioned by Saffman (1978) and also by Martin (1976) concerning the existence of essence in the problem.

Among the answers so far is to move along the von Neumann's suggestion with the hope of breaking the deadlock, but not only by extensive well -planned computational efforts. It has been obvious for quite a while that DNS is not as powerful as claimed, von Neumann was too optimistic especially as concerns large Reynolds numbers. Physical experiments are badly needed especially at large Reynolds numbers as advised by Kolmogorov. Among the most important observations are those associated with the rotational and dissipative nature of turbulence and its nonlocal properties. Though there is no theory to guide the observations we are lucky to be in possession of the NSE. It is true that there is little substantial theoretical use of NSE in turbulence, since there is almost no way to use them explicitly in theoretical approaches, e.g. by

solving them by 'hand'. However, there are several ways to do this implicitly, i.e. by indirect use of NSE and their consequences. For example, looking at the NSE and their consequences such as those for vorticity strain, helicity and some others, enables us to recognize the dynamically important quantities and physical processes involved. In other words, NSE and their consequences tell us at least in part what quantities and relations should be studied, see e.g. pp. 366–7 in Tsinober (2009) for the equations of evolution of enstrophy and strain production revealing the invariant quantities $\omega_i\omega_j\Pi_{ij}$ and $s_{ik}s_{kj}\Pi_{ij}$. Another example is the equation for the "energy" of strain s^2 discussed in Chap. 7 above which by itself allowed us to arrive at the unequivocal conclusion about the key role of the strain self production in the issue of dissipation anomaly, self-amplification of the field of velocity derivatives (including vorticity) rather than that of vorticity. Even the very form of the Lagrangian formulation of NSE as compared to the Eulerian one provokes nontrivial and important questions of a conceptual nature.

One of the problems is that any serious planning of experiments (numerical, laboratory, field) cannot be undertaken without reasonable a priori basic understanding. The lack of such comprises a real deadlock in turbulence research. So the success of basic research depends strongly on scientific curiosity and those who are not afraid of real adventure. Despite and because of the state of affairs in the field it is extremely rewarding to venture into this mysterious world and the hope is on intelligent, clever and courageous young people who are not overly obsessed with just publications, etc. and money.

An important question is what not to do. As the reader has already realized from the text the list is not short. In any case one must definitely be extremely careful in the "validation" of 'theories', as the latter disappear pretty fast, unless the "new idea" looks trustworthy and attractive enough. The observational aspect is not that trivial in such a highly-dimensional system as turbulence if one does not stay with the hopeless choice of one velocity component, although even that is not that easy. But in such a case the price is non-trivial issues of interpretation and many other.

10.4.1 Paradigmatic Versus Pragmatic

The future of basic research depends strongly on questions of paradigmatic nature independently and as contrasted to pragmatic needs in applications. It is far more difficult close to impossible to ask the right questions to promote the basic research. However, this contrast is only short-term, as the history of natural sciences shows, disappears in the long run. The contrast may extend for a long time in cases when "science" is pursued solely/mainly for the purposes of making better weapons and neater gadgets and, of course, money. But in such a case it is destined to degeneration as science:

There are no such things as applied sciences, only applications of science Louis Paster 1872, Address 11 Sept 1872, Comptes Rendus des Travaux du Congress Viticole et Sericole de Lyon, 9–14 September, 1882, p – 49.

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Chapter 11
Appendix. Essential Quotations

Abstract Along with a relatively limited number of citations in the text we bring a small collection of citations in this Appendix, more can be found in Tsinober (2009) especially in the text and also Appendices A and B. In this reference one of the aims was an extensive treatment of the dialogue within the turbulence community with an emphasis on problems of a conceptual nature.

Here we put more emphasis on the early recognition in the community of the fundamental difficulties concerning the theoretical aspects of the problem, the consequent absence of theory based on first principles and inadequacy of tools to handle both the problem and the phenomenon of turbulence. This rather peculiar state of affai rs exists along with a set of deterministic differential equations, the Navier–Stokes equations, these probably containing all of turbulence, although most of our knowledge about turbulence has come from observations and experiments, laboratory, field and later numerical. The claim that theory is supposed to guide and give meaning to observation is somewhat exaggerated, see, e.g. page 1, although definitely it is of great imporatnce especially if it is more broadly understood as theoretical thinking.

Along with the absence of a genuine theory there is no consensus on what the problem(s) of turbulence is (are) and what would constitute its (their) solution. Neither is there agreement on what constitutes understanding which can only be brought about by a genuine theory.

This state of affairs was understood long ago by many outstanding people, Kolmogorov, von Neumann, Wiener and many other as quoted below and also in Tsinober (2009). Related issues concern the multitude of "approaches" and the continuing diversity of opinions on what is important, what the main questions are and similar.

In any case the outstanding theoretrical thinking and efforts of great people in the basic aspects of turbulence is of the utmost importance for everybody dealing with the problem. In view of the absence of a genuine theory it is especially vital for experimentalists, both physical and numerical.

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11.1 To Preface

11.1.1 On Absence of Genuine Theory

• *My overall impression of the Symposium is that no really new and important ideas have been presented... I think we must admit that little new theory has been put before us.* Batchelor 1959 Some reflections on the theoretical problems raised at the Symposium, Proceedings of a Symposium on Atmospheric diffusion and air pollution, editors F.N. Frenkiel and P.A. Sheppard, Advances in Geophysics, **6**, 449–452.

• *Formal mathematical investigations have produced remarkably little value... A number of general procedures for calculation of various dynamical aspects of homogeneous turbulence have been devised, but none of them impresses me as being likely either to advance our understanding of turbulence or to achieve results on which we can place reliance...The universal similarity theory of the small-scale components of the motion stands out in this rather grey picture as a valuable contribution.* Batchelor G.K. 1962 The dynamics of homogeneous turbulence: introductory remarks, In A. Favre, editor, Mécanique de la Turbulence, Colloques Interntionaux du CNRS, No. 108, Marseille 28 aôut-2 septembre 1961, p. 96.

• *Turbulence was probably invented by the Devil on the seventh day of Creation when the Good Lord wasn't looking.* P. Bradshaw 1994 Experiments in Fluids, **16**, 203.

• *There is a physical problem that is common to many fields, that is very old, and that has not been solved. It is not the problem of finding new fundamental particles, but something left over from a long time ago—over a hundred years. Nobody in physics has really been able to analyze it mathematically satisfactorily in spite of its importance to the sister sciences. It is the analysis of circulating or turbulent fluids.* R.P. Feynman, R.B. Leighton, and M.L. Sands. 1965 The Feynman Lectures on Physics, Vol. I Addison-Wesley Publishing Company, Sect. 3–7.

• *Turbulence is the last great unsolved problem of classical physics. Remarks of this sort have been variously attributed to Sommerfeld, Einstein, and* ***Feynman****, although no one seems to know precise references, and searches of some likely sources have been unproductive. Of course, the allegation is a matter of fact, not much in need of support by a quotation from a distinguished author. However, it would be interesting to know when the matter was first recognized.* P.J. Holmes G. Berkooz and J.L. Lumley 1996 Turbulence, coherent structures, dynamical systems and symmetry, Cambridge University Press.

• *As a doctorate I proposed to Heisenberg no theme from Spectroscopy but the difficult problem of Turbulence, in the hope, that WENN IRGENDEINER (if anybody), would solve this problem. However, the problem is until now not solved.* A. Sommerfeld 1942, *Scientia*, Nov./Dez 1942.

• *I soon understood that there was little hope of developing a pure, closed theory, and because of absence of such a theory the investigation must be based on hypotheses obtained on processing experimental data,* Kolmogorov A.N. 1985, in

notes preceding the papers on turbulence in the first volume of his selected papers; English translation, Selected works of A.N. Kolmogorov, **I**, ed. Tikhomirov, p. 487, Kluwer, 1991.

- *…the essential mathematical complications of the subject were only disclosed by actual experience with the physical counterparts of these equations…The entire experience with the subject indicates that the purely analytical approach is beset with difficulties, which at this moment are still prohibitive. The reason for this is probably as was indicated above: That our intuitive relationship to the subject is still too loose—not having succeeded at anything like deep mathematical penetration in any part of the subject, we are still quite disoriented as to the relevant factors, and as to the proper analytical machinery to be used. Under these conditions there might be some hope to "break the deadlock" by extensive, but well planned computational efforts.* J. von Neumann 1949 Recent theories of turbulence - A report to Office of Naval Research. Collected works, **6** (1963), pp. 468–469, ed. Taub., A.H., Pergamon. Note the 'bias' of a mathematician/theoretician: equations first, but cf. the statement by Kolmogorov 1985.
- *It remains to call attention to the chief outstanding difficulty (i.e. turbulence) of our subject.* H. Lamb 1927 Hydrodynamics, p. 651.
- *I am an old man now, and when I die and go to Heaven there are two matters on which I hope for enlightenment. One is quantum electrodynamics and the other is the turbulent motion of fluids. And about the former I am rather optimistic.* Sir Horace Lamb 1932 as quoted by S. Goldstein 1969, ARFM, **1**, 23.
- *No complete quantitative theory of turbulence has yet been evolved. A complete theory of turbulence (which does not yet exist) would have to make it possible…* L.D. Landau and Lifshits E.M. Fluid Mechanics1987 pp. 130, 147.
- *Turbulence is the graveyard of theories*, H.W. Liepmann, 1997 as cited by S.J. Kline, 1997 A brief history of boundary layer structure research, in Self-sustaining mechanisms of wall turbulence, editor R.L. Panton, p.4., Comp. Mech Publ.
- *It is much easier to present nice rational linear analysis than it is to wade into the morass that is our understanding of turbulence dynamics. With the analysis, professor and students feel more comfortable; even the reputation of turbulence may be improved, since the students will find it not as bad as they had expected. A discussion of turbulence dynamics would create only anxiety and a perception that the field is put together out of folklore and arm waving* Lumley, J.L. 1987 Review of 'Turbulence and random processes in fluid mechanics' by M.T. Landahl and E. Mollo-Christensen, J. Fluid Mech., **183**, 566–567.
- *Even after 100 years turbulence studies are still in their infancy. We do have a crude practical working understanding of many turbulence phenomena but certainly nothing approaching comprehensive theory and nothing that will provide predictions of an accuracy demanded by designers.* Lumley, J.L. and Yaglom, A.M. 2001 A century of turbulence, *Flow, Turbulence and Combustion*, **66**, 241–286.
- *It is at this point that the study of turbulence does prove to be an exception: the applied physics involvement is almost completely absent. In view of the extraordinary practical importance of turbulence…, this is quite astonishing. Yet the reason for such apparent neglect is easily found. Quite simply the fundamental problems of*

turbulence are still unresolved. D. McComb 1990 The physics of turbulence, Oxford Univ. Press p. vii.

- *While the experimental techniques that have been invaluable in understanding phase transitions promise to be very useful in the study of hydrodynamic phenomena, I suspect that the recent addition to our theoretical arsenal may be less effective than many had hoped.* Martin, P.C. 1976 The onset of turbulence: A review of recent developments in theory and experiment, in: L. Pál and P. Szépfalusy, editors, Statistical physics: Proceedings of the International Conference, Budapest, 5–29 August, 1975, pp. 69–96. Amsterdam: North-Holland.
- *…a fundamental theoretical understanding is still lacking.* M. Nelkin, 1994 Universality and scaling in fully developed turbulence, Adv. Phys., **43**, 143.
- *The existence of an asymptotic statistical state is strongly suggested experimentally, in the sense that reproducible statistical results are obtained. However, physical plausibility aside, it is embarrassing that such an important feature of turbulence as its statistical stability should remain mathematically unresol.ved, but such is the nature of the subject.* Orszag, S.A. 1977, Lectures on the statistical theory of turbulence, in: R. Balian and J.-L.Peube, editors, *Fluid Dynamics*, pp. 235–374, Gordon and Breach.
- *…we should not altogether neglect the possibility that there is no such thing as 'turbulence'. That is to say, it is not meaningful to talk of the properties of a turbulent flow independently of the physical situation in which it arises. In searching for a theory of turbulence, perhaps we are looking for a chimera,* P. G. Saffman 1978 Problems and progress in the theory of turbulence, Lect Notes in Phys., **76** (II), 276.
- *One of the things that I always found troubling in the study of the problem of turbulence is that I am not quite sure what the theoretical turbulence problem actually is… One reason I think we have so much difficulty in solving it, is that we are not really sure what it is… I just cannot think of anything where a genuine prediction for the dynamics of turbulent flow has been confirmed by an experiment. So we have a big vast empty field.* P. G. Saffman 1991 in The Global Geometry of Turbulence, NATO ASI Ser. B 268, ed. J. Jimenez, pp. 348, 349, Plenum.
- *In contrast to this experimental cornucopia, theory can offer only a few crumbs.* Siggia, E. 1994 High Rayleigh number convection, Ann. Rev. Fluid Mech., **26**, 137–168.
- *In spite of a huge number of papers and a large amount of research on turbulence, it remains an unsolved problem left for future generations.* Sinai, Ya.G. 1999 Mathematical problems of turbulence, Physica, **A263**, 565–566.
- *…the absence of a sound theory is one of the most disturbing aspects of the turbulence syndrome.* R.W. Stewart, 1969, Turbulence Nat. Committee for Fluid Motion Films, dist. Encyclopedia Britannica Educational Corp.
- *Sometimes experiments provide us with so beautiful and clear results that it is a shame on theorists that they cannot interpret them,* Yudovich, V.I. 2003. Eleven great problems of mathematical hydrodynamics. Mosc Math J 3:711–737
- *It has been realized since the beginning that the problem of turbulence is a statistical problem; that is a problem in which we study instead of the motion of a given system, the distribution of motions in a family of systems… It has not, however, been*

adequately realized just what has to be assumed in a statistical theory of turbulence, Wiener, N. 1938 Homogeneous chaos, *American Journal of Mathematics*, **60**, 897–936.

11.2 To Chap. 1

11.2.1 On Multitude of "Approaches"; for More See Also Chaps. 3 and 9 in Tsinober 2009

- *Kolmogorov's ideas on the experimentalist's difficulties in distinguishing between quasi-periodic systems with many basic frequencies and genuinely chaotic systems have not yet been formalized.* Arnold 1991.
- *From the point of view of theoretical physics, turbulence is a classical field theory, out of equilibrium and in a strong coupling regime*, Announcement by Chinese Academy of Sciences of a programme New Directions in Turbulence, held at Bejing Mar 12 to Apr 20, 2012.
- An example from the mathematical community by Foias et al. 2001: *The word turbulence has different meanings to different people,which indicates that turbulence is a complex and multifaceted phenomenon. For mathematicians, outstanding problems revolve around the Navier–Stokes equations (such as wellposedness and low-viscosity behavior,especially in the presence of walls or singular vortices). For physicists,major questions include ergodicity and statistical behavior as related to statistical mechanics of turbulence. Engineers would like responses to questions simple to articulate but amazingly difficult to answer: What are the heat transfer properties of a turbulent flow? What are the forces applied by a fluid to its boundary (be it a pipe or an airfoil)? To others pursuing the dynamical system approach, of interest is the large time behavior of the flow. Another ambitious question for engineers is the control of turbulence (to either reduce or enhance it), which is already within reach. Finally, a major goal in turbulence research – of interest to all and toward which progress is constantly made – is trustworthy and reliable computation of turbulent flows (see e.g. Orszag [1970] and Ferziger, Mehta, and Reynolds[1977]).*
- *In the present-day statistical fluid mechanics, it is always implied that the fluid mechanical fields of a turbulent flow are random fields in the sense used in probability theory.* Monin and Yaglom, 1971, pp. 3–4, 7.
- *It is natural to assume that in a turbulent flow…the...fluid dynamic variables, will be random fields* (Monin and Yaglom 1971, p. 214).
- *From the very beginning it was clear that the theory of random functions of many variables (random fields), whose development only started at that time, must be the underlying mathematical technique.* A.N. Kolmogorov, 1985 in notes preceding the papers on turbulence in the first volume of his selected papers, English translation, Tikhomirov, 1991, p. 487.

- A list of interest is from the engineering community by Lumley 1990: *Turbulence is rent by factionalism. Traditional approaches in the field are under attack, and one hears intemperate statements against long time averaging, Reynolds decomposition and so forth. Some of these are reminiscent of the Einstein–Heisenberg controversy over quantum mechanics, and smack of a mistrust of any statistical approach. Coherent structure people sound like The Emperors's new Clothes when they say that all turbulent flows consist primarily of coherent structures, in the face of visual evidence to the contrary. Dynamical systems theory people are sure that turbulence is chaos. Simulators have convinced many that we will be able to compute anything within a decade…The card-carrying physicists dismiss everything that has been done on turbulence from Osborne Reynolds until the last decade. Cellular Automata were hailed on their appearance as the answer to a maidens prayer, so far as turbulence was concerned.* Lumley 1990, as quoted by Cantwell, B.J. 1990 Future directions in turbulence research and the role of organized motion, in J.L. Lumley (ed)., *Whither turbulence?*, pp. 97–131, Springer.
- *I think that the k-space decomposition does actually obscure the physics*. Moffatt, H.K. 1990a in: J.L. Lumley, editor, Whither turbulence? Turbulence at the crossroads, Springer, Berlin, p. 296.
- *. . . one may never be able to realistically determine the fine-scale structure and dynamical details of attractors of even moderate dimension. The theoretical tools that characterize attractors of moderate or large dimensions in terms of the modest amounts of information gleaned from trajectories* [i.e. particular solutions]. . . *do not exist…they are more likely to be probabilistic than geometric in nature.* Guckenheimer, J. 1986 Strange attractors in fluids: another view, Ann. Rev. Fluid. Mech., **18,** 15–31.

11.3 To Epilogue

11.3.1 On the Continuing Diversity of Opinins on What Is Important, What Are the Main Questions and Related

- *. . .it is amazing how many different, nearly orthogonal, points of view there are about a phenomenon which is governed by Newton's innocent-looking, linear second law of motion, with a little help or hindrance from viscosity*. Bradshaw 2003 Review of Turbulent flows by P.S. Bernard and J.M. Wallace
- *…A principal objective of any theory of fluid motion is the prediction of the spread of matter or "tracer" within the fluid*, Bennet, A. 2006 Lagrangian fluid dynamics, CUP.
- *…The main ambition of the modern theory of turbulence is to explain the physical mechanisms of intermittency and anomalous scaling in different physical systems, and to understand what is really universal in the inertial-interval statistics.*

Falkovich, G., Gawedzki, K., and Vegassola, M. 2001 Particles and fields in fluid turbulence, Rev. Mod. Phys., **73**, 913–975.

- *…If we assume as a basic starting point in every theory of turbulence its representation in terms of spectral coefficients, statistical or physical averages, or more generally simple objects conditionally extracted by a weak background, turbulence modelling could be defined reductively, as the art of writing the equations that produce directly such quantities.* Germano, M. (1999) Basic issues of turbulence modelling, in: A. Gyr, W. Kinzelbach and A. Tsinober, editors, Fundamental Problematic Issues in Turbulence, 213–219, Birkhäuser.
- *…The separation of scales is a central problem in turbulence.* Germano 2012 The simplest decomposition of a turbulent field, Physica **D 241**, 284–287
- *. . .A property of turbulent motion is that the boundary conditions do not suffice to determine the detailed flow field but only average or mean properties. For example, pipe flow or the flow behind a grid in a wind tunnel at large Reynolds number is such that it is impossible to determine from the equations of motion the detailed flow at any instant. The true aim of turbulence theory is to predict the mean properties and their dependence on the boundary conditions.* Saffman P.G., 1968 Lectures on homogeneous turbulence, in: N.J. Zabusky, editor, Topics in nonlinear physics, pp. 485–614, Springer.
- *. . . An early goal of the statistical theory of turbulence was to obtain a finite, closed set of equations for average quantities, including the mean velocity arid the energy spectrum. That goal is now viewed to be unrealistic. The goal nowadays is to reduce to a manageable number the many degrees of freedom necessary to describe the flow, to determine the equations governing the dynamics of the reduced degrees of freedom, and to solve those equations analytically or numerically to calculate fundamental quantities that characterize the flow. Thus a theory may treat all or only some of the degrees of freedom statistically.* Frisch, U. and Orszag, S.A. 1990 Turbulence: challenges for theory and experiment, Phys. Today, **43**, 24–32.
- *. . .Herein lies the central obstac1e of the entire theory of turbulence known as the closure problem,* Jovanovi ć, J. 2004 The statistical dynamics of turbulence, Springer.
- *When coupled to the basic conservation laws of fluid flow, such treatment, however, leads to an unclosed system of equations: a consequence reasoned, in the scientific community, the closure problem. This is the central and still unresolved issue of turbulence which emphasizes its chief peculiarity: our inability to do reliable predictions even on the global flow behavior. The equations of motion for this covariance contain third order moments of the velocity field, the equations of motion for the third-order moments contain fourth-order moments, and so forth, ad injinitum. A central goal of turbulence theory is the closing of this infinite chain of coupled equations into a determinate set containing only moments below some finite order.* Kraichnan 1957 The structure of isotropic turbulence at very high Reynolds numbers, J. Fluid Mech., **5**, 497–543.
- *. . .The central problem of turbulence is to find the analog of the Gibbs distribution for the energy cascade. The mathematical formulation of this problem is amazingly simple. In the inertial range we can neglect the viscosity and forcing and*

study the Eulerian dynamics of an ideal incompressible fluid. Migdal, A.A. 1995 Turbulence as Statistics of Vortex Cells, in Mineev, V.P., editor, The first Landau Institute Summer School, 1993, Gordon and Breach, pp. 178–204.

- …Following is a set of questions distributed by John Lumley at the meeting Turbulence at the Crossroads Whither Tubulence? held at Cornell University, Ithaca, NY, March 22–24, 1989.

1. What are the advantages and disadvantages of long-time averaging (Reynolds averaging)? Is this a technique that is intellectually bankrupt, that systematically excludes certain information, that has mislead us over the years, or is it still viable if used judiciously?

2. Where do we stand on statistical approaches generally? Is it a problem that mean quantities seldom occur, or that the things being described statistically have considerable spatial structure? How do we feel about statistical approaches close to critical values of parameters, when the flow is relatively ordered?

3. What is our position on coherent structures? Do most turbulent flows consist of more- or-less organized structures, or is there a large range of organization/disorganization in flows with different etiology? How can we use this inform ation?

4. What role do we feel Dynamical Systems Theory can play in turbulence in open flow systems? What is the evidence for strange attractors in fluid turbulence? Are there Reynolds/Rayleigh/Richardson number limitations on the utility of these approaches?

5. Where do we stand on large eddy simulation and exact simulation vis a vis computer development? Is it only a matter of a couple of decades until we will be able to calculate everything, and hence we can soon abandon all modeling and experiment, or are some things still going to be out of reach? Which things?

6. How do we feel about modeling? Granted that there has been a lot of bad modeling done, there are more fundamental issues: Is it necessary? Is it potentially capable of real predictive value? What are the limitations of current approaches, if any? (e.g.- is the fact that it is based on Reynolds averaging a fatal flaw? Will it always be necessary to have zonal models?) Is it essential to include, I or example, coherent structures explicitly in modeling?

7. What role can we expect cellular automata to p1ay in the future of turbulence? The accuracy and cost relative to finite difference schemes appear to be respectively low and high, but are there loopholes? Are current schemes Galilaean invariant, and does it matter? Is there a niche where this approach can be useful?

8. The physics community appears to feel that most work on turbulence until the recent past was essentially engineering, and did not shed light on the heart of the problem. Many research managers in Washington seem to feel that, if this work was engineering, it was not responsive to program needs; only new and different approaches seem to attract support now. Are these perceptions of past accomplishments accurate, or are they a mix of specialty chauvinism, a failure to appreciate the difficulty of the turbulence problem, and the natural desire of a manager to make changes that will bear fruit during his tour of duty?

- *…The wake behind the cylinder exhibits a flow field which is chaotic in both space and time, but whose averages and statistical properties are stable. It is these averages and statistical properties that we want to understand*. Nelkin, M. 1989. What Do We Know about Self-Similarity in Fluid Turbulence, J. Stat. Phys., **54**, 1–15.
- *…We are not interested in details of the mechanism of energy input at small wave numbers (are they not important??) This problem has been nicely solved by Edwards* Nelkin, M. 1974 Turbulence, critical fluctuations, and intermittency *Phys. Rev.,* **A9**, 388–395
- *…What are the measures describing turbulence? The question arises because invariance under time evolution (the Hopf equation) is not enough to specify a unique probability measure which would describe turbulence. One knows indeed that strange attractors typically carry uncountably many disjoint invariant probability measures. The ones one wants probably satisfy stability under small stochastic perturbations. This is a powerful restriction, but hard to exploit,* Ruelle, D. 1984 Conceptual Problems of Weak and Strong Turbulence, Phys. Rep., **103**, 81–85.
- *…However, it might well be possible to get a very good approximation to the average velocity curve with much less work than that required to take and average all the measurements. Even more importantly, a successful approximation method would almost certainly illuminate the physics of turbulent flow. These are the hopes on which turbulence closure theory rests.* Salmon, R. 1998 Lectures on geophysical fluid dynamics, Oxford University Press.
- *…Do the Navier–Stokes equations on a 3-dimensinal domain Ω have a unique smooth solution for all time?… The solution of this problem might well be a fundamental step toward the very big problem of understanding turbulence*. Smale, S., 1998 Mathematical problems for the next century, *The Mathematical Intelligencer*, **20**, 7–15.

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