# MODELS VERSUS PHYSICAL LAWS, FIRST PRINCIPLES OR WHY MODELS WORK?"

*Wolfgang Pauli Institute, Vienna 2-4 February, 2011*

*Arkady Tsinober*

## Introductory notes for the general discussions:

### Questions, doubts, etc.

*"Why modeling works?", "Models versus physical laws/first principles" or "Modeling versus physics and mathematics in turbulence" "What is the meaning of the term `works' "?, "What is the meaning of experimental validation of models?" "Can models clarify the physics and produce genuine predictions or they are just a kind (?) of 'post-diction' and sophisticated methods of data description/fitting?"*

*Correlations after experiments done is bloody bad\*. Only prediction is science.* FRED HOYLE 1957, *The Black Cloud, Harper, N-Y.*

\*These are "postdictions"

# A proposal for constraining the discussions

A. Minimum of philosophy and excessive generalizations.

B. The truth is provided by a solution (possibly "statistical") of a "master" problem such as an IC and BC problem for PDE, for "simplicity" the NSE desirably without stratification, rotation, combustion, etc.

C. A model is almost "everything" not precisely the B. above. **The big ? is how much of "stripping" is adequate.**

We absolutely must leave room for doubt or there is no progress and no learning. There is no learning without posing a question. And a question requires doubt...Now the freedom of doubt, which is absolutely essential for the development of science, was born from a struggle with constituted authorities... FEYNMANN, 1964

*My personal **doubts** began (and never stopped) long ago from a simple observation:*
Computing velocity increments $\Delta u = u(x+r)-u(x)$ one encounters also large instantaneous dissipation at the ends $(x, x+r)$.

*Thus the Second Kolmogorov hypothesis involves a strong assumption that the dissipative events { such that at least at one of their ends $(x, x+r)$ the instantaneous dissipation $\varepsilon > q \langle \varepsilon \rangle$ with $q > 1$} do not matter for the statistics of velocity increments so that , e.g.*

...the mechanism of turbulent energy transport is not affected by the viscosity... the nonlinear terms are not affected by the viscosity. Kovasznay, 1948.

To (dis)prove this one needs access to instantaneous dissipation at large Reynolds numbers, see below

*To heat up*

- ❖ *Is it the RR* $^*$ *for the RR* $^{**}$ *if it "works"?*
- ❖ *Should the RR be for the RR if it "works"?*
- ❖ *(How) is it important to get the RR for the RR?*
- ❖ *Parameterization* $^{***}$ *and mimicking - are they necessarily the RRRR* (or perhaps the RRWR)!

*Essentially, all models are wrong but some are useful*, BOX AND DRAPER 1987 (Empirical model-building and response surfaces, Wiley series in probability and mathematical statistics. Applied probability and statistics. New York: John Wiley & Sons)

*even wrong theories may help in designing machines*, RICHARD FEYNMANN, 1996, (Lectures on Computation, Addison-Wesley)

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*RR – the right result, **RR - for the right reason

*** *parameterizations - the representation of key processes without resolving them* VON STORCH 2009

# Most frequently the RRWR* may be obtained by dimensional arguments:

*. . . 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.

## An exampl from debate of Obukhov and Batchelor in 1959

G. K. BATCHELOR. *I do not think that the agreement obtained by Obukhov with the Kolmogoroff and Richardson expressions is a confirmation of his assumption that turbulent diffusion can be regarded as a Markov process. That agreement seems to me to be necessary simply on dimensional grounds.*

A. M. OBUKHOV. *I believe, conversely, that the agreement indicates the possibility of applying a Fokker-Planck type of equation to turbulent diffusion problems.*

see Obukhov, A.M. 1959 Description of turbulence in terms of Lagrangian variables*, Advances in Geophysics*, **6,** 113-116; ATMOSPHERIC DIFFUSION AND AIR POLLUTION, Proceedings of a Symposium held at Oxford, August 24 -29,1958.

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*The right results for the wrong reason

# *On decompositions an related*

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The common approaches both in theory and data analysis in turbulence are reductionist ones, i.e., some decompositions of the flow field. There is a multitude of these from formal to heuristic ones. However, there are several non-trivial and generic difficulties with any decomposition mainly due to the nonlinear and nonlocal nature of turbulence. Large scale modeling is an outstanding (but not the only) victim of both, though nonlinearity is considered as the main guilty. It looks that nonlocatity is not less malignant.

*By nonlocality I mean (among other things) the direct an bidirectional coupling between large (resolved) and small (unresolved) scales, see Tsinober 2009, ch.6 An informal conceptual introduction to turbulence, Springer, xix, 464 pp.*

One of the popular paradigmatic examples is the heuristic decomposition on energy-containing (ECR), inertial (IR) and dissipative ranges (DR). It is massively accepted that the statistical properties of IR (and CR too) at large Reynolds numbers are universal (in some sense) and independent of viscosity/nature of dissipation and consequently of the properties of DR., which appears to be conceptually not correct.

*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* (HOLMES, BERKOOZ AND LUMLEY, 1996).

There are plenty of such statements, for more see, e.g. pp. 103, 335 in Tsinober 2009 *An informal conceptual introduction to turbulence,* Springer, xix, 464 pp.

It is the assumed universality (there is a spectrum of what this means) which forms some basis for a variety of modeling approaches all  assuming that *turbulence can be split into two groups: one consisting of the resolved geometry and regime-specific scales — the so-called energy containing scales; and the other associated with the unresolved smallest eddies, for which the presumably more-universal flow dynamics is represented with subgrid scale (SGS) closure models* (GRINSHTEIN 2009).

*The difficulty of these approaches is that there is no real separation between the large and small scales and there is  no "natural" decomposition . All decompositions are  "human made" . The exception is the NSE as a systematic approximate solution of the closure problem such as, e.g., the Chapman-Enskog development for Boltzmann's equation.  There exists a regime in which  the scale of variation of hydrodynamic variables is much larger than the molecular mean free path. The success of NSE closure  is – in  the first place - due to this scale separation. There is no such a scale separation in the case of LES, etc.*

From my last message

1. Thus the first issue concerns a set of questions as a consequence of universality (or not) of the unresolved/small/subgrid scales (SS). Whatever the meaning of the SS (non)universality, today there is some evidence that SS are not universal, for instance, due to nonlocal effects as, e.g. manifested in direct and bidirectional coupling between large and small scales. Consequently, it is difficult to agree that SS "do not care" about things like control of turbulent flows (both in utilitarian engineering sense and in the sense of mathematical theory of PDE's), differences in forcing, boundary and initial/inflow conditions, etc., even if all of them occur in LS.

A more annoying question is about small scale and/or broad-band excitation (forcing, additives, and boundary roughness). The SS appear to be not just a passive sink of energy of the LS, they react back on LS in various ways, so that it would be too presumptuously to claim that the properties of LS do not depend essentially on what happens in the unresolved small scales.

Hence again the question about the possibility and meaning of modeling/parameterization of SS from the basic point of view, i.e. the "solution" (if such exists at all) of the old problem of closure.

2.  To put it differently (but not identically), the issue is whether (or not) a low-dimensional description of turbulent flows is justified/possible from the basic point of view. Isn't it too subjective to qualify the large-scale (resolved) eddies as the most important ones?  A vitally important part of physics of turbulence resides in the small/unresolved scales. It is true that most of the energy contained in a flow is represented by the resolved large scales (LS), but can one claim that all important properties of LS do not depend essentially on what happens in the unresolved small scales?

3. A closely related question is about the relevance of Euler equations  to turbulence. The main reason for this question in the context of this Meeting is that Euler is used in one way or another for modeling.  In particular, it is endemically claimed that in the inertial range the flow is described by the Euler equations. There are two problems with such a statement. From the purely formal point the meaning of it is not clear for a PDE. From the physical point there are recent experimental indications that even at $Re\lambda \sim 104$ the concept of inertial range (as well as the dissipative) is not well defined, at least in physical space.

# Two more related questions

**4. Isn't it too subjective to qualify the large-scale (resolved) eddies as the most important ones because they *carry the bulk share of energy*. A vitally important part of physics of turbulence resides in the small/unresolved scales. It is true that most of the energy contained in a flow is represented by the resolved large scales (LS), but can one claim that all important properties of LS do not depend essentially on what happens in the unresolved small scales?**

**What about the scales responsible for turbulence production?  Are they really necessarily that large?  For example, those where most of vorticity (and strain) is produced.  A similar question about the near wall regions and sharp interfaces.**

**5. What about the "encouraging" insensitivity to the subfilter model claimed 15 years ago? Is it true that as the numerical resolution increases the results converge and become insensitive to the subfilter model? Is still the main expected physical role of the unresolved subgrid motions the dissipation of the resolved turbulence energy?**

# *THE QUESTION*

...*if* the IC information contained in the filtered-out smaller and SGS spatial scales can significantly alter the evolution of the larger scales of motion and practical integral measures, then the use of any LES for their prediction as currently posed is dubious and not rationally or scientifically justifiable.

GRINSHTEIN 2009, P. 2936

How can we know something/ anything about this IF ?

*…if* the IC information contained in the filtered-out smaller and SGS spatial scales can significantly alter the evolution of the larger scales of motion and practical integral measures, then the use of any LES for their prediction as currently posed is dubious and not rationally or scientifically justifiable. GRINSHTEIN 2009, p. 2936

How can we know something/ anything about this IF without knowing anything about the filtered-out smaller and SGS spatial scales (SS)? Or how much should we know about the **real SS** at **large** Reynolds numbers?

# The conventional inertial and dissipative ranges (CIR an CDR) are not well defined:

*Direct experimental evidence based on data at $Re_\lambda \sim 10^4$ with access to the field of velocity derivatives including dissipation*

Tsinober 2009 *An informal conceptual introduction to turbulence,* Springer, xix, 464 pp.

Kholmyansky, M. and Tsinober, A. (2009) On an alternative explanation of anomalous scaling and how well-defined is the concept of inertial range*, Phys. Letters, A373,* 2364–2367.

Gulitski, G., Kholmyansky, M., Kinzelbach, W., Lüthi, B., Tsinober, A. and Yorish, S. (2007) Velocity and temperature derivatives in high-Reynolds-number turbulent flows in the atmospheric surface layer. Parts 1–3, *J. Fluid Mech.,* **589**, 57–123.

Arkady Tsinober

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# Reminding I

## *The conventionally defined inertial range (CDIR)*

### KOLMOGOROV 1941a

$$\eta \ll r \ll L; \; \eta = (\nu^3/\varepsilon)^{1/4}$$

The massively accepted assumption/ hypothesis:

*In the inertial range,*
*the viscosity plays in principle no role.*

RUELLE, 1974.

# KOLMOGOROV 1941A

**The second hypothesis of similarity.**† *If the moduli of the vectors $y^{(k)}$ and of their differences $y^{(k)} - y^{(k')}$ (where $k \neq k'$) are large in comparison with $\lambda$, then the distribution laws $F_n$ are uniquely determined by the quantity $\bar{\epsilon}$ and do not depend on $\nu$.*

† In terms of the schematical representation of turbulence developed in the footnote ‡, $\lambda$ is the scale of the finest pulsations, whose energy is directly dispersed into heat energy due to viscosity. The sense of the second hypothesis of similarity consists in that the mechanism of translation of energy from larger pulsations to the finer ones is for pulsations of intermediate orders, for which $l^{(k)}$ is large in comparison with $\lambda$, independent from viscosity.

❖ These are the 3n-dimensional distribution laws of probabilities for the velocity increments

В т о р а я  г и п о т е з а  п о д о б и я⁴. *Если абсолютные величины векторов $y^{(k)}$ и их разностей $y^{(k)} - y^{(k')}$ (где $k' \neq k$) велики по сравнению с $\lambda$, то законы распределения $F_n$ однозначно определяются величиной $\bar{\epsilon}$ и не зависят от $\nu$.*

⁴ В терминах схематического представления о турбулентности, развитого в примечании 2, $\lambda$ есть масштаб наиболее мелких пульсаций, энергия которых непосредственно рассеивается в тепловую благодаря вязкости. Смысл второй гипотезы подобия заключается в том, что механизм передачи энергии от более крупных пульсаций к более мелким для пульсаций промежуточных порядков, для которых $l^{(k)}$ больше чем $\lambda$, не зависит от вязкости.
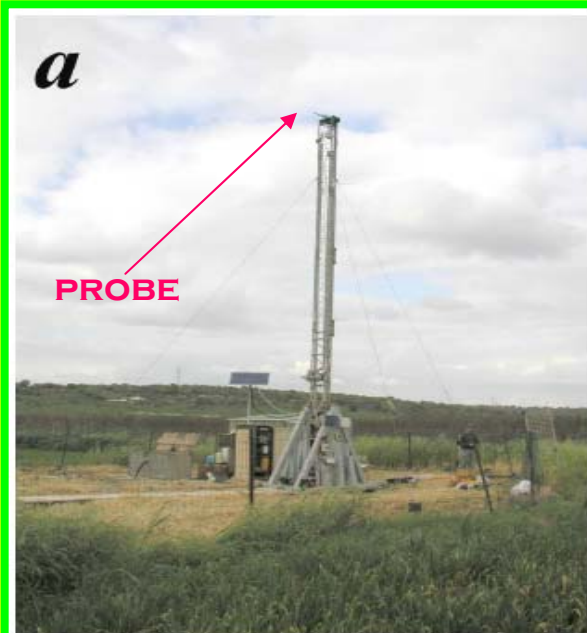
*My personal **doubts** began from a simple observation:*

*Computing velocity increments $\Delta u = u(x+r)-u(x)$ one encounters also large instantaneous dissipation at the ends $(x, x+r)$.*
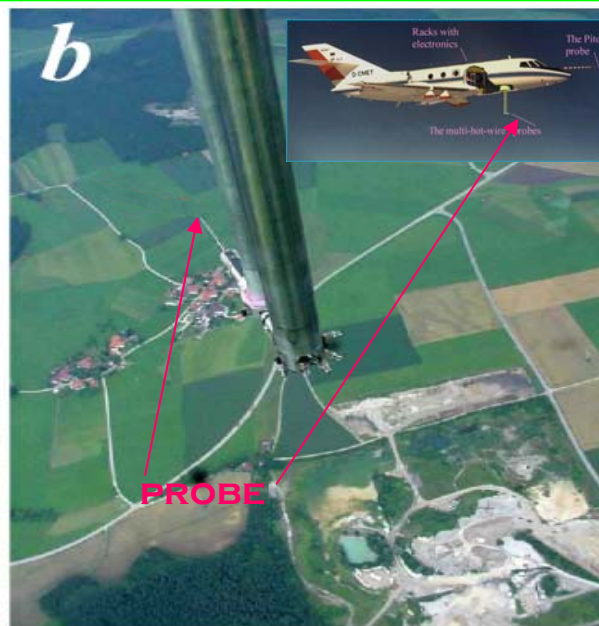
*Thus the Second Kolmogorov hypothesis involves a strong assumption that the dissipative events { such that at least at one of their ends $(x, x+r)$ the instantaneous dissipation $\varepsilon > q \langle\varepsilon\rangle$ with $q > 1$} do not matter for the statistics of velocity increments and*

*…the mechanism of turbulent energy transport is not affected by the viscosity… the nonlinear terms are not affected by the viscosity. Kovasznay, 1948.*

*To (dis)prove this one needs access to instantaneous dissipation at large Reynolds numbers.*

**a** PROBE

**b** PROBE

Racks with electronics — The Pitot probe

The multi-hot-wire probes

**c** PROBE

**Kfar Glikson measurement station, Israel, the probe on the mast (a). 1999**

**Airborne experiment, Germany, the probe in the flight (b). machine (c). 2000**

**Sils-Maria experiment, Switzerland, the probe on the lifting machine (c). 2004**

# THE MARIA SILS SITE, SWITZERLAND

Elevation 1850 m over the sea level
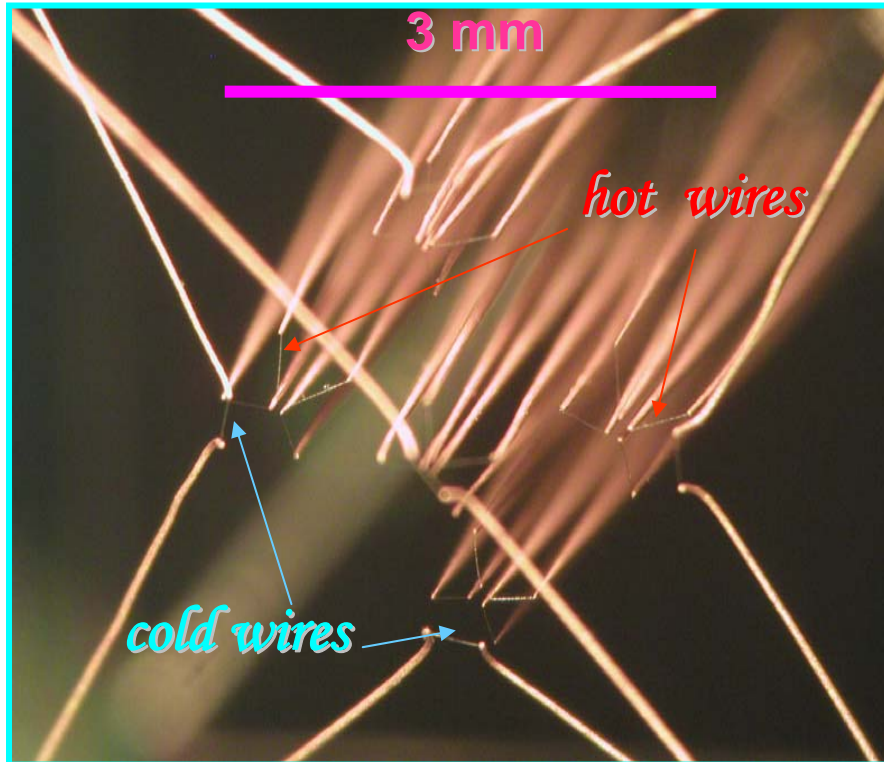
The runs were recorded at seven heights from 0.8 to 10 m above the ground

The experiment was performed in collaboration with the Institute of Hydromechanics and Water Resources Management, ETH Zurich

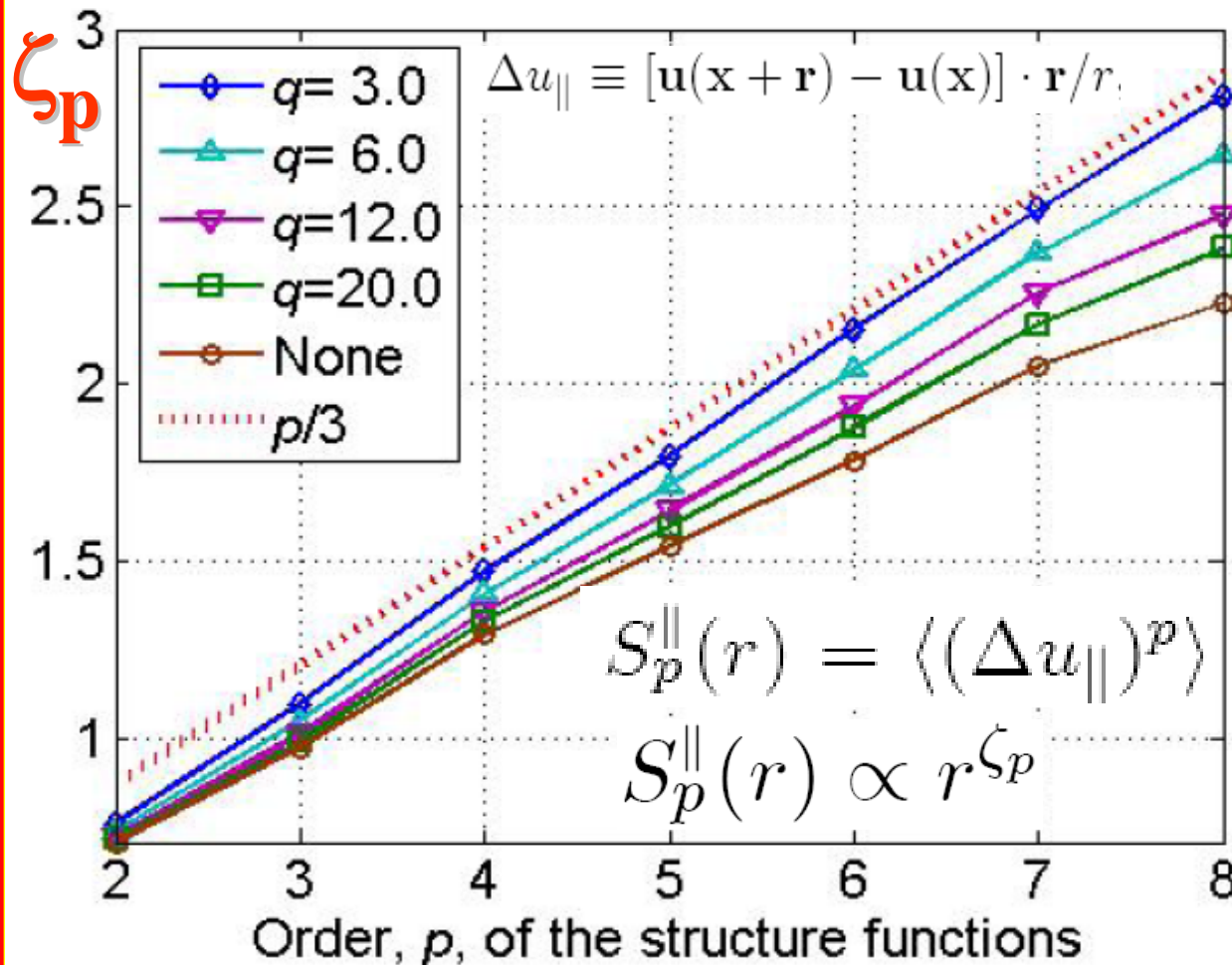Wind direction ("Maloja wind")

# THE PROBE





3 mm

hot wires

cold wires

The tip of the probe

Manganin is used as a material for the sensor prongs instead of tungsten because the temperature coefficient of the electrical resistance of manganin is 400 times smaller than that of tungsten.

**HISTOGRAMS** of the increments of the longitudinal velocity component for the full data and the same data in which the strong dissipative events with different thresholds were removed.. $r/\eta = 40$ corresponds to the lower edge of the inertial range. (a). $r/\eta = 400$ is deep in the inertial range (b)

An event $\Delta u = u(x+r)-u(x)$ is qualified as a strong dissipative if at least at one of its ends $(x, x+r)$ the instantaneous dissipation $\varepsilon > q \langle \varepsilon \rangle$ for $q > 1$

SCALING EXPONENTS, $\zeta_p$, of structure functions for the longitudinal velocity component for the full data and the same data in which the strong dissipative events with different thresholds were removed.

$\zeta_p$

$\Delta u_\parallel \equiv [\mathbf{u}(\mathbf{x} + \mathbf{r}) - \mathbf{u}(\mathbf{x})] \cdot \mathbf{r}/r$

- $q = 3.0$
- $q = 6.0$
- $q = 12.0$
- $q = 20.0$
- None
- $p/3$

$$S_p^\parallel(r) = \langle (\Delta u_\parallel)^p \rangle$$

$$S_p^\parallel(r) \propto r^{\zeta_p}$$

Order, $p$, of the structure functions

An event $\Delta u = u(x+r) - u(x)$ is qualified as a strong dissipative if at least at one of its ends $(x, x+r)$ the instantaneous dissipation $\varepsilon > q \langle \varepsilon \rangle$ for $q > 1$

# The 4/5 law is not a pure inertial relation at large Re?

$$S_3^{\parallel}(\mathrm{r}) = -(4/5)\langle\varepsilon\rangle\mathrm{r} + 6\nu\mathrm{d}S_2^{\parallel}(\mathrm{r})/\mathrm{d}r,$$
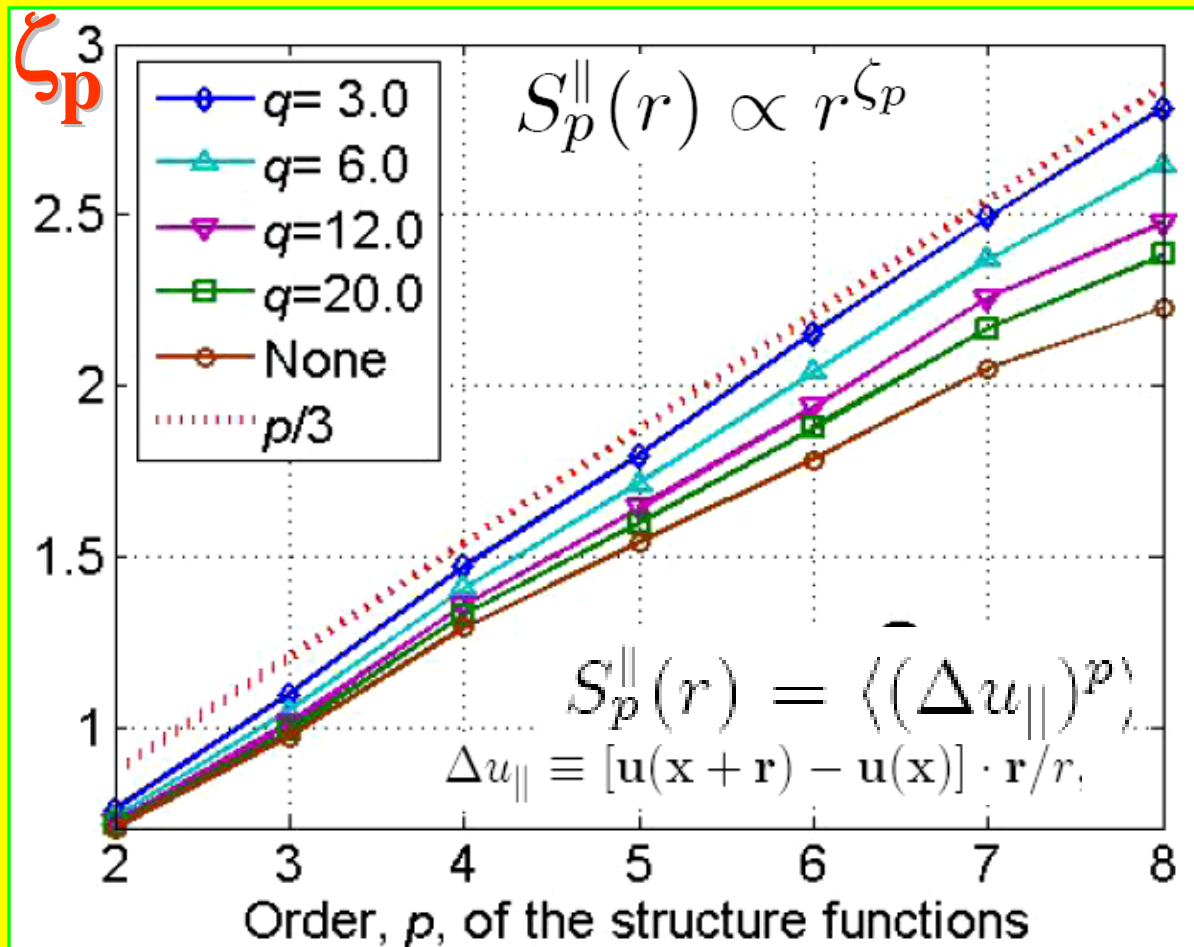
$$S_p^{\parallel}(r) = \langle(\Delta u_{\parallel})^p\rangle$$

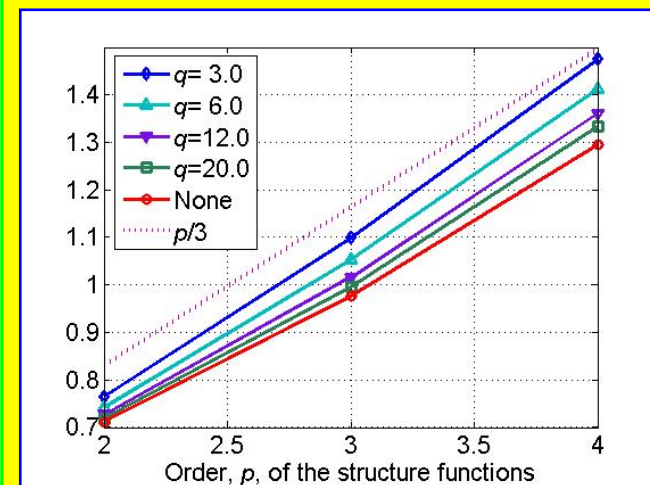$$\Delta u_{\parallel} \equiv [\mathbf{u}(\mathbf{x}+\mathbf{r}) - \mathbf{u}(\mathbf{x})]\cdot\mathbf{r}/r.$$

Strong dissipative events **DO** contribute to the 4/5 law, and removing them leads – among other things – to an increase of the scaling exponent above unity, see below.

An important point here is that the neglected viscous term in the von Karman–Howarth equation, $6\nu\mathrm{d}S_2(\mathrm{r})/\mathrm{d}r$, **does not contain ALL the viscous contributions**. Those which are present in the structure function **S$_3$ itself** remain and keep the 4/5 law precise. **In this sense the 4/5 law is not a pure inertial law.**

# Scaling exponents, $\zeta_p$, of structure functions for the longitudinal velocity component for the full data and the same data in which the strong dissipative events with different thresholds were removed.
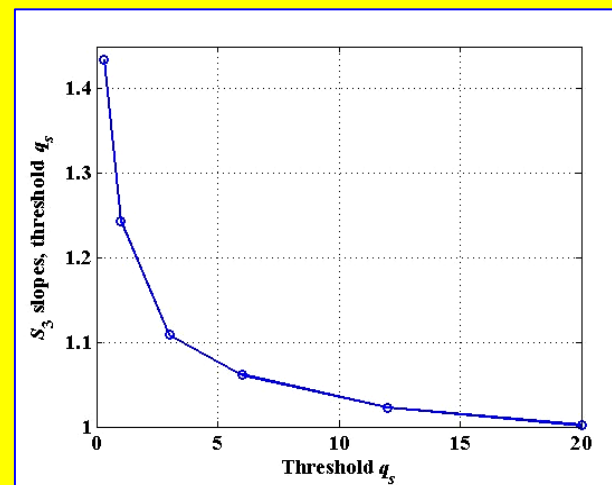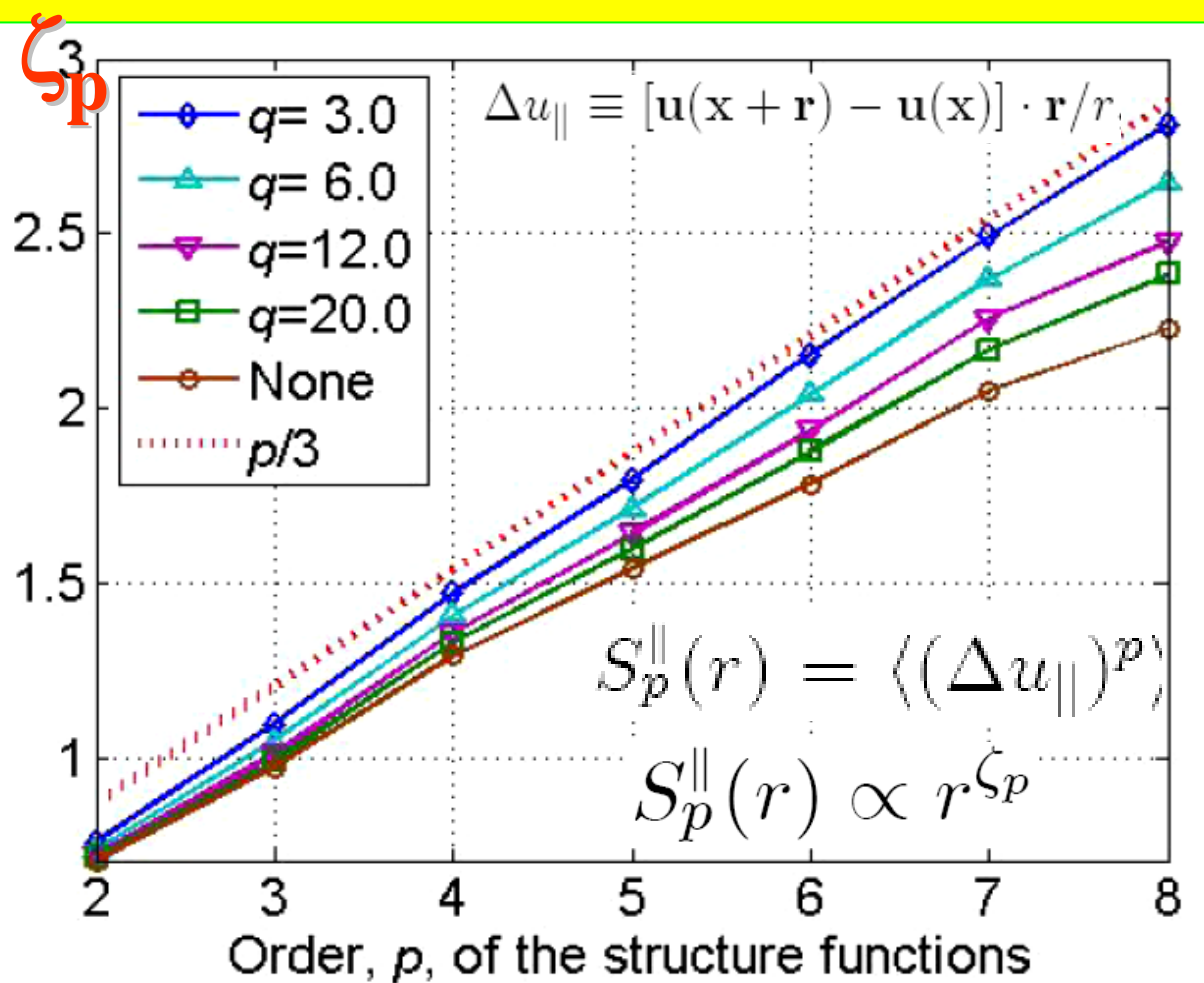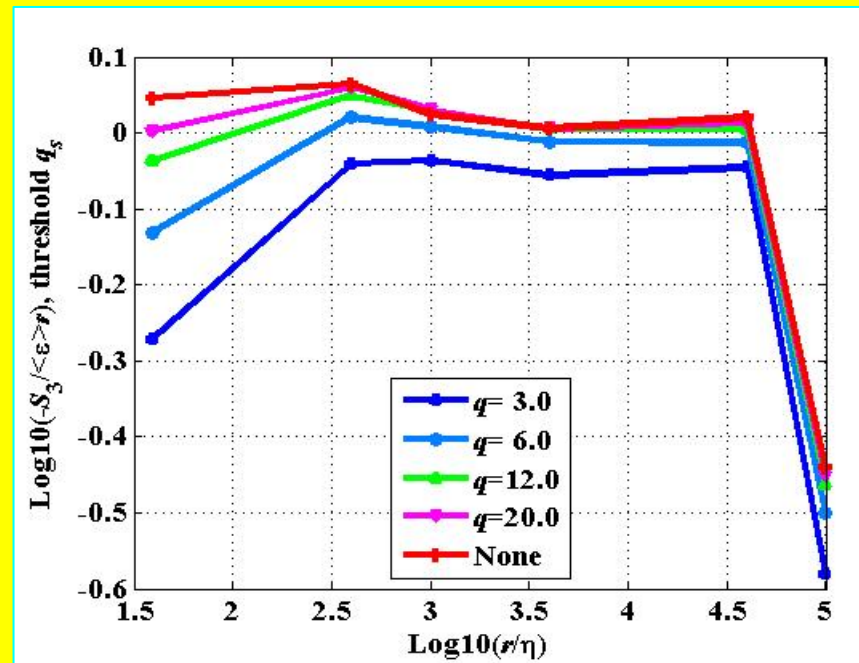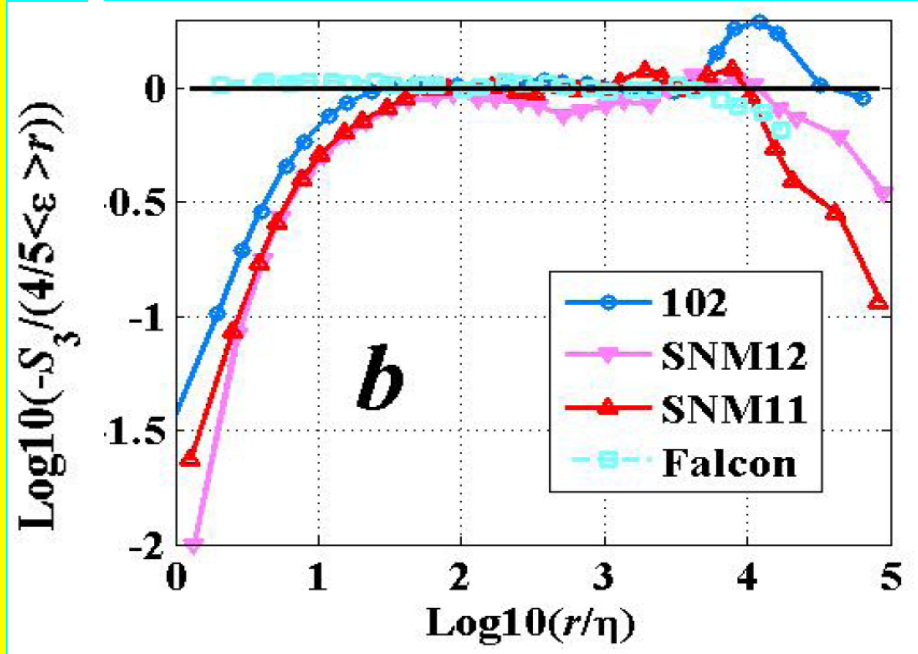


An event $\Delta u = u(x+r) - u(x)$ is qualified as a strong dissipative if at least at one of its ends $(x, x+r)$ the instantaneous dissipation

$$\varepsilon > q \langle \varepsilon \rangle \text{ for } q > 1$$

# Scaling exponents, $\zeta_p$, of structure functions for the longitudinal velocity component for the full data and the same data in which the strong dissipative events with different thresholds were removed.



$\zeta_p$

Legend:
- $q = 3.0$
- $q = 6.0$
- $q = 12.0$
- $q = 20.0$
- None
- $p/3$

$\Delta u_{\|} \equiv [\mathbf{u}(\mathbf{x}+\mathbf{r}) - \mathbf{u}(\mathbf{x})] \cdot \mathbf{r}/r$

$$S_p^{\|}(r) = \langle (\Delta u_{\|})^p \rangle$$

$$S_p^{\|}(r) \propto r^{\zeta_p}$$

Order, $p$, of the structure functions

An event

$\Delta u = u(x+r) - u(x)$

is qualified as a strong dissipative if at least at one of its ends $(x, x+r)$ the instantaneous dissipation

$\varepsilon > q \langle \varepsilon \rangle$ for $q > 1$
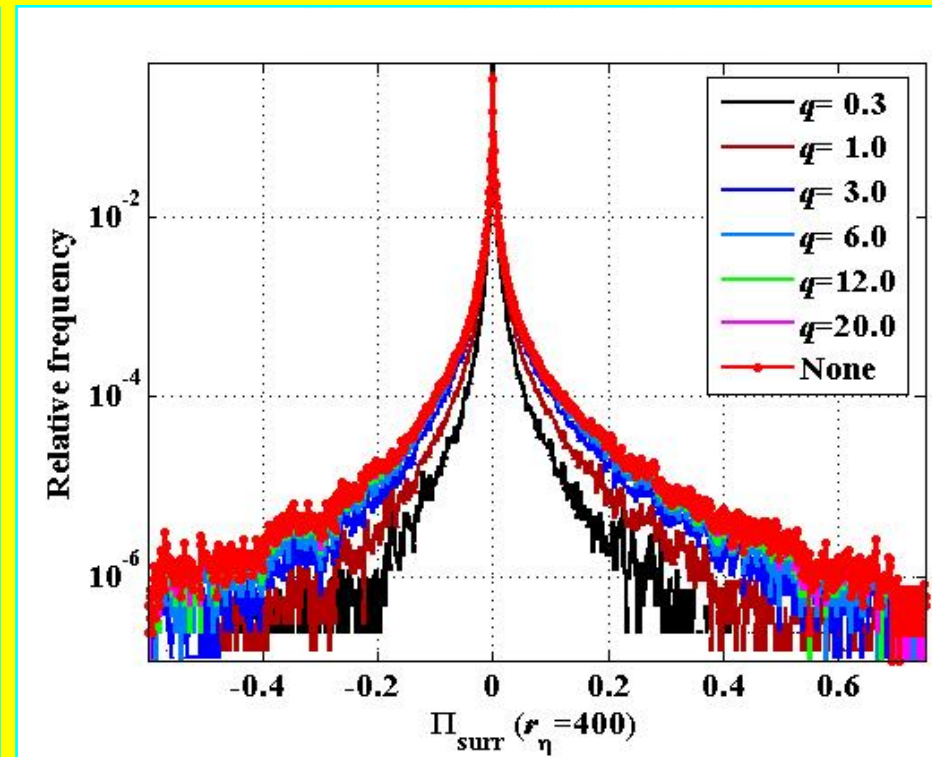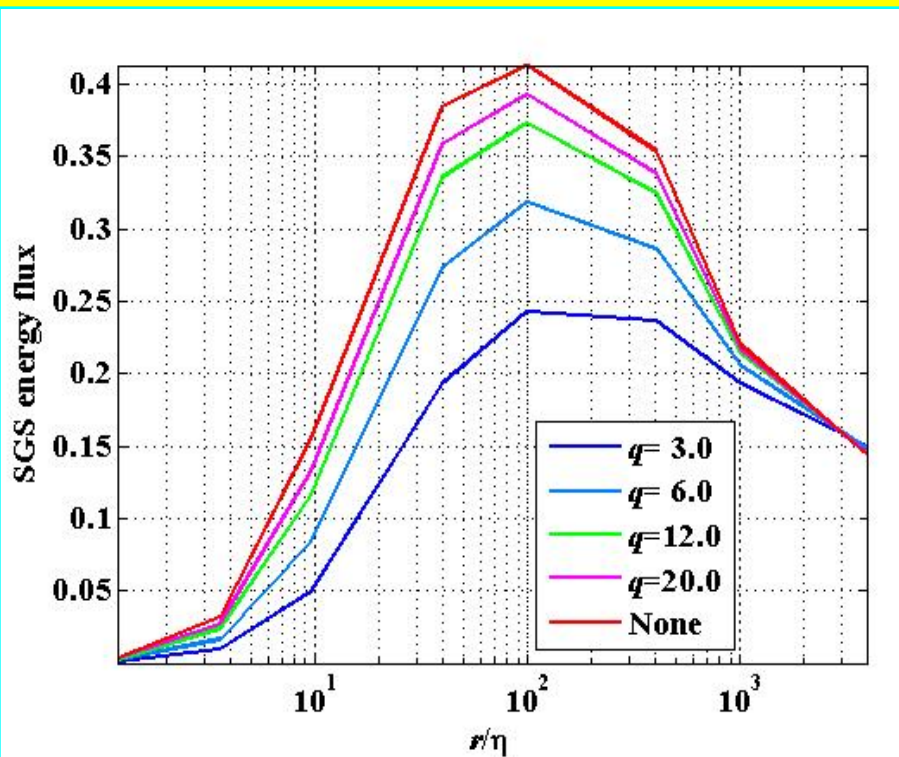


$S_3$ slopes, threshold $q_s$

Threshold $q_s$

# The subgrid scale energy flux $\Pi$

$$\Pi(x;r) = -\tau_{ik}\,[s_{ik}]; \quad \tau_{ik} = [u_i u_k] - [u_i][u_k]$$
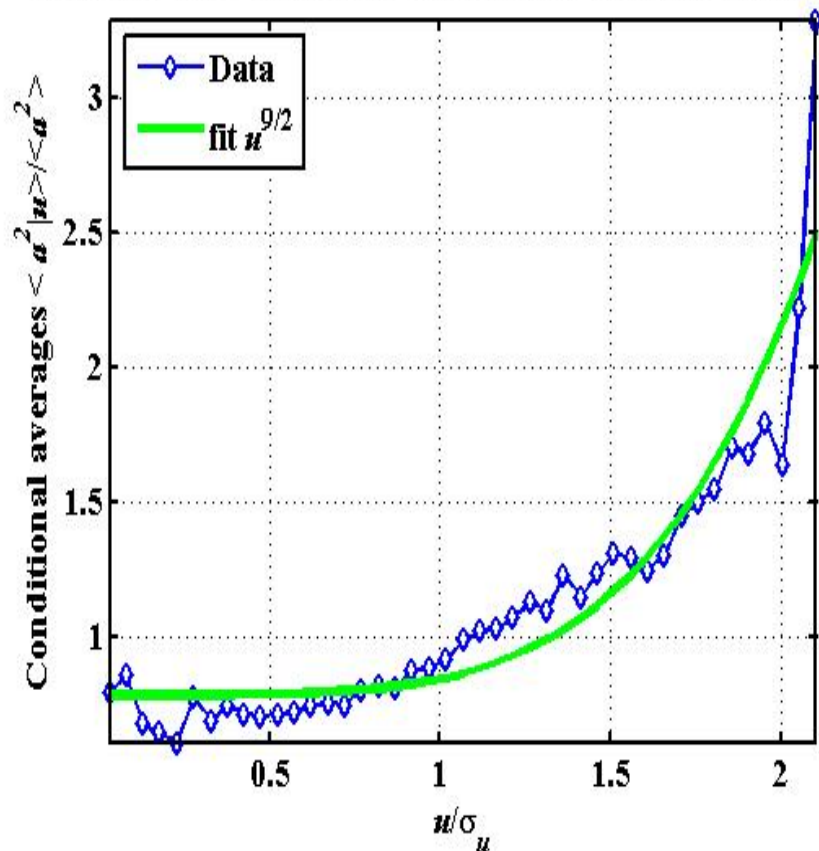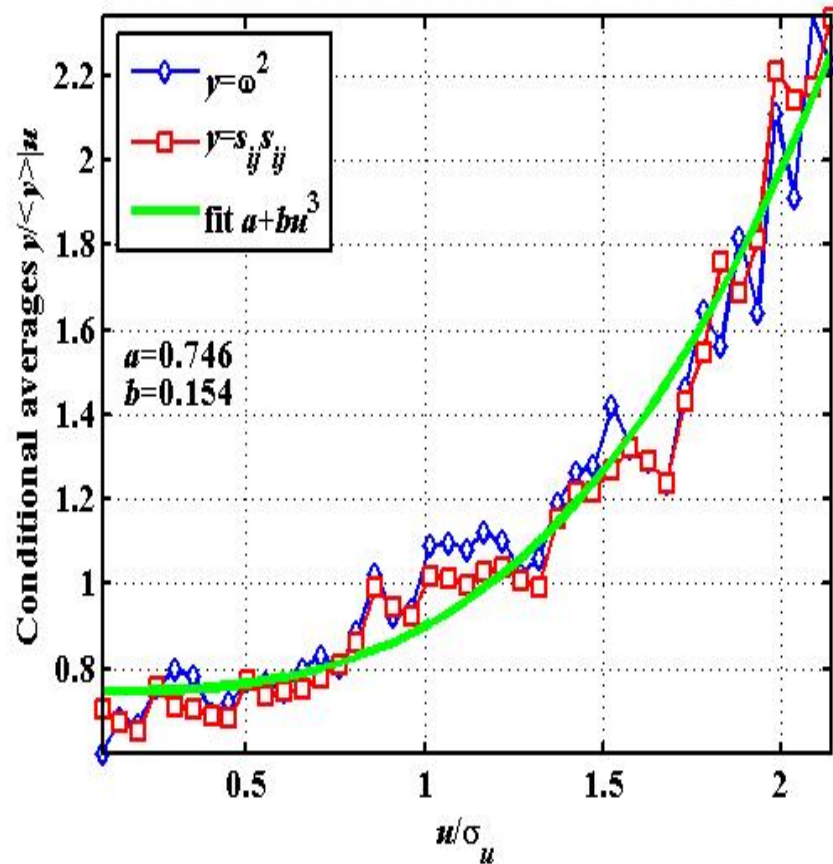
[...] - a Gaussian one-dimensional filter of width $r$

$\langle\Pi\rangle$            PDF

# Statistical dependence of small on large scales.

Enstrophy $\omega^2$, total strain $s^2$ and squared acceleration $a^2$ conditioned on magnitude of the velocity fluctuation vector, Field experiment, Sils-Maria, Switzerland, 2004, $Re_\lambda = 6800$ (Gulitskii et al. 2007, *J. Fluid Mech.,, 589,* )

# MAIN POINTS

**Based on data at $Re_\lambda \sim 10^4$ with access to the field of velocity derivatives including dissipation**

*Among the most exciting is the issue whether it is correct to neglect viscosity in the conventionally defined inertial range*

There is a substantial number of strong dissipative (!) events contributing significantly to the PDF of $\Delta u_i(r)$ in the conventionally defined inertial range (CIR) at high Reynolds numbers.

Thus the CIR is ill-defined in the sense that the statistics of $\Delta u_i(r)$ in the CIR is not independent of viscosity (in contrast with the 2nd Kolmogorov hypothesis).

Consequently, the dissipative range (CDR) is not well defined either.

In other words the CIR and CDR do not live separately "side by side", but e.g. strongly dissipative events are present and play an essential role throughout the whole CIR such as the "anomalous" scaling of CIR. Thus 'anomalous scaling' is not an attribute of CIR (and is not a manifestation of "IR intermittency" either). It is important that this is not the same as, e.g. "taking into account" the fluctuations of dissipation in the CIR.

Vice versa the properties of CDR depend on what happens in larger scales.

**Correlations after experiments done is bloody bad\*. Only prediction is science.** FRED HOYLE 1957, *The Black Cloud, Harper, N-Y.*

\*These are "postdictions"