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High order moment scaling laws in wall-bounded turbulent shear flows and beyond: A fact-check

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Abstract

A brief fact-check is presented to a seminar [INI, 23rd March 2022] recently held by M. Oberlack on symmetries and its applications to turbulence. The seminar was part of the programme “*Mathematical aspects of turbulence: where do we stand?*”, organised by the Isaac Newton Institute (INI) [↗](#). This fact-check will reveal important information that the speaker did not show or discuss. The conveyed impression of a flawless and well-tested scaling theory as presented is not true and is far from being the case. In a second part, all questions that were asked during and after the talk will be correctly answered in detail.

1. Fact-check of the presentation

Considered herein will only be the last three slides 59-61, where the speaker attempts to refute his approach against criticism. All the other slides he showed before are not new and have already been clearly refuted, once in 2020/21 [1, 2], and once again recently in [3].

Slide 59:

The results shown are highly trivial and do not prove anything that the presented symmetry-based scaling approach works. The reason why these results are trivial has been thoroughly discussed in [3] (see Sec. 2, especially footnote 2). For example, that the two scaling exponents σ_1 and σ_2 are almost the same and close to 2 has nothing to do with “intermittent scaling”. It only reflects the trivial fact that all even moments of U_2 and U_3 have a non-zero local extremum at channel center, like U_1 . And since these local extrema all start off quadratically, it is not surprising that we get a quadratic power-law scaling $(x_2/h)^2$ for all those moments close to channel center. Sure, the further away we go from channel center, the less it will be a pure quadratic scaling since the higher-order Taylor terms slowly start to kick in.

This structure of parallel lines in a log-log-plot with a constant slope can be trivially achieved with any function $f(x)$ which is non-zero at $x = 0$ (when presented in deficit form). See Fig. 1, where e.g. $f(x) = \cos(x)$ and its powers in deficit form up to order 6 is shown. With a suitable scaling, the curves can additionally either be evenly distributed or made to collapse. Close to $x = 0$, the slope is 2, since the cos-function starts off with a quadratic extremum.

In Fig. 2, the case $f(x) = \sin(x)$ is shown, which does not allow for such a structure of parallel lines as before, simply because $f(x)$ is zero at $x = 0$. In Fig. 3, however, $f(x) = \sin(x) + 1$ is shown, which works again. The slope close to $x = 0$ is now of course 1, since the sin-function starts off linearly.

Hence, what is shown on slide 59 is just trivial Taylor asymptotics around channel center, and not some special turbulent flow property.

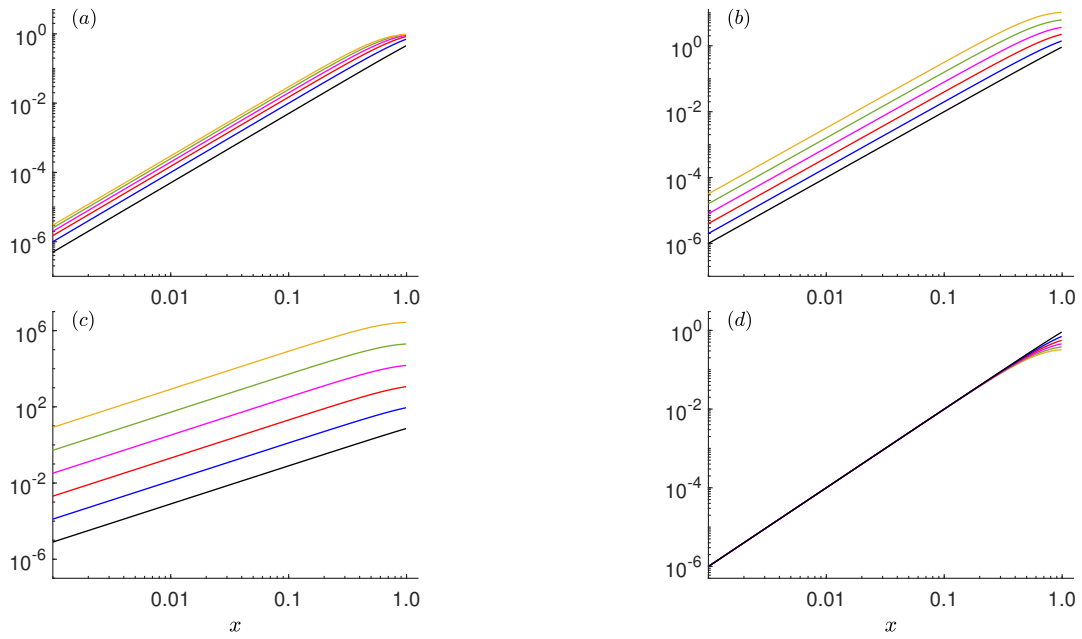


Figure 1: $c_n |f(x)^n - f(0)^n|$, $n = 1, \dots, 6$ (from bottom to top), for $f(x) = \cos(x)$ and normalization $c_n = 1$ (a), $c_n = 2^n/n$ (b), $c_n = 16^n/n$ (c), $c_n = 2/n$ (d).

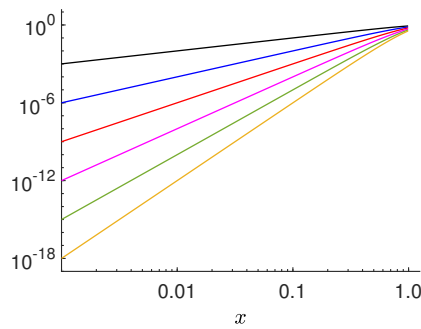


Figure 2: $|f(x)^n - f(0)^n|$, $n = 1, \dots, 6$ (from top to bottom), for $f(x) = \sin(x)$.

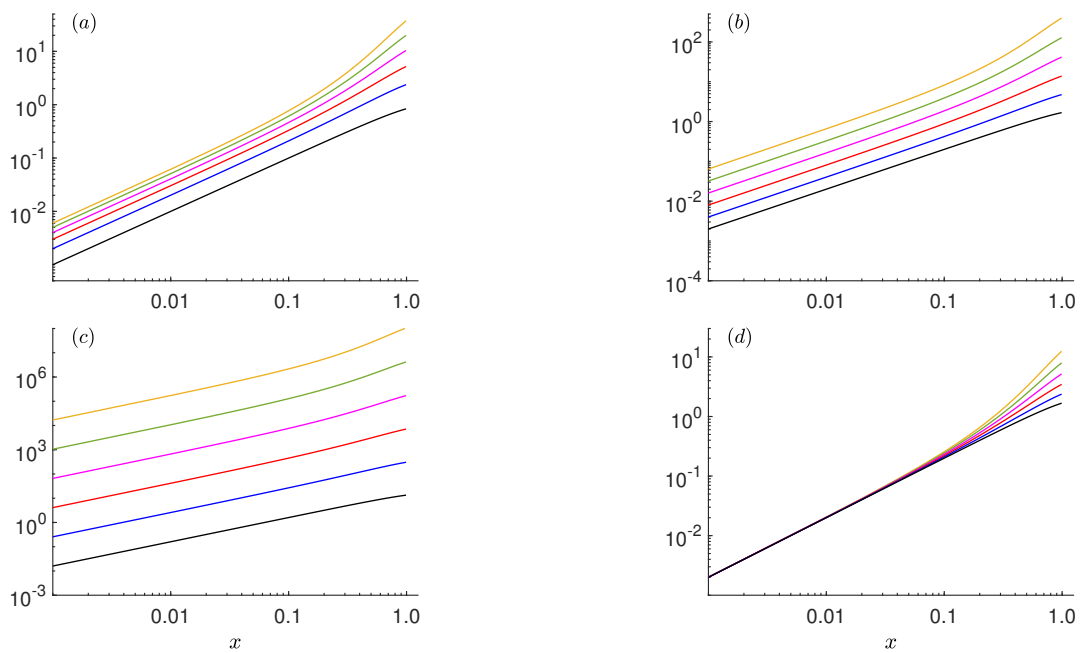


Figure 3: $c_n |f(x)^n - f(0)^n|$, $n = 1, \dots, 6$ (from bottom to top), for $f(x) = \sin(x) + 1$ and normalization $c_n = 1$ (a), $c_n = 2^n/n$ (b), $c_n = 16^n/n$ (c), $c_n = 2/n$ (d).

And, that the speaker only shows the diagonal moments on slide 59, and not the off-diagonal ones, is also clear now. Because, if he would show e.g. $\overline{U_1^n U_2}$, where his scaling law will again be the same as for any other moment (since his scaling is always isotropic), it will fail. Why? Because, since in channel flow we have $\overline{U_1^n U_2} = \overline{u_1^n u_2} + \sum_{k=1}^{n-1} \binom{n}{k} \overline{U_1^k} \overline{u_1^{n-k} u_2}$, they are simply zero at channel center (as $\overline{u_1^n u_2}$ are all odd functions), which means that a structure of parallel lines does not exist for this case (analogous to Fig. 2), and therefore cannot be “predicted” by Oberlack’s scaling theory, although it should, according to him, but in reality it does not.

What is also not shown to the audience is the scaling failure of the Reynolds stress $\overline{u_1 u_1}$ in that region, as shown in Fig. 3a in [3]. Although the streamwise full-field correlation $\overline{U_1 U_1}$ scales nicely as shown on slide 51, it fails when rewritten for the fluctuation moment $\overline{u_1 u_1}$. Why? Because here the mean field has to be subtracted again. That means, although the scaling close to channel center is trivial and has slope 2 (since $\overline{u_1 u_1}$ has a quadratic minimum at $x = 0$), Oberlack’s scaling fails.

Slide 60:

That the Reynolds-stress $\overline{u_1 u_1}$ scales in the log-layer as shown is not surprising. But it scales very unnaturally, because $\overline{u_1 u_1}$, which varies by order 1, has to be fitted with parameters of order 100. This kind of scaling was already shown by us in Fig. 3b in [3]. More interesting, however, is the observation at the next higher order, at $n = 3$, where an immense scaling error sets in, which for higher orders even worsens [3], but which, noticeably, is neither shown nor discussed.

There is a simple reason for this immense scaling failure, which was proven independently several times already in [4, 5], [6], [7], and as also explained again in [3]: Oberlack’s statistical “symmetries” are nonphysical in that they cannot be realized for the simple fact that they violate the classical principle of cause and effect between the fluctuations and the mean fields.

Important to note here is that this violation is suppressed and not visible when analyzing the scaling of the full-field correlations due to the overwhelming dominance of the mean field over the fluctuations in the flow regions considered, but becomes measurable and clearly visible when analyzing the corresponding fluctuation correlations, i.e., when subtracting the mean field from the full-field moments.

What the speaker also fails to mention here is the fact that the spanwise moments $\overline{U_3^n}$, which are equivalent to the spanwise fluctuation moments $\overline{u_3^n}$, lead to a contradictory scaling in the log-layer, as he himself proved in [8] (see Fig. 3 and its explanation at the end of Sec. 2). The reason for this failure is rooted in the simple fact that Oberlack’s scaling approach always leads to isotropic scaling, regardless of the length scale considered, which, of course, is a nonsensical result for a highly anisotropic flow as channel flow on large length scales he intends to study.

It’s telling to see how the speaker does not mention all this crucial information.

Slide 61:

The shown invariant PDF-result is based on the same flawed assumptions as his scaling laws. That they all can be recovered from a PDF proves nothing about their correctness. Once again, it is important to note that these flawed assumptions only become visible and measurable when bijectively rewriting to the fluctuation moments. Because, as already mentioned, for the full-field moments the error is suppressed, due to the overwhelming dominance of the mean field over the fluctuations.

To clearly contrast Oberlack’s inconsistent and non-realizable approach, a consistent and realizable symmetry-based scaling approach is presented in Sec. 4 in [3].

2. To all questions not properly or correctly answered by the speaker

1. “Are we looking here for uniqueness?” (27:32)

Anyone with a bit of experience in Lie-group symmetry analysis knows the basic fact (except for the speaker, seemingly) that for unclosed systems the strong concept of symmetry turns into the weaker concept of equivalence. That means, solutions are not necessarily mapped to solutions anymore. Also, when correctly performing a Lie-group symmetry analysis on an infinite chain of equations, it always will result to an infinite dimensional Lie-algebra. That means, the original closure problem of equations only shifts to a closure problem of symmetries. Nothing is gained here from the uniqueness point of view when using the Lie-group symmetry method, as everything remains arbitrary. Thus any desirable invariant function can be constructed with this method, and not only those few as always shown by the speaker. Ultimately this means that the choice of an invariance is made by the user and not dictated by theory. The speaker, however, incorrigibly keeps choosing the same two nonphysical and thus non-realizable statistical symmetries for almost a decade now, although an infinite amount of other possible choices is available — in Sec. 4 in [3] it is shown how to choose physical ones. See also Appendix A therein for a more detailed discussion.

2. “Additive symmetry is not Galilean symmetry?” (41:05)

Indeed, Oberlack’s additive statistical symmetry is not related to Galilean invariance in any form,¹ as was first proven in [5] (see Sec. IV). However, what the speaker then says about Galilean invariance is not true. Because fact is, channel flow is Galilean invariant in the streamwise direction, also in the stationary regime. See Eq. (4.3) in [3], which in Reynolds-decomposed form reads as (4.6), being a valid invariance for both the full unreduced statistical equations (4.4) as well as for the reduced ones (4.5) in the statistically stationary regime. It is therefore not true that one needs Oberlack’s statistical translation symmetry (which by the way is even unphysical) in order to generate a constant shift in the mean flow.

To obtain a consistent invariant scaling law for the mean field, the Galilean invariance is absolutely necessary, as shown in [3]. To then obtain a statistical translation invariance for all higher-order moments, it is surely not the way as the speaker proposes, since his approach is statistically not realizable. The correct way is given and explained in Sec. 4 in [3], by using a non-Gaussian white-noise process.

3. “Are the introduced symmetries also applicable to other flows?” (42:58)

No, because Oberlack’s two statistical symmetries are nonphysical per se. See e.g. Oberlack’s correction for the case of jet flow [9]. There, both his statistical scaling and translation symmetry do not apply. Specifically, his scaling invariance is not relevant and even gets broken for jet flow, while his translation invariance has to be put to zero to avoid contradictions in the fluctuation moments. In a later publication [10], he ignored and obscured these facts again by showing again only the misleading perfect scaling of the full-field moments, instead of the fluctuation moments, which clearly would have revealed that his approach is flawed.

Also this crucial information is not mentioned by the speaker. The audience is supposed to get the impression that everything presented works just fine. But that’s far from being the case.

¹For the common (deterministic) Galilean invariance, this non-relation is already immediately obvious for the second full-field moment $H_2 := \overline{U_1^2}$: While Oberlack’s translation symmetry is given by a constant shift $H_2^* = H_2 + a_2$, the Galilean induced invariance is given by a non-constant one, $H_2^* = H_2 + 2H_1c + c^2$, where c is the Galilean boost of the mean field $H_1 := \overline{U_1}$, i.e. where $H_1^* = H_1 + c$.

4. “Do the scalings also hold in 2D?” (45:18)

Yes, one will get the very same scaling laws in 2D as in 3D when using Oberlack’s approach. There will be no difference at all, simply because his symmetries are independent of the physical dimension. For example, the very same log-law in 2D as in 3D will be obtained, again with a *universal* von-Kármán constant, but which does not hold in 2D (see e.g. [11]).

The speaker mentions here that he has not solved the turbulence problem. But his recent PRL paper [12], however, reads differently, conveying the impression that he has solved it at least for the log-layer and the core region analytically by first principles.

5. “Does one need Oberlack’s symmetries to get the log-law for the mean flow?” (52:30)

No, one does not. One only needs Galilean invariance and the two scaling symmetries of the Euler equations (where the two Euler scalings have to be based on the same group parameter). That means, classical symmetries are already fully sufficient to get the log-law. Any new symmetries are not needed.

What the speaker is saying here is simply not true. Sure, if one consistently wants to include also higher moments in the analysis, then one has to proceed as shown in Sec. 4 in [3], and not in the way as the speaker is proposing, as it will only lead to inconsistencies later on.

Please note that a correctly applied Lie-group symmetry analysis does not necessarily lead to a log-law for the mean flow in the inertial region. It can also result to a power-law just as well, because when putting the translation group parameter for the mean flow to zero, the wall boundaries can be well implemented without breaking any scaling symmetry. As a result, a 2-parametric power-law for the mean flow in the inertial region is then obtained. See the last paragraph on p. 4, continuing on p. 5 in [3].

6. “Does Galilean invariance imply Oberlack’s statistical translation symmetry?” (53:36)

No, it does not. Oberlack’s statistical translation symmetry cannot be mathematically derived from Galilean invariance. Also from a physical point of view, they are totally unrelated. While Galilean invariance is a physical invariance that can be consistently applied to any order of the full-field moments, Oberlack’s statistical translation symmetry is a nonphysical invariance which cannot be realized by any means, simply because it’s violating the classical principle of cause and effect.

7. “Can the analysis tell at which location the scaling symmetry breaks?” (54:17)

No, it can not. The group parameters in Oberlack’s scaling approach are by construction all independent of position and Reynolds number. The symmetry breaking that the speaker allows for in the inertial region, is not dictated by his theory, but is done by him in an ad-hoc manner, just to get the log-law as a “symmetry-induced” result. As already said before (see answer to question 5 above), a correctly performed symmetry analysis also allows for the option that no scaling symmetry needs to be broken, because the wall boundary conditions can be consistently implemented without breaking any scaling symmetry.

However, if one wants to break the scaling at a specific location somewhere in the channel (a decision made by user and not dictated by theory), then there are many ways how one

can do that. For example, when taking the friction velocity u_τ as the symmetry-breaking parameter, then, as it was correctly asked, why not better to break the scaling in the inner viscous sublayer than in the outer inertial layer. Because the underlying assumption of inviscid Euler scaling (as used by the speaker) is definitely more appropriate further away from the wall than close to it. A symmetry breaking with u_τ should therefore rather occur more close to the wall, where viscous forces become relevant and where thus the assumption of inviscid Euler scaling no longer holds and breaks down. But when doing this, Oberlack’s approach will yield a non-valid result, namely a log-law in the viscous sublayer.

This example clearly shows again one of the weaknesses of a Lie-group symmetry analysis in turbulence: Although a physically meaningful conclusion can be made, it cannot be implemented without further ado. In other words, the Lie-group symmetry analysis in turbulence is a method that needs modelling, i.e. it’s just another common trial-and-error method, and not a first-principle approach as misleadingly claimed by Oberlack for more than two decades now.

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