Comment

Comment on ‘Conformal invariance of the zero-vorticity Lagrangian path in 2D turbulence’

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Abstract

The current claim by Grebenev et al (2019 J. Phys. A: Math. Theor. 52 335501), namely that the inviscid and unclosed 2D Lundgren–Monin–Novikov (LMN) equations on a zero-vorticity Lagrangian path admit conformal invariance, is based on a flawed analysis published earlier by Grebenev et al (2017 J. Phys. A: Math. Theor. 50 435502). All results and conclusions made before in the Eulerian picture were now extended by Grebenev et al to the Lagrangian picture. Although we have already commented on these errors in Frewer and Khujadze (2018 arXiv:1802.02490; 2021 J. Phys. A: Math. Theor. 54 438002) and consistently refuted their previous study, we deem it necessary to address and discuss these errors again in the new formulation and notation of Grebenev et al (2019 J. Phys. A: Math. Theor. 52 335501) as it will offer new insights into this issue.

Keywords: Lie groups, Lie symmetries, conformal invariance, symmetry breaking, probability density functions, integro-differential equations, turbulence

1. Introduction and a remark on the notation

The current publication [1] is flawed in the very same way as their previous one [2]. Their proposed analytical proof, namely that the (unclosed and non-modelled) PDF vorticity equations in the 2D inviscid turbulent flow case admit conformal invariance on a zero-vorticity characteristic, is false and misleading.

Despite the fact that the considered system of equations (2)–(4) in [1] for the considered case \( n = 1 \) (see section 3 in [1]) is unclosed and inherently would therefore allow for an unclosed set of invariances by itself, this system does not admit conformal invariance, neither in the

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Eulerian nor in the Lagrangian formulation, and this irrespective of whether a zero-vorticity characteristic is considered or not.

Note that when in the following all equation, section and page numbers in the present text appear as black, they refer to [1], while all in blue refer to this comment. Further note that when comparing the results between [1, 2], each is based on a different notation. The variables and functions

\[ x^1, x^2, x'^1, x'^2, \xi^0, \xi^1, \xi^2, \xi^3, \xi^4, \xi^5, \xi^6, \eta^0 = \eta_{f1}, \eta' = \eta_{f2}, b^0, b', \]

used in [2], and likewise in our comments [3, 4], were renamed in [1] to

\[ x, y, x', y', \xi', \xi'', \xi', \xi'', \xi', \xi'', \eta', \eta'', b_1, b_2, \]

respectively. Also note here that \( \eta^1, \eta^2, b_1, b_2 \) in [1] do not correspond to \( \eta^1, \eta^2, b_1, b_2 \) in [2]. They are different functions: while the former ones directly refer to the symmetry solutions of \( f_1 \) and \( f_2 \), the latter ones only refer to the symmetry solutions of the auxiliary (non-local) functions \( J_1 \) and \( J_2 \) as defined in [2], which then in turn defines the invariant transformations of the local functions \( f_1 \) and \( f_2 \). Furthermore, the composite variable \( y = (x, \omega, x', \omega') \) as defined in [2] is not used anymore in [1].

2. A closer look on the actual analysis performed in [1]

To justify their analysis in [1], the authors make the following three consecutive statements on p 2:

(a) Based on the results obtained in [2], they claim ‘that the infinite Lundgren–Monin–Novikov (LMN) hierarchy truncated to the first equation, i.e. considering the evolution of the one-point probability density function (PDF) for \( f_1(x_1, \omega_1, t) \), is conformally invariant on the zero-vorticity characteristic equation, though limited to the inviscid case’.

(b) Result (a) was obtained because they ‘performed a group classification of the equation for \( f_1 \) with respect to the parameter \( \omega_1 \) and proved that for \( \omega_1 = 0 \) there exists an extension of the symmetry group, which is exactly the conformal group (CG)’.

(c) Employing then result (b), ‘the conformal invariance of the \( f_1(x_1, \omega_1, t)|_{\omega_1=0} \)–equation implies invariance of the characteristic equation’.

These three statements summarize the argumentative strategy in [1]: while statement (b) is declared as a valid assumption, since it is directly based on the results obtained in [2], the third statement (c) was proven in section 4 in order to re-validate again the statement made in (a).

However, statement (b) we have already clearly refuted in our previous comments [3, 4]. Hence, statement (b) cannot be used as a valid assumption to prove (c). Although this proof itself, as presented in section 4, is technically correct, in that it correctly shows that if the PDF equation on a zero-vorticity isoline, i.e., if the equation for \( f_1(x_1, \omega_1, t)|_{\omega_1=0} \) is conformally invariant, then, of course, the characteristic equations (11) and (12) are invariant too. This is not the problem. The problem is that their proof of (c) is based on the incorrect initial assumption that the \( f_1(x_1, \omega_1, t)|_{\omega_1=0} \)–equation is conformally invariant.

But fact is that the \( f_1(x_1, \omega_1, t)|_{\omega_1=0} \)–equation is not conformally invariant. Statement (b) is therefore an invalid statement as we have clearly shown in [3, 4]. Still it is declared as a valid statement and essentially summarizes section 3 in [1]: the results shown therein are exactly the same from [2] we already refuted. No new arguments or a new invariance analysis is presented.
This section is just a direct copy of the key results obtained in [2], only up to a different notation we mentioned before in section 1.

The final but incorrect result in section 3 in [1], namely the infinitesimal invariance operator (45), now enters the proof in section 4 as an assumption. Switching to the complex plane, it is now shown that the characteristic equations (11) and (12), which in the complex formulation turn to the single equation (19), and then finally to (46), admits this invariance given by (45). But, as already said, this proof is invalid because it is based on the incorrect result (45).

It should be clear here that it is not the system of characteristic equations (11) and (12) itself which breaks the conformal invariance, but only the evolution equation (2) of the PDFs $f_n$, along with their constraint (4), which breaks it. And since these PDFs are necessary to evaluate the terms (14) and (15) for the evolution equations of the characteristic curves (11) and (12), they thus do not show this invariance, irrespective of whether a zero-vorticity isoline is considered or not. In other words, the crucial invariance breaking constraints which arise are because of the existence of (2)–(4), and not because of (11) and (12).

To be as unambiguous as possible, let us repeat this conclusion once again in different words: since the characteristic equations (11) and (12) are by definition directly dependent on the dynamics of the PDFs through relations (14) and (15), they cannot be considered as stand-alone equations. They are directly coupled to the equations for the PDFs (2)–(4). Therefore, if we want to perform a consistent invariance analysis on the characteristic equations (11) and (12), we have to consider the full system of coupled equations, namely (11)–(12) and (2)–(4) simultaneously, and not (11)–(12) from (2)–(4) separately. But now, since in this coupled system the equations for the PDFs (2)–(4) do not admit conformal invariance, then neither do the characteristic equations (11)–(12). And again this is true irrespective of whether a zero-vorticity isoline is considered or not.

As a side note it is worthwhile to point out that their unequivocal formulation of the above statement (b) explicitly confirms in fact our conclusion that in [2] no invariance analysis on a dimensionally reduced set of ordinary differential equations was performed. Instead, as said in (a) and (b), only an invariance analysis on the LMN-truncated and thus dimensionally non-reduced equation for $f_1$ was carried out, where in the end the results were just restricted to a zero-vorticity isoline $f_1|_{\omega_1=0}$. And it is exactly this system we have proven in [3, 4] that it does not admit conformal invariance.

3. Further points for correction in [1]

This section lists all further incorrect or misleading statements made in [1], for which we either provide a correction (if possible) or briefly explain why a correction is necessary in each case.

3.1. The issue of the normalization constraint

Their statement in section 4.1 on p 12 that ‘the invariance of the normalisation conditions (4) under the action of the group $G$ (25)–(31), (41) and (42) is evident and was derived before in [1]’ is not true, and its falsity can be easily proven, even in the rationale of [1]. To prove this, we will proceed in the following only with the information and reasoning as it is provided and presented in [1], to demonstrate that if the authors would have done a thorough transformation of the normalization condition (4) along the lines of their own reasoning, they would have immediately realized that the conformal invariance need to get broken in order to have compatibility with (4). But such a thorough transformation on (4) has not been done by them,
and therefore they miss this crucial fact. Here we provide this analysis, forming parts of our complete survey already given in [3, 4].

In particular, we will now demonstrate that when transforming already the very first normalization condition in (4) for \( n = 1 \), then it does not stay invariant under the group \( G \) as proposed in [1], which according to them consists only of the elements (25)–(31), (41) and (42). We start by asking that if this normalization condition is valid in the new transformed variables

\[
\int d\omega^* f_1^* = 1 \quad \iff \quad 0 = 1 - \int d\omega^* f_1^*,
\]

would it then stay invariant when transforming it back to its old variables? The \(^*\)-symbol above denotes the new variables which are connected to the old variables via the infinitesimal group transformation according to (30) and (41) as given in [1]

\[
\omega^* = \omega + \epsilon \cdot \xi^\omega + \mathcal{O}(\epsilon^2), \quad f_1^* = f_1 + \epsilon \cdot \eta^1 + \mathcal{O}(\epsilon^2),
\]

where \( \epsilon \ll 1 \) is the infinitesimal group parameter. Since (3) is a non-local relation in the variable \( \omega \) to be transformed, we obviously need a transformation rule (4) for \( \omega \) which is valid for all \( \omega \in \mathbb{R} \), simply because (3) sums over all values of \( \omega \) without any exceptions. Hence, we need a \( \xi^\omega \) which is valid for all \( \omega \in \mathbb{R} \). Using only the information provided in [1], as notably compiled on p 7, the infinitesimal \( \xi^\omega \) can thus only be of the form

\[
\xi^\omega = \begin{cases} 
6c^{11}(x) \cdot \omega, & \text{for } \omega = 0, \\
c \cdot \omega, & \text{for } \omega \neq 0, \ c \neq 0,
\end{cases}
\]

where \( c \) is some arbitrary constant. The function \( c \cdot \omega \), for the case \( \omega \neq 0 \) in (5), is a direct consequence of result (30)\(^3\) when restricted by (24)\(^4\), such that \( \xi^\omega \) is independent of the spatial coordinates \( x = (x, y) \), when \( \omega \neq 0 \). Note that \( \xi^\omega \) (5) is continuously differentiable at \( \omega = 0 \), i.e., \( \lim_{h \to 0} (\xi^\omega|_{\omega=0+h} - \xi^\omega|_{\omega=0})/h = \xi^\omega|_{\omega=0} \) exists, with \( \lim_{\omega \to 0} \xi^\omega = \xi^\omega|_{\omega=0} \), and therefore the group element \( \xi^\omega \) is not violating the smoothness-axiom of Lie-groups. Hence, for all \( \omega \in \mathbb{R} \) the \( \omega \)-derivative of (5) reads

\[
\xi^\omega \omega|_{\omega=0} = c, \quad \forall \omega \in \mathbb{R},
\]

which, of course, also includes the derivative at \( \omega = 0 \), which again explicitly reads

\[
\xi^\omega \omega|_{\omega=0} = \lim_{h \to 0} (\xi^\omega|_{\omega=0+h} - \xi^\omega|_{\omega=0})/h = \lim_{h \to 0} (\xi^\omega|_{\omega=0+h} - \xi^\omega|_{\omega=0})/h = \lim_{h \to 0} \frac{c \cdot (0 + h) - 6c^{11}(x) \cdot 0}{h} = c.
\]

In the rationale of [1], the correct infinitesimal transformation rule to transform (3) is therefore given by (4), where \( \xi^\omega \) is given by (5) along with (6), and \( \eta^1 \) by (41), which has the structural form: \( \eta^1 = -6c^{11}(x)f_1 - (C_1 + C_2)f_1 + b_1(x, \omega, t) \). Now, let us transform (3) exactly

\(^3\)Note that (30) is, as declared in [1], the result when solving the non-local equations without any normalization. A detailed explanation is given in [2]—see e.g. top of p 8.

\(^4\)Constraint (24) is continuously valid for all \( \omega \in \mathbb{R} \) without any exceptions [3, 4].
according to this rule and see what happens:
\[
0 = 1 - \int d\omega^* f_1^* = 1 - \int d\omega \left( \frac{\partial f_1^*}{\partial \omega} \right) (f_1 + \epsilon \eta^1 + \mathcal{O}(\epsilon^2))
\]
\[
= 1 - \int d\omega \left( 1 + \epsilon \xi^\omega \right) (f_1 + \epsilon \eta^1) + \mathcal{O}(\epsilon^2)
\]
\[
= 1 - \int d\omega \left( f_1 + \epsilon (\eta^1 + \xi^\omega f_1) \right) + \mathcal{O}(\epsilon^2)
\]
\[
= 1 - \int d\omega \left( f_1 + \epsilon \left( -6c^{11}(x)f_1 - (C_1 + C_2)f_1 + b(x, \omega, t) + cf_1 \right) \right) + \mathcal{O}(\epsilon^2)
\]
\[
\int = 0
\]
\[
= \left( 6c^{11}(x) + C_1 + C_2 - c \right) \int d\omega f_1 = \int d\omega b_1(x, \omega, t) + \mathcal{O}(\epsilon)
\]
\[
= (C_1 + C_2, \text{ see (44)}^5)
\]
\[
= \left( 6c^{11}(x) - c \right) + \mathcal{O}(\epsilon), \quad (8)
\]
which can only be satisfied if
\[
6c^{11}(x) - c = 0, \quad \forall x \in \mathbb{R}^2, \quad (9)
\]
that is, if and only if $c^{11}$ is a constant for all coordinates $x$, which of course breaks the conformal invariance. Hence, we obtain the correct final result that the normalization condition (4) is only compatible to the considered symmetry group $G$ if it contains a spatially independent function $c^{11}$, which of course is completely opposite as to what is claimed in section 4.1 on p 12 in [1] by wrongly allowing for a spatial dependence in $c^{11}$.

Note that it is not a minor issue that the normalization condition (4) breaks the conformal invariance, e.g., by saying then let us ignore the normalization condition from the system in order to restore this invariance. The normalization condition cannot be ignored, because it is an internal condition that guarantees that any PDF solution $f_\omega$ physically valid during evolution, or as [1] correctly puts it: "physically meaningful fields satisfy the properties (4-6)" [section 4.1, p 12].

In other words, if an invariance operator for the PDF system, as given in [1] by (2), is not compatible to the normalization condition (4), then physical solutions can get mapped to unphysical ones. Therefore the normalization is an important ingredient in any PDF system and should be respected within a symmetry analysis. The same is true for all other internal constraints that go along with such a PDF system, all necessary to ensure physical PDF solutions. In this regard, please see our supplementing comment [5], which criticizes an even earlier publication by Grebenev [6], in that new invariance groups get proposed therein which

5 The second integral result in (44) in [1] misses the constant $C_1$ next to $C_2$. The problem is that the authors distributed the constant $C_1$ in their new version differently than in their earlier version, where $C_1$ was also associated to the solution of $\xi^\omega$ (see result (38) in [2]), while in the new version not. Nevertheless, all the different redistributions of these constants have no effect on the proof given above. In our comment [3, 4] we even perform this proof (8) on a more general basis, since a complete and thorough symmetry analysis shows that $C_1$ and $C_2$ may also depend on $\omega$, with the consequence then that they may not be pulled in front of the $\omega$-integrations anymore as it was done in (8).

But also with this generalized proof (see section 2.2 therein) we come of course to the same conclusion in that the proposed conformal symmetry is not compatible with the normalization condition.

6 Note that (9) could already have been obtained by taking the naive $\omega$-derivative of $\xi^\omega$ (5), with result $\xi^\omega = 6c^{11}(x)$, for $\omega = 0$, and $\xi^\omega = c$, for $\omega \neq 0$, and then by enforcing the Lie-group axiom of continuity to $\xi^\omega$ at $\omega = 0$. 

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obviously are not compatible to the full PDF system when including all internal constraints, i.e., ultimately, in [6] non-physical symmetries are getting proposed.

**Remark:** Regarding the proof of this section, please also see our follow-up publication in arXiv, where we discuss this proof once again, since it has been misrepresented in the Reply by Grebenev et al.

### 3.2. The issue of the zero-vorticity constraint

The following statement in [1] on p 8 that ‘these relationships [(40)] explicitly demonstrate the exceptional role of the zero-vorticity constraint \( \omega = 0 \) to guarantee that \( c_{11} \) and \( c_{22} \) are non-trivial functions and the CG appears for \( \omega = 0 \)’ is false. In section 2.3 in [3, 4] we clearly demonstrate that the choice \( \omega = 0 \) is not exceptional at all, because the obtained invariances can always be equivalently re-formulated such that any arbitrary but fixed value of \( \omega \) will do the same job as the particular choice \( \omega = 0 \)—see therein particularly our result (2.28) in [3], or (58) in [4], for an alternative \( \xi \omega \) and its subsequent discussion. Hence, opposite to their claim, the choice of a zero-vorticity constraint \( \omega = 0 \) plays no exceptional role, resulting even in the fact that the proposed conformal invariance is not only broken for \( \omega = 0 \), but for any \( \omega \in \mathbb{R} \), thus refuting [1, 2] in its most general form.

### 3.3. The issue of the global group

On p 9 in [1], the global form (49)–(54) of the local conformal generator \( S \) (45) is presented. The problem hereby is that this representation (49)–(54) is argued to be valid on any 2D-domain for all possible local generators \( \xi^x \) and \( \xi^y \) in (45) when only restricted to be harmonic conjugates on that same domain. However, as is well-known, one carefully should distinguish in 2D between those conformal transformations which are local and those which are global. Because the global ones do not have a global inverse, and therefore do not form a group, except for only one, the projective conformal (Möbius) group which bijectively maps the Riemann sphere onto itself. Hence, strictly speaking, there is only a single true conformal global group in two dimensions—see e.g. [7] for a more detailed discussion. Hence a global analysis as initiated in section 4 in [1] may not be generally valid, but has to come with restrictions to certain sub-domains where those arguments may be valid. In particular when constructing global inverses, as the attempt to invert (49)–(54), special care should be taken, as these transformations may not exist.

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7 Equivalence transformations can be successfully applied for example to classify unclosed differential equations according to the number of symmetries they admit when specifying the unclosed terms (see e.g. [9–14]). A typical task in this context sometimes is to find a specification of the unclosed terms such that the maximal symmetry algebra is gained. Once the equation is closed by a such a group classification, invariant solutions can be determined. But in how far these equations and their solutions are physically relevant and whether they can be matched to empirical data is not clarified a priori by this approach, in particular if such a pure Lie-group-based type of modelling is performed fully detached from empirical research. In this regard, special attention has to be given to the unclosed statistical equations of turbulence as considered herein, since the unclosed two-point PDF in (2)–(6) in [1] for \( n = 1 \), is only an analytical and theoretical unknown, but not an empirical one since it is fully determined by the underlying deterministic Navier–Stokes equations, which again are well-known for to break statistical symmetries in turbulence within intermittent events (see e.g. [15]). Hence extra caution has to be exercised when employing a pure symmetry-based modeling to turbulence.

8 The equivalence transformation (42) (for \( c_{11}^1 = c_{11}^2 = 0 \) is physically realizable only if the transformed field \( f_z^* \) can be generated as a PDF-solution of the deterministic Navier–Stokes equations according to its transformation rule \( f_z^* = f_z + \epsilon \cdot \eta^z + O(\epsilon^2) \), where we assume that the non-transformed field \( f_z \) already constitutes a PDF-solution. In other words, if \( f_z^* \) cannot emerge dynamically from \( f_z \) via the deterministic and thus closed Navier–Stokes equations, then the equivalence (42) (for \( c_{11}^1 = c_{11}^2 = 0 \) is nonphysical. To prove whether this equivalence is physically realizable or not, is beyond the scope of this article. However, there are a few examples of statistical Navier–Stokes equivalences which are clearly nonphysical—see e.g. [5, 16–19].
4. Final remarks

R1. It should be clear that our comment did not question the (possible) existence of conformal invariance in 2D turbulence, as e.g. indicated in [8]. What is criticized and refuted herein is only the algebraic derivation by Grebenev et al and their simplifying idea that the conformal invariance group would naturally arise from the first-order unclosed PDF-formulation of the 2D (inviscid) Navier–Stokes equations when only analyzing these by means of a classical Lie-group symmetry approach. In fact, as we have proven, their algebraic derivation for conformal invariance of the 2D LMN vorticity equations is flawed in both the Eulerian [2] as well as in the Lagrangian picture [1].

R2. Important to note in this overall discussion is that all invariant transformations put forward in [1] are only equivalence and not true symmetry transformations, simply due to that we are dealing here with an unclosed system of equations (2) and (3), where, for \( n = 1 \), the dynamical rule of the two-point PDF \( f_2 \) is not known beforehand. In contrast to a true symmetry transformation, which maps a solution of a specific (closed) equation to a new solution of the same equation, an equivalence transform acts in a weaker sense in that it only maps an (unclosed) equation to a new (unclosed) equation of the same class.\(^7\)

Of course, it is trivial and goes without saying that if once a real solution for \( f_2 \) is known, and if the equivalence (42) itself (for \( c_1^{11} = c_2^{11} = 0 \)) is physically realizable,\(^8\) then this equivalence turns into a symmetry transformation and \( f_2 \) gets mapped to a new solution according to the rule \( f_2^* = f_2 + \epsilon \cdot \eta + O(\epsilon^2) \). But since this is not the case here, any valid invariant transformation in (24)–(45) (for \( c_1^{11} = c_2^{11} = 0 \)) will thus at this stage only map between equations and not between solutions, where \( f_2 \) then is the unknown source or sink term, or collectively the unknown constitutive law of these equations.

Hence, even for all invariant transformations that still remain valid in [1], we cannot expect any information about the inner solution structure of the one-point PDF equation as long as the dynamical equation for the two-point PDF \( f_2 \) is not modeled. Without empirical modeling it is clear that the closure problem of turbulence cannot be circumvented by just employing the method of a Lie-group symmetry analysis. For more details on this issue, see e.g. [16, 20–22] and the references therein.

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