A review of "Lewis Fry Richardson Medal Lecture - How many modes are needed to predict climate bifurcations? : Lessons from an experiment" by Dubrulle et al., NPG-2021-19

General comment

This manuscript focuses on a controversial and challenging question: *what can we learn about climate dynamics from a laboratory fluid experiment?*, and it claims that there is a lot to learn! This optimistic answer is argued using a given generality of dynamical systems, more precisely stochastic attractors, and turbulence phenomenology, namely cascades and finally multifractal intermittency. Moreover, this approach is presented in a nice and elegant way. It certainly helps readers to emerge smoothly within the text, despite some sharp theoretical corners that remain beyond the present paper considerations.

The paper is presented as a review, which is basically true as far as the work of the team and its laboratories is concerned. However, to my opinion, it would gain from taking into account the earlier findings of other teams although they did not start from a laboratory experiment, but, for example, from the stochastic interactions between weather and climate, both analysed in a multifractal way.

A certain number of clarifications seem necessary, in particular on the pivotal issue of the energy flux conservation. A subsequent question is whether all the conclusions would hold in the absence of energy flux conservation. Below is a series of detailed comments and some suggestions which will hopefully be useful for a revision of this challenging manuscript.

Detailed comments and suggestions

Title

It is surprising to see « Lewis Fry Richardson Medal Lecture » in the title, at least because the lecture was given by the Medalist, whereas the paper is cosigned by 4 other persons (see suggestions for L16-28 below).

Affiliations

Affiliations 1 and 2 are identical.

L16-28:

This paragraph should be rewritten slightly to make it clear that this article is based on the 2021 Lewis Fry Richardson Medal Lecture presented by B. Dubrulle (avoiding to put it in the title) and that both are the result of personal intuitions and collective work.

L75-78:

The current "astronomical number" of degrees of freedom is obtained in a 3D isotropic framework, while even larger number of degrees of freedom ($N \approx 10^{27}$) has been obtained in an anisotropic framework (e.g., Schertzer and Lovejoy, 1991), *a priori* much more relevant for the atmosphere.

L89-93:

It should be mentioned that the eddy viscosity has been known since at least Richardson (1926) to be highly scale-dependent, so the quoted estimates are only relevant at given scales that need be

specified. Moreover, the action of small scales on larger ones is not limited to eddy viscosity with the presence of a beating / backscatter / renormalized forcing term (e.g., Forster et al, 1977; Frisch et al., 1980; Herring et al. 1982, and references therein).
The introduction of an eddy viscosity is far from being a "procedure [which] may sound foolish and bound to failure", whereas it corresponds to a not so sophisticated renormalisation of the Green function that is indispensable to prevent accumulation of energy at the smallest explicit scales and the resulting explosion of the model (see references above).
However, it is far from being sophisticated enough, particularly with regard to the necessity to also renormalise forcing and intermittency. So there is no reason to claim that the resulting simulations are that close to reality. In particular, without providing the objective metrics that have been developed to measure it. (Minor note: the XXX surrounding the reference Flato et al., 2013 must be suppressed).

Fig2 caption and other places
It would be important to clarify that what is called "angular momentum" is *in fact* its projection on the vertical axis oriented from bottom to top.

L145-152
It might be useful to give more specific names to $O(2)$ and $SO(2)$, e.g. "(general) orthogonal group" and "special orthogonal group" respectively, instead of just the generic name "symmetry group" for both, as well as to recall that $SO(2)$ is the (connected) component of $O(2)$ whose elements have +1 determinants, the other component having determinants -1. This can be particularly useful in understanding the reduction of $O(2)$ into $SO(2)$ with the breakdown of rotational symmetry $\mathscr{R}_\pi$.

L242
It would be useful to make the mentioned stochastic Duffing attractor much less mysterious, for exemple, that it results from the coupling of the periodic forcing of the classical deterministic Duffing attractor (2D) with a Langevin equation (1D).

L323-325
The figure reference should point to Fig.11b, not Fig.12b.
$C(h)$ is rather the (statistical) co-dimension of the support of the singularity h than a multifractal spectrum (Halsey et al., 1986), which originally denoted the (geometrical) dimension $f(h)= 3-C(h)$ introduced by Parisi and Frisch (1985), where $C(h)$ is then constrained to be ≤ 3 (in a 3D embedding space). On the relations between dimension and co-dimension formalisms see, for instance, Schertzer and Lovejoy (2011) and references therein. In particular, singularities h such that $C(h) > 3$ are almost surely not observable on a unique sample (Hubert et al. 1973).
The fact that the minimum of $C(h)$ occurs at $h_0 = 0.35 \approx 1/3$ is rather related to a weak intermittency of the mean energy flux, more precisely to a low co-dimension C_1 of the later: $C_1 = 3h_0 - 1 = 0.05$ with both the assumptions of log-normality and conservation of the energy flux, i.e., the strict scale invariance of the mean energy flux density $\varepsilon_\ell \approx \delta_\ell u^3/\ell \approx \ell^{-\gamma}; \gamma = 1 - 3h$. However, this estimate strongly disagrees with the other estimate $C_1 = 1$ obtained from $C(0)=1$, which belongs to the empirical parabolic fit $C(h)$ plotted in Fig.11b. This is because this adjustment supports the first assumption, but not the second.
Indeed, when obtained from the co-dimension $c_\varepsilon(\gamma)$ of a conservative flux: $c_\varepsilon(\gamma) = (\gamma + C_1)^2/4C_1$, the function $C(h)$ depends on the unique parameter C_1, whereas here it requires two independent parameters (horizontal shift and rescaling) to obtain a vertically orientated parabola. Although there is no obvious theoretical reason compelling that the points (0.35, 0) and (0,1) belong to the curve

$c(h)$, this seems to be supported by the empirical extrapolation. Furthermore, the empirical fit is precisely in agreement with the conservation of the statistical moment of order $q_1 \approx 0.18$ instead of $q = 1$ and all statistical moments of order $q > q_1$ diverge with increasing resolution, including the mean flux ($q = 1$). The latter corresponds to the estimate $h \approx 0.17$ given by the authors as the smallest measurable singularity.
I think the current failure to conserve the energy flux needs to be stated clearly and whether it is a system or a model failure needs to be clarified. A similar clarification should be brought on the impossibility to measure singularities h lower/ γ higher than those of the mean flux, as well as on the nature of this impossibility.

L328-333
It is important to distinguish the energy flux Π_ℓ from its density ε_ℓ (i.e. it is not just a terminology issue): Π_ℓ results from the 3D integration on a volume ℓ^3 of its density ε_η at much smaller scales $\eta << \ell$. Strong fluctuations of ε_η may induce multifractal transitions that cause flux scaling deviates from the naive/dimensional scaling $\ell^{3-\gamma}$ (for instance, Schertzer and Lovejoy (2011) and references therein).

L335-360
Regarding the above discussion on the conservation of the mean energy flux, the "computational nightmare" currently envisaged by the authors seems to be:
- either overoptimistic if the mean energy flux is effectively conserved. Indeed, the authors bound below the necessary range of explicit scales by $\eta_{h=h_{min}}$, which only guarantees the presence of singularities that are almost surely present on a unique sample, not the rarest ones that are generated by extreme events;
- or over-pessimistic if the mean energy flux is not conserved: the singularity h_{min} will be not actually reached.

Conclusions
...

References

Frisch, U., Lesieur, M. and Schertzer, D. (1980) 'Comments on the quasi-normal Markovian approximation for fully-developed turbulence', Journal of Fluid Mechanics, 97(1), pp. 181–192.
Forster, D., Nelson, D. R. and Stephen, M. J. (1977) 'Large-distance and long-time properties of a randomly stirred fluid', Physical Review A, 16, pp. 732–749. doi: 10.1103/PhysRevA.16.732.,
Halsey, T. C. et al. (1986) 'Fractal measures and their singularities: the characterization of strange sets', Physical Review A, 33, pp. 1141–1151.
Herring, J. R. et al. (1982) 'A comparative assessment of spectral closures as applied to passive scalar diffusion', Journal of Fluid Mechanics, 124, pp. 411–437. doi: 10.1017/S0022112082002560.
Hubert, P. et al. (1993) 'Multifractals and extreme rainfall events', Geophysical Research Letters, 20(10), p. 931. doi: 10.1029/93GL01245.
Parisi, G. and Frisch, U. (1985) 'On the singularity structure of fully developed turbulence', in Ghil, M., Benzi, R., and Parisi, G. (eds) Turbulence and predictability in geophysical fluid dynamics and climate dynamics. Amsterdam: North Holland, pp. 84–88.
Richardson, L. F. (1926) 'Atmospheric diffusion shown on a distance-neighbour graph', Proc. Roy. Soc., A110, pp. 709–737.
Schertzer, D. and Lovejoy, S. (1991) 'Nonlinear geodynamical variability: multiple singularities, universality and observables', in Schertzer, D. and Lovejoy, S. (eds) Non-linear variability in geophysics: Scaling and Fractals. Kluwer, pp. 41–82.
Schertzer, D. and Lovejoy, S. (2011) 'Multifractals, Generalized Scale Invariance and Complexity in Geophysics', International Journal of Bifurcation and Chaos, 21(12), pp. 3417–3456.