Comment on the authors' response to my referee's comments

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I very much appreciate that the authors ensure that most of my comments are addressed in some way in the current version of the manuscript. However 3 questions remain open:

A. *Regarding the authors' response to my comment on the figure reference in the following text (initially L325): « We have computed this multifractal spectrum in the von Kármán flow, using both experimental measurements and numerical simulations. The result is shown in Figure 12b. »:*
It is difficult to understand why the authors continue to refer here to local energy transfers (Figure 12b). Therefore, either the figure reference should point to the multifractal spectrum (Figure 11b), as I previously suggested, or the text should be clarified.

B. *Regarding the authors' response to my comments on the conservation of the energy flux (initially L323-325)*
I do not think that the arguments put forward by the authors can fully satisfy the readers of NPG, at least for the following reasons:

i) It should be explained why statistics are not sufficient to ensure the convergence of signed moments (assuming "unsigned" was a typo), while they are considered sufficient for absolute moments over the same range of statistical orders.
ii) I suppose that when mentioning the signed moments, the authors have in mind Kolmogorov's 4/5-law which gives a signed proxy for the energy flux density and whose derivation rigor has sometimes been debated.
iii) Then in particular in the inertial range, the relevance of the unsigned proxy obtained by the absolute moments of the third order could not be completely excluded.
iv) However, the latter is statistically scale independent for the moment of order $q \approx 0.18$ (instead of $q = 1$) and its moments diverge for higher orders q with increasing resolution, including its mean ($q = 1$).
v) This high ratio (≈ 5) between these two statistical orders may bring into question whether the average signed proxy will be scale independent. For instance, it seems difficult to "correct" this ratio by the renormalisation of the exponents mentioned by the authors, and whose practice (references will be welcome) tends to accredit that there is no empirical evidence of such a large statistical deviation.

Overall, I remain convinced that these facts should be addressed in the revised version. Indeed, a review paper is an opportunity to point out difficulties that could have been overlooked previously. To help the readers of NPG form their own opinions on this challenging question, the already suggested clarifications on the co-dimension formalism should also be taken into account, at least partially. Notably because it was intentionally designed for unsigned quantities.

C. *Regarding the authors' response to my comments on the "computational nightmare" (initially L335-360):*
It is not so obvious that « a reasonable balance in between the two options » has been struck by the authors, as my comment is not only "theoretically" correct, but it also does not depend on the data availability. I think that the authors could easily start to state their point of view and then mention the possible shortcomings that I mentioned.