NPG-2021-19 : Reply to comments from Referee #2

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.

We thank the Referee for their positive comment.

However 3 questions remain open:

Point A: L323-325 *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..*

The referee is right. We are sorry about our previous response, we thought he referred to another figure (the true figure 12b). We have now made the correction.

Point B: Regardind the author's respons to my comments on energy flux: 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_ _≈ _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.*

We have enlarged the discussion, to explain the difference between signed and unsigned velocity increments, developped more the link with Kolmogorov 4/3 law and stressed that the codimension interpretation was made for unsigned quantities. We have also added new data to the Figure 11b, and added a discussion regarding h_min

Point C: Regarding the author's response to my comment on the computational nightmare

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.

We have done what is suggested by referee and described the two optimistic and pessimistic options.