Answer to reviewers

Daniel Wolfensberger, Auguste Gires, Ioulia Tchiguirinskaia, Daniel Schertzer and Alexis Berne

October 20, 2017

1 Associate Editor

We thank the associate editor for his helpful comments. We have performed the request changes.

1. *There are quite a few technical, typographical errors that have to be taken care of. There is a general issue of readability because of English language errors. Please have a native speaker correct and run a spell checker.*

   The paper has been carefully reread and numerous errors have been corrected. The article has also been corrected by a native speaker (Tim Raupach), and we are quite confident that it has gained significantly in readibility.

2. *Caption to Table 1 is wrong*

   This has been fixed. The caption now reads:

   List and description of all synoptical and topographical descriptors used in the multifractal characterization of the climatology of precipitation intensities.

3. *I wonder whether how you calculate your rain rates in COSMO using the exponential functions and fixed slope parameter (single moment code) is consistent with the radar Z-R relationship.*

   Thank you for this very relevant remark. Indeed, we didn't think enough about this issue. We have now added a whole paragraph dealing with this issue at the beginning of Section 4.

   The power-law $Z - R$ relationship by (Marshall and Palmer, 1948), which is used by MeteoSwiss to derive the QPE, does not correspond with the $Z - R$ relationships derived from the COSMO microphysical parameterizations. This explains part of the discrepancy between radar QPE and simulated precipitation intensities, but in general should not impact the validity of the multifractal comparison. Indeed, if the $Z - R$ relationship derived from the COSMO parameterizations can be approximated by a power-law, then the correction needed to account for discrepancies in $Z - R$ relationships is itself a power-law: $R_{\text{corr}} = aR^b$, where R_{corr} is the precipitation intensity one would obtain by first converting COSMO precipition intensities to reflectivities and then back to precipition intensities using the radar QPE $Z - R$ relationship. It can be shown (Tessier et al., 1993) that in the context of universal multifractals, the corrected field will have the same value of α and the same scaling properties as the original field, while C_1 will be multiplied by b^α. Moreover, for the one-moment scheme, it was observed that while the intercept parameter a changes significantly, the exponent parameter b is almost the same: $R_{\text{corr}} = 0.68R^{0.98}$. As the exponent 0.98 is close to unity, this implies an almost direct proportionality, and as such even C_1 should barely be affected. Note that this power-law was derived by using the T-matrix method (Mishchenko, 1996) to compute radar cross-sections at C-band.

For the two-moment scheme, things are more complicated as no one-to-one relationship exists between rainrate and reflectivity. However, a rough estimation of the error on C_1 was derived by considering a representative set of rainy time steps from all events. The estimated values of b for the two-moment scheme varied between 0.81 and 1.23, which would imply a maximum relative error on C_1 of 51% on spatial fields and 23% on time series.

Overall, correcting precipitation fields for discrepancies in $Z - R$ relation is a difficult task, especially in the solid phase and for the two-moment scheme, where deriving a $Z - R$ relation from the model parameterization is difficult. However, in the multifractal context, only C_1 should be affected and only with significant solid precipitation or when using the two-moments scheme. This should be kept in mind when interpretating C_1 values.

4. *Please pay attention to your section numbering, which seems to have failed in the track change version.*

   Unfortunately this issue appeared when preparing the reviewed paper version (with blue fonts) using a macro. We apologize for not seing this when submitting the reviewed paper. This has of course been fixed now.

2 Anonymous Referee #1

We thank the anonymous referee #2 for his helpful comments and review. We have performed the required changes whenever it was possible.

2.1 Specific comments

1. - *In my opinion, the climatological analysis could be greatly simplified and streamlined by directly using the Köppen classification as descriptor instead of the meteorological and geographical parameters. As shown in Figs. 3 and B3, the meteorological and geographical parameters used in the climatological analysis are not independent: Potential vorticity is highly correlated to altitude and temperature to latitude. It would therefore be enough to use either the meteorological or the geographical set of parameters - or, as suggested, the Köppen classification, which both parameters sets are also related to.*

   It is indeed possible to retrieve a similar classification by aggregating some of Köppen classes, however the choice of the aggregation could be perceived as somewhat arbitrary. Additionaly, the Köppen classification is available only over land areas. We have thus decided to stay with the original classification but have added a paragraph in the appendix (C3), where both classifications are compared, and which shows that the obtained results are quite consistant.

2. - *The above suggestions would exclude the characteristics of the precipitation distribution (total amount, variance, wet fraction) as descriptors. Especially the wet fraction is currently used to explain the relationship between climatological classification as determined by latitude and altitude to the multifractal parameters. I am not fully convinced by the corresponding argument because the precipitation characteristics seem to be already "half-way" to the multifractal parameters. I would instead try to explain the multifractal parameters based on the typical precipitation types that correspond to the Köppen classification (e.g. frontal). Also the case studies from the second part might be useful here.*

Considering the small COSMO-2 area, most areas will be typically affected by frontal precipitation.

3. *- The TM coefficient is not included on p. 16, l 3 because it is not a real parameter. Why is it shown and discussed in Figs. 3, 5 and 6? The paper does not seem consistent in terms of the multifractal parameters investigated: The fractal dimension D_l is only used in the first part (without introduction) and Fig. 10 does not discuss γ_s. Is there a specific motivation for these choices?*

   Our claim that R^2 is not a proper characteristic of an area was probably exagerated. Indeed the quality of the scaling of precipitation within an area and how much the precipitation in this area is multifractal can be considered as a relevant characteristic. We have thus decided to also include this parameter in the statistical comparison. It appears that the R^2 is signicantly different between all clusters, so there is no explicit mention of it in the text, since only the non-significant parameters are mentioned.

   Thank you for pointing out that we forgot to include D_f (fractal dimension) in our theoretical explanations (Section 3) and in our second part of the results section. We have restructured the theoretical explanation to start with the definition of D_f and then generalize the theory to multifractals, which should be easier to folow.

   Let ϵ be a normalized (divided by its mean) conservative field, which can be one or two dimensional (time serie or spatial map). The fractal dimensions D_f of a field indicates how the binary field (where all values larger than a given threshold are set to 1) scales with the resolution. The resolution is defined by the ratio between outer scale L and observation scale l ($\lambda = L/l$).

$$N_\lambda = \lambda^{-D_f} \tag{1}$$

   where N_λ is the number of positive samples (rainy pixels for example) at a given resolution, which can be obtained with the help of box counting.

   It is possible to interpret this result in a probabilistic way. Indeed, let's consider a line or cube of size l. Pr is the probability that it intersects the field. This probability scales with the resolution:

$$\mathrm{Pr} = \frac{\lambda^{D_f}}{\lambda^{D}} = \lambda^{-c_F} \tag{2}$$

   where D is the dimension of the field (1 for a timeserie, 2 for a spatial field) and $c_F = D - D_f$ is called the fractal codimension of the field.

   It is clear that D_f (and c_F) depend on the threshold that is used. Several thresholds and corresponding values of D_f are hence required to characterize the field.

   In the results section, we have added D_f in Table 4. and we also discuss it in the text.

   In terms of fractal dimension D_f, it can be seen that for all events, both in space and time, the radar QPE has the most discontinuous binary precipitation field due to its smaller values of D_f. COSMO simulations are characterized by larger values of D_f indicating a wider coverage of precipitation and less convoluted precipitation fields and timeseries. It is interesting to notice that the two-moment scheme gives values of D_f that are closer to those of QPE, which indicates that it is better at simulating small-scale variations in the temporal and spatial occurence of precipitation.

For the last part (timeseries of multifractal parameters), it is true that we consider only α and C_1, which are the two main parameters of the multifractal framework, and explain the scaling behaviour accross all scales. We have made this more explicit in the text by writing "universal multifractal parameters" instead of simply "multifractal parameters".

4. *Why is the new analysis in part 1 performed on the direct fields - despit $H > 0.5$ for the spatial analysis but on the fluctuation fields in the second part?*

   Thank you for this remark. Indeed, we haven't noticed this inconsistency. Initially, we didn't consider fluctuations of the fields for practical reasons because they are quite expensive to compute, since our dataset is huge. We have thus repeated this analysis by using the fluctuations of the fields when $H > 0.5$. Unfortunately, this seems to break the observed spatial structure of MF parameters. This can be explained by the fact that taking the fluctuations of the fields is only a very simplistic method to dealing with non-conservation. The proper way of dealing with this issue would be to perform a fractional integration in the Fourier space, which would allow a much more smoother correction (the correction will be proportionnal to H), which would preserve the spatial structure of the data. Moreover, working with fluctuations will not help when $H < 0$. Ultimately, in order to be consistant we have decided to treat all zones in the same way, just like we treated all timesteps in the same way in the second part of our work, and to consider the original fields instead of their fluctuations. This is now justified in the text:

   Even though some areas are characterized by values of $H > 0.5$ (non-conservative fields), it was decided to treat all areas in a consistant way, by working on the original fields instead of the fluctuations. Indeed, it was observed that using fluctuations for areas where $H > 0.5$, was causing important discontinuities in the spatial structure of MF parameters. Indeed, using fluctuations is only a crude way to address the non-conservativity of the fields. A proper correction using fractional integration, would allow for a much smoother correction, since it is proportional to H, but is computationaly intractable because of the very large dataset that is used.

5. *Why are both, Fig. 7 and Fig. 8, needed? Are they not redundant, both providing the same scaling insights?*

   Fig. 7 and 8. are complimentary and they imply different moments, the spectral density is related to the second-moment, while the TM analysis is computed from many moments. However in Fig. 8, for sake of simplicity, we only show the TM analysis with $q = 1.5$. Indeed, we forgot to add the mention that $q = 1.5$, so this has been added to the caption of Fig. 8. Since α and C_1 are widely discussed in the paper, we thought it be relevant to show at least one TM analysis, since the slope of the best fit-line in the TM analysis at moment q is equal to $K(q)$. We have made this more clear by changing the first lines of the corresponding explanation in the text:

   Figure 8 shows the trace-moment (TM) analysis in time and space of the three events for $q = 1.5$. The value of $K(q = 1.5)$ is the slope of the best-fit lines. Repeating the TM analysis for various values of q allows to characterize $K(q)$ and to estimate α and C_1.

6. *p.21 —l 2: in general, the error of the parameters is not necessarily the same as the significance of the model. In how far can the discrepancies between the spatial and temporal analysis be used to estimate the systematic error of the parameters.*

   Generally, discrepancies between the spatial and temporal analysis cannot systematically be related to estimation errors in the MF parameters because they can also occur when a simple space/time advection model is not a realistic model for the considered precipitation system, for example when precipitation is not frontal or when strong orographic effects occur, which is often the case in the Alps.

2.2 Technical corrections

1. *- p13, l 31 + p 15, l 12, 13: The qualifier "much" does not seem appropriate and could be left out.*

   These qualifiers have been removed according to your suggestions.

2. *- Fig. 3: This is a very busy figure with lots of information. I think a different color scale, which only shows blue and red for high correlation coefficients and lighter/whiter colors in between would make the figure easier to grasp. Also, it could be helpful not to show (or hatch or replace by gray squares) correlations that disagree in sign between the spatial and temporal analysis, thus only showing robust correlations. Also, it would be helpful to visually separate the blocks with descriptors and multifractal parameters.*

   The figure has been changed according to your suggestions, and the caption has been updated.

3. *- Fig. 7: It would be helpful to add fit lines for the COSMO data as well - it is hard to follow the qualitative discussion about whether the 1- or 2-moment schemes scales more like the radar by having to guess where the lines might lie.*

   Fit lines are not really relevant in this case, since no single scaling regime can be observed for the model precipitation intensities. The idea of this plot is more to show how the radar spectral density seems to be more or less linear while the ones of the model are clearly curved.

3 Anonymous Referee #2

We thank the anonymous referee #2 for his helpful comments and review and for giving us a chance to publish our work. We have performed the required changes, which are listed below.

3.1 Minor comments

1. *There are still issues with grammar, especially comma usage, that make the text hard to follow in places. For example, on Pg. 13, "...two-dimensional geophysical process and in time, we consider..." is confusing without a comma before "and". Moreover, when introducing a sentence with "For x", a comma is needed after "x". Another example is on Pg. 14, Line 4, where a comma is needed before "follows".*

   The synthax and grammar of the paper have been improved and many commas have been added where necessary. The paper has also been proofread by a native speaker who confirmed the modifications and added some.

2. *Throughout the paper, there are many erroneous subsection numbers that should be removed.*

   Unfortunately this issue appeared when preparing the reviewed paper version (with blue fonts) using a macro. We apologize for not seing this when submitting the reviewed paper. This has of course been fixed now.

3. *Throughout the paper, there are many erroneous line breaks within sentences that need to be removed.*

   This issue has been fixed.

4. *Periods are missing at the end of several figure captions.*

   All necessary periods have been added.

5. *The added figures in Appendix A and Appendix B are nice; however, I would suggest just included them in the main text unless you add text surrounding them in the corresponding appendix.*

   We have decided to move the text associated with the corresponding figures to the appendix.

6. *Please review the reference – there are formatting inconsistencies.*

   Inconsistencies in the references have been checked for and should be fixed now.

7. *Pg. 1, Line 1: Reword – "allows to characterize" is awkward.*

   This has been changed to ("allows" was removed from the sentence):

   The framework of universal multifractals (UM) characterizes the spatio-temporal

8. *Pg. 2 Line 34: Change "simulations of" to "simulated".*

   Fixed.

9. *Table 1: Use "Wind speed"; add a space between units to avoid confusion.*

   Fixed.

10. *Pg. 5, Line 2: Should be "These data are".*

    Fixed.

11. *Pg. 5, Line 3: Need to add "the" before "beginning".*

    Fixed.

12. *Pg. 6, Line 17: QPE already defined.*

    The definition has been removed.

13. *Fig. 2: Enlarge to make it easier to read.*

    The figure is now double column, which makes it significantly larger.

14. *Pg. 11, Line 11: Move to end of prior paragraph.*

    Fixed.

15. *Pg. 13, Line 24: Change to "second-order".*

    Fixed.

16. *Fig. 3: The caption is incorrect – there is no left and right panel.* Changed "left" and "right" to "top" and "bottom.

17. *Pg. 15, Line 15: Consider replacing "spectacular" with a more descriptive term.*

    We replaced "spectacular" by "marked".

18. *Pg. 16, Line 1: Define ANOVA and use capital letters.*

    We removed the word "anova" from the description of the MANOVA and replaced it by: MANOVA (multivariate analysis of variance).

19. *Pg. 16, Line 2: Change to "tests".* Fixed.

20. *Pg. 18, Line 25: Change "that" to "which".* Fixed.

21. *Pg. 18, Lines 28-30: The wording is awkward; reword.* We have rewritten the sentence as:

    This comparison requires to run the COSMO model at the radar temporal resolution (5 minutes) in a very expensive setup (two-moments scheme)

    which should be easier to read and understand.

22. *Pg. 20, Line 20: Change to "large-scale".* Fixed.

23. *Pg. 20, Line 25: Use the variables from the table instead of just H.*

    Unfortunately, we were not totally sure to understand this point. We have removed the "s" in "multifractal parameters H", in order to make it more clear that we compare one single parameter H but for several events and data types.

24. *Pg. 20, Line 32: Use "one-moment scheme".*

    Fixed.

25. *Pg. 20, Line 33: You are comparing QPE between the radar and model, not the radar QPE with the scheme itself; reword.*

    This has been reworded as:

    In space, the trend is not as obvious and the match between the radar QPE and the precipitation intensities simulated with the one-moment scheme seems better.

26. *Pg. 24: Line 2: Use "south".*

    Fixed.

27. *Section 5.4: The section is a bit confusing because you start off saying that you will focus on the third event; however, there are two paragraphs on the first and second. Either omit the discussion of the first and second events or reword/reorganize the beginning of the section.*

    Maybe this part was confusing, because there are no paragraphs on the first and second events. We have tried to make this more clear by rewriting the beginning.

    To study the timeseries of multifractal parameters, the focus is put on the third (convective) event which shows the largest temporal variability. It was observed that the conclusions drawn for the third event in terms of discrepancies between radar and model multifractal parameters can be generalized to all events.

    This sentence is then followed by a detailed analysis of all phases of event 3 only. The last paragraph generalizes the conclusions to the other two events as announced at the beginning.

28. *Pg. 20, Line 20: Change to "large-scale".*

    Fixed.

29. *Pg. 24, Line 29: This should not be a paragraph on its own.*

    This has been fixed.

30. *Pg. 25, Lines 1-2: The sentence is awkward; reword.*

    The sentence has been reworded to try to make it more clear and explicit:

    Using a mobile window would make the discrepancies in multifractal parameters depend much more on the small-scale structures of simulated precipitation intensities, since it would strongly reduce the effect of misplacements of the simulated precipitation systems.

31. *Figure B3: Place figure title below color bar so that it is clear to the reader what the colors represent. For potential vorticity, use units of 10-5 K m2 kg-1 s-1 to remove the large number of zeroes in the colorbar.*

    We have fixed the figure according to your suggestions.