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Interactive comment on "Multifractal evaluation of simulated precipitation intensities from the COSMO NWP model" *by* Daniel Wolfensberger et al.

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Answer to reviewers

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The new version of the paper is provided as a supplement to this comment. The new parts are shown in blue, the discarded parts are not shown (to make it easier to read).

1 Review 1

1.1 General comments

Wolfensberger et al. interpret precipitation pattern as universal multifractals and explore the feasibility of this approach with regards to (i) investigating the sensitivity of precipitation pattern to orography and the choice of the cloud microphysics scheme (ii) evaluating a NWP model with observations. Multifractal methods have never been presented in ACP so that an application of this technique within the scope of atmospheric chemistry and physics is very interesting. The analysis of Wolfensberger et al. is somewhat unsatisfactory, however. It remains in large parts descriptive and only touches on interpreting the results of the multifractal analysis in relation to the underlying dynamics and physics. In particular, it does not become completely clear, what practical insights can be gained from the multifractal analysis as compared to the simple scaling analysis. By elaborating on these issues, the manuscript could be strongly improved and make the potential of multifractal analyses accessible to a broad readership of ACP.

1.2 Specific comments

1. *The analysis of liquid water contents (Section 4) seems to be strongly hampered by the non-conservativity of the fields. Given also the weak conclusions on the sensitivity to orography ("the dynamics of the weather event are more important than orography") and microphysics scheme ("the more complex scheme results in more variability"), it might be worth a thought if this section really strengthens the analysis or if the paper could be reduced to the surface analysis in Section 5.*

   We have removed this part of the paper and replaced it with a climatological study of simulated precipitation intensities using the universal multifractal framework, we hope that this will illustrate in a more intuitive way the relation between multifractals and atmospheric variables.

2. *Given that many readers of ACP might not be familiar with multifractals, it might be helpful to expand Section 3 with an example that shows how the appearance of a field changes for changing values of α, C_1 and γ_s.*

   We have added two plots (Figures A.1 and A.2 in the appendix) which illustrate the effect of changing α and C_1 and H on the appearance of a two-dimensional multifractal fields.

3. *What can be learned from the (non)-conservativity of a field?*

A value of H larger than 0 indicates that the field is smoother than the observed field from a direct multifractal cascade process and a value of H smaller than 0 indicates that the field is too discontinuous. When comparing H between COSMO and the radar QPE, one frequently observes that H is larger on the simulations, indicating that the spatial structure of the simulated fields is likely to be too smooth. We have tried to emphasize this in the text, when talking about the value of H.

4. *The locations of the scaling breaks differ between the scaling and the multifractal analysis. What are the corresponding interpretations and which scale should, e.g. a model developer take into account when trying to identify the responsible model process?*

   In the QPE part, the scaling breaks agree quite well between the spectral analysis (Figure 7) and the TM analysis (Figure 8). Part of the discrepancy you refer to was coming from the fact that the x-axis of Figure 7 was wrong. This has been fixed now (Observation scales goes from 2 to 128 instead of 1 to 64).

5. *Neither the model nor the radar data are in agreement with the simple space-time scaling model. This result should be discussed, especially in view of the correspondence between CAPE and multifractal parameters. This correspondence indicates a close relationship between precipitation pattern and dynamics, similar to the assumption underlying the simple space-time scaling model. In addition, the agreement found by Gires et al. (2011) for Meso-NH and corresponding radar data should be addressed.*

   Concerning the correspondence between CAPE and MF parameters, since this part has been removed from the paper there is no need to address the issue. In terms of agreement found with Gires et al., a new paragraph has been added in the paper at the end of Section 5.3:

   Gires et al. (2011) found different breaks for a Cevenol event (strong precipitation events occuring in Fall in the South of France), i.e. roughly 16 km in space and 1h in time, and a better agreement with a simple space time model but only for large scales which are not the primary focus of this study. These differences could be associated with the fact that the topography of the area analysed in this paper is more pronounced than in **?**. It should also be noted that the values of UM parameters α and C_1 on the relevant range of scales exhibit a better agreement between observations and model simulations in this paper.

6. *I could not quite follow the interpretation of Fig 12 and Table 2 (see below). For Fig 14, I wonder how relevant (although presumably significant) observed differences are (see below).*

   Since the data that is used is not instrumental (i.e. model data so not affected by noise), the only source of uncertainty comes form the numerical estimation method of multifractal parameters. The DTM method is generally quite robust, and if there is good scaling $R^2 > 0.95$, the uncertainty of the multifractal parameters would be negligible. In our case, since we used only the scale ranges with sufficiently good scaling we get values of R^2 that are usually very close to 1, with the smallest being 0.92. So though we cannot precisely state the uncertainty associated with the MF parameters, we think that are quite trustful. We have added the following sentence at the end of section 5.1

   Note that in the considered range of scales the quality of scaling measured by the R^2 parameter is quite good (average R^2 in space = 0.963 $\pm$ 0.024, in time 0.956 $\pm$ 0.017. This implies that the uncertainty associated with the α and C_1 parameters retrieved with the DTM method is small.

1.3 Technical corrections

1. *p7, L16 and 18: Should this read Zone 2 and Zone 3 instead of Zone 1 and 2?*

This has been corrected and adjusted to agree with the modifications of the first part of the paper

2. *p9, L11: Isn't this an upper threshold that the values of ϵ_λ fall below rather than to exceed it?*

   Indeed, thank you for pointing this out. The sentence has been corrected

3. *p9, L13: c(γ) instead of c(λ)*

   This was corrected, thanks

4. *p9, Eq 5: K_c instead of K*

   Yes, we added the subscript

5. *Eq 6: What is D?*

   Thank you for pointing this out, D is the dimension of the field (1 for a timeserie, 2 for a spatial field). We have added this sentence in the text.

   D is the dimension of the field (1 for a timeserie, 2 for a spatial field)

6. *p12, Eq 14: no italics for subscript "time"*

   Fixed

   Points 7 to 12 correspond to parts that has been removed so these points are not relevant anymore

13. *p20, L2: To me, there seems to be a scaling break in the radar data at 4km*

    There is indeed a scaling break at around 8 km (and not 4 km since as stated before there was an error in the x-axis labels) for the radar QPE for the second event only. We added this info. For the other events we do not see a scaling break with a comparable intensity to the scaling breaks observed for the COSMO precipitation.

Both radar and simulations show a weak scaling break at around 8 km.

14. *p21, L6: Doesn't the spectrum show an under-representation of large features in the model as compared to the radar?*

    Yes we have added the following explanations in the text:

    This is especially visible for the last (convective) event, where the COSMO simulations show weak scaling (β close to zero). This implies that the simulated rainfall intensities are dominated by small-scale features, while large scale features are underestimated. Note also that for large scale features, the power density function of COSMO simulations correspond to white noise, indicating that the COSMO model has a shorter decorrelation range than the radar data.

15. *p21, L8: Both, the one and two moment scheme have $\approx$ 0 for the last event.*

    Yes this quite true, we have changed the sentence accordingly, see last point.

16. *p21, L18: In space, QPE H is smallest for April 8 with a value of 0.342, not for March 26*

    Yes indeed, the sentence was ambiguous. We only wanted to say that when compared with COSMO H values, then for the first event, the radar H is the smallest of the three. We have made this more clear in the text

    Taking the radar as reference, one sees that the convective event is characterized by the largest values of H followed by the snowfall event and the stratiform event.

17. *p21, L23: Equation 12, not 14; I assume, this time the resulting fields are conservative? This should be mentioned.*

    We fixed the reference to the equation. We have also added the following sentence at the end of the paragraph to make it more clear:

Note that while this does not result in perfectly conservative fields, it still makes them more conservative since all values of $|H|$ are smaller than 0.5 after taking the fluctuations.

18. *p22, L7: event instead of events*

    Fixed

19. *p22, L14: How meaningful is a clear difference of 1.34 or 1.35, respectively, compared to 1.28 for practical purposes? Asked differently, what is the accuracy of these values?*

    See last point of the *Specific comments* section.

20. *p28, L7: Equation 12 instead of 14*

    See Point 18

2 Review 2

2.1 Major comments

1. *As noted above, universal multifractals is something not seen in most ACP publications. Thus,would highly recommend a table outlining the variables of interest and how they are tied to the analysis. In the text, it would be very beneficial to include a subsection that outlines what each variable means in terms of increases and decreases and relate this back to the physical context of the systems of interest.*

   We have added a table (Table 3) that gives an overview of all relevant parameters in the universal multifractal framework as well an interpretation of their effects on precipitation. In the appendix we have also added some illustrations of how this multifractal parameters affect the structure of a spatial field. We have also thoroughly changed the first part of the paper in order to better explain the link between the MF parameters and the meteorological and geographic variables.

2. *Again, as noted above, much of the analysis is cursory at best, leaving the reader scratching his/her head for an explanation. For example, Lines 15-19 on Page 21 and Lines 10-11 on Page 22. There are many more. I kept asking myself why? I realize that the paper is already a bit long and so adding more analysis will make it cumbersome to read. However, perhaps it is worth excluding the effects of topography and microphysics and focus more on the model-observation comparison? Leave the effects of topography and microphysics for a subsequent publication once the groundwork is published, especially since the results for these aspects seem case-dependent.*

   We have decided to keep the microphysical aspect as we thought that it would be interesting to see how the same model could produce quite different spatial structures depending on its configuration and since the paper is much shorter now than it was before. The effect of topography is considered in the first part of the paper which is new but it replaces the whole section about the water contents (first part) which has been discarded as it was not very conclusive. Some parts have been shortened as well such as the very last part (study of the timelines of multifractals) which was quite redundant between events.

3. *A follow on to the previous point is that there are many instances where I felt as if there should have been a figure to reference and yet nothing was referenced. For example, the paragraphs beginning on Line 4 of Page 15, Line 33 of Page 17, and Line 1 on Page 21. There are other instances as well where it was not clear if a figure in the text was intended to be referenced or if analysis was conducted and not presented in the text. Along the same lines, some of the figures are very difficult to read due to the small font. Moreover, while I realize that the observations are difficult to retrieve at low elevations, the model can and does simulate such levels. I was a bit puzzled as to why these levels were removed from the initial analysis. Perhaps they should be included why just examining the model and then removed for comparisons with observations.*

   The issue with missing model levels was coming from the fact that the multifractal framework does not handle missing data which naturally occur when interpolating model data at fixed altitudes. In order to addres this limitation, we have now focused the first part of the paper on the precipitation at the ground level as simulated by the model, so the lower troposhere is not ignored anymore.

4. *Lastly, I could not understand Table 2; some background and explanation is clearly warranted.*

   We have provided a better description of the contents of Table 2 (Now Table 4)

   Table 4 displays the non-conservation parameters H evaluated for timeseries of precipitation intensities (analysis in time) and for spatial fields of precipitation intensities (analysis in space), for both the radar QPE (in regular font), the COSMO one-moment scheme (in bold) and the COSMO two-moments scheme (in italic) and for all events

2.2 Minor comments

1. *The organization is quite nice; however, there are several grammar errors (especially punctuation), and there are issues with figure and equation referencing. Moreover, units are inconsistent (e.g., g versus mg).*

   We have checked all the references and have fixed the problematic ones, it should be fine from this point of view now. In terms of units they should be consistent now. The first part of the paper has been changed and we only deal with precipitation now, either in mm (for accumulated quantities) or mm $\cdot$hr^{-1} (for precipitation intensities). Note however that multifractals parameters are not sensitive to units, so even using inconsistent units should not impact the results.

2. *Consider not capitalizing words like east and west.*

   We have put all cardinal directions in lowercase

3. *I would recommend including references throughout section 2.1 for all assumptions that go into the model.*

   Unfortunately most of COSMO's parameterizations have not been published in peer-reviewed jour- nals. We have included all references to peer-reviewed work we could find (this includes a few new references, e.g. Rutledge et al 1983 and Lin et al. 1983

4. *Consider using section instead of chapter*

   The part where the word "chapter" was used has been removed from the paper so this is not an issue anymore.

5. *In equation 1, the variables do not match with the subsequent descriptions*

   Yes indeed, thanks for pointing this out, we changed the λ in the equation to a Λ in order to avoid confusion with the resolution in the multifractal framework. We have now fixed the description to use Λ as well.

6. *Line 29 on Page 3: Should this be number concentration?*

   Yes indeed this is a better choice. We have changed "concentration" to "number concentration" and "mass fraction" to "mass concentration" to be more explicit and consistant.

7. *Line 30 on Page 3: Should this be mass mixing rations?*

   See Point 6

8. *Table 1: Consider writing out number*

   This has been fixed accordingly

9. *Line 8 on Page 5: What is meant by size being a power of two?*

   This paragraph is not in the paper anymore

10. *Line 4 on Page 7: Correct the definition of PPI.*

    We have changed this to "plane position indicator (PPI) "

11. *Lines 6-7 on Page 17: Reword to improve clarity.*

    As this part is not in the paper anymore, this issue is not relevant anymore

12. *Line 2 on Page 20: What is meant by opposite of the slope.*

    We have tried to make this more explicit by writing:

    A best-fit line is shown for the radar QPE from which the value of β is computed. β is equal to $-m$, where m is the slope of the best-fit line.