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Interactive comment on "Spatial variability of snow precipitation and accumulation in COSMO–WRF simulations and radar estimations over complex terrain" *by* Franziska Gerber et al.

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Review of paper

"Spatial variability of snow precipitation and accumulation in COSMO–WRF simulations and radar estimations over complex terrain"

by F. Gerber et al.

submitted to The Cryosphere

This paper presents a comparison of snow precipitation fields from very high-resolution WRF simulations and radar estimates for three recent snowfall events near Davos

(Switzerland). WRF simulations are performed in LES mode down to 50 m horizontal grid spacing. The technical challenges of this project are enormous and I congratulate the authors for doing this detailed analysis. Unfortunately, the WRF simulations of snow precipitation deviate rather strongly from radar observations (- I have a slightly more negative impression of the model performance than the authors because of the partially huge differences in near-surface winds and strong precipitation overestimations) and this model-observation differences make it challenging to learn a lot from these simulations. The authors use a set of statistical tools and in my impression the variogram analysis is particularly interesting and delivers the most valuable results of this study. I recommend to accept this study for publication with major revisions in order to improve the readability of this paper and to clarify a range of issues (see detailed comments below).

Major comments ————

A) I found it rather difficult to read the paper and follow the story-line. I very strongly recommend that the authors add a paragraph at the end of the introduction, where they clearly outline the specific objectives of the study and why they need the tools they use to address these objectives. In the current manuscript I was missing a clear roadmap: what are the objectives and questions? Why do we look at a certain diagnostic? What in the end did we learn from a specific section?

B) To further increase the readability, the text should be improved and several unclear statements amended. Several examples are given below in the minor comments. In several paragraphs I was left with a rather unclear impression of what I should have learned from reading the paragraph - a more reader-friendly writing style would help.

Minor comments ————

1) p. 1 line 3: the term "particle-flow interaction" appears several times in the paper but is never explained. I am not sure whether I understand the term, is it about how the turbulent flow influences the pathway of individual snow particles? Is this really the

topic of this study? Please clarify.

2) p. 1 line 5: it looks as if COSMO-WRF was the name of the reanalysis data, but this is not what you mean - please clarify.

3) p. 1 line 10: not clear what "variability relative to the domain wide variability" means

4) p. 1 line 11: "quite substantial" is not scientific language, can you be more specific?

5) p. 1 line 11: The last sentence of the abstract "However, ..." seems rather unconnected to the previous sentences

6) p. 2 line 7: here and in many other places: why are references not in chronological order?

7) p. 2 line 31 and p. 5 line 20ff: you refer to the COSMO-2 analysis fields as "reanalyses", which is most likely wrong. If I am not mistaken, then the fields you use are the operationally produced hourly analysis fields. The term REanalysis would only be appropriate if the older analyses were recalculated with a newer data assimilation approach. If this is the case, then it should be explained.

8) p. 3 line 1: I know what you want to say, but this is a strange sentence. Of course, relevant measurements can also be made with surface barometers, it just depends on what you want to measure!

9) p. 3 line 4: "orographic mechanisms": not clear to me what you mean

10) p. 3 line 20: here and in other places, use km for kilometer and m for meter

11) p. 3 line 33: mb should read hPa

12) p. 4 line 2: I would rather say that this is the northeastern part of Graubünden(see also p. 6 line 19); no need to write "(CH)"

13) p. 4 line 7: I find "mesoscale mode" a bit confusing, why not just LES and non-LES mode? And - see Table 1 - is there really no PBL scheme used in LES mode? I thought

that for the small-scale turbulence you still need a parameterization?

14) p. 5 line 3: what is a "combined simulation"?

15) p. 6 line 10: units (here K m-1) should not be in italics (see also several other places)

16) p. 7 line 31: should read "restrict ... to only four ..."

17) p. 8 line 21: I don't understand: "three different domains" but then four resolutions?

18) p. 8 line 27: please explain what a "lag distance bin" is and for what purpose you will use the semivariance

19) p. 10 last line: "For all stations ...": there are only two –> "For both stations ..."; then "matches well", hm, Fig. 2 shows deviations up to 6K, which is a lot!

20) Figure 2: I find the overestimation of winds in WRF for the first two cases rather dramatic (my impression is that this should be described more clearly); is there any reason why winds are so much better for the 3rd case?

21) p. 11 line 5: here I find the "COSMO blaming" a bit too strong, because for instance Fig. 2 shows that COSMO is doing better for the 2nd case. Rather often, the 2-m RH values are 100%, which indicates fog. Could it be that correctly simulating fog is a particular challenge for WRF?

22) p. 12 line 5: "For some stations ...", but you only discuss two!

23) p. 12 line 10: "could partially reduce overestimated wind ...": unclear, reduce compared to what?

24) p. 12 lines 12 and 20: "reasons ... may be manifold" is not a strong statement (and it should not be repeated). It seems that the "reasons are unknown", which is fine but it should be stated maybe in this way.

25) p 12 line 18: but if very small-scale topography matters so much, why then is

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COSMO performing better for some cases?

26) p. 12 line 25: improve writing "in precipitation is observed for precipitation ..."

27) p. 12 line 27: The sentence "Although ..." is too long and very difficult to understand, in particular, why does the sentence start with "Although"?

28) p. 12 lines 28 and 34: I am a bit lost, aren't these two sentences contradicting? Once southerly flow leads to a N-S gradient and then for southerly advection there is a S-N gradient ...

29) p. 13 line 4: Another example where reading/understanding is difficult: "Disagreement of patterns ...": you probably mean "between WRF and radar", and then "strong smoothing of topography": you probably mean "in the model". Please improve the writing to make this type of sentences much clearer.

30) p. 13 line 7: "precipitation fed to domain d02" but before, on p. 5 line 4, you wrote that "hydrometeors cannot be used as a boundary condition in WRF simulations": how does this go together? What do you mean by "feeding precipitation"?

31) p. 13 line 11: you show the overestimation of winds only at 4-5m! It is not evident that winds are also too strong higher above.

32) p. 13 line 19: I don't understand partially filtered vs. median-filtered

33) Figure 4: I find it unfortunate that the different panels for the same case have different scales. Also, I find it difficult to interpret the topography contours: for non-locals it is not clear where valley and mountains are. A color plot of topography only could help.

34) Figure 5: I don't understand the difference between the 300 and 450m resolutions: why is this so important? why has it such a large effect? and why is this effect different for radar than WRF for the first case?

35) p. 15 line 2: Hm, isn't this a complicated way of saying that the median is higher if

there is more precipitation?

36) p. 15 line 3: what is meant by "shifted"? In WRF relative to radar?

37) p. 16 line 15: I don't understand "strong smoothing may not allow for precipitation to evolve"

38) p. 17 line 21 (and in other places): not sure that "idealistic" is the right term, do you mean "simplistic"?

39) p. 17 line 22: what is "particle dynamics"?

40) Figure 6: I first overlooked the small 10ˆ4 beneath the x-axis and wondered about the values. Why not just write ticks at 5, 10, ... km?

41) p. 22: to me, these two statements are contradicting each other: first you say that topographic features are underrepresented (I agree) and then that subsidence and lifting are too strong - but aren't they linked to topography?

42) p. 23 line 17: not sure that I understand "When observing with a critical eye", what is observed here?

43) p. 24 line 2: I don't understand this important sentence: I somehow understand that you say that WRF has too simplified cloud microphysics, but then why "due to the given resolution"? Do you say that the microphysics is OK for another resolution?

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