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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.

Anonymous Referee #2

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General considerations

In this paper, the authors use a COSMO-WRF dynamical downscaling suite (one-way nesting) to investigate the ability of the numerical model to reproduce the spatial variability of precipitation in complex mountainous terrain. The authors have a very valuable (and quite novel) data set at hand for their analysis and perform model simulations, which indeed are able to address the raised questions. The paper is well presented (but see the quite numerous 'detailed comments'), the analysis is sound and the authors do, in general, appropriately value their results and critically assess them. Even if I have two comments that are termed 'major', they will easily be addressed, so that

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Discussion paper

my overall disposition is ‘minor revisions’.

Major comments

1) Explanation of WRF offsets (Section 3.1, point inter-comparison). It is argued that these offsets were (partly) ‘due to offsets in the COSMO-2 input’ (p11, l. 2/5; p12, l. 11). This is a weak argument. Either the domain and time settings (e.g., distance from the inflow domain border; spin-up time) are chosen in a way that allows the downscaled model run to develop its own local conditions - which would demonstrate the improvement (or deterioration) due to the higher resolution, or the downscaling is essentially useless. This argument should be re-considered. Furthermore, for wind speed in particular, e.g. Jimenez and Dudhia (2012) come to a quite different conclusion than the present authors (but based on simulations for which the roughness length is not artificially enhanced). The present results should be at least discussed in the light of their results. Also, it should be noted that apparently WRF can produce some 8 m/s wind speed at times, while the observation is around 2 m/s or even less. On other days (different wind direction!) the WRF bias is not nearly as large. This does not seem to be a problem of mere roughness length or some problem in the ‘model physics’. To me, this rather looks like a problem with the height attribution (or ‘correction’ - using a logarithmic [neutral?] profile - of the modelled profile to the observation height). Could it be that the actual observation height was ‘2 m minus snow depth’ (rather than 2 m as in the model)? So, in case of a deep snow layer at the observational site the actual observation height would possibly be much smaller. In any case, this issue should be analysed in some more detail.

2) Overall conclusions: the authors nicely demonstrate the impact of topography (in different model resolutions) on the quality of the spatial variability of precipitation estimates by the model. In the conclusions, however, the arguments are somehow mixed up. First, (p24, l.2) it is argued that the difficulty in reproducing the small-scale variability is ‘most likely [due to] the representation of cloud dynamics and microphysics in the model [being] too idealistic’. This is then supported by a number of arguments, all refer-

ring to the topography (. . .). Then, some few lines later (p24, l. 6) it is concluded 'This shows that especially for small-scale variability a better representation of the complex terrain is essential to reproduce precipitation variability'. So, what is it now? The terrain representation or the microphysics being to idealized? In my view, the results of the authors have demonstrated the impact of terrain (and resolution), but no conclusions can be drawn concerning the microphysics and cloud dynamics. Certainly, the authors have not done that – and so, such a 'conclusion' concerning microphysics and cloud dynamics cannot be maintained.

Minor comments

P3, l.1 means, not mean

P3, l. 33 hPa, not mb

P4, l. 4 set up, not setup

P4, l. 4 with refined vertical levels: from the above I understand that also d3 has refined vertical levels (60). Does this sentence mean, that these are not within the boundary layer? Please explain.

P4, l.6 nests run in 'LES'-mode: for d2 (450 m horizontal grid spacing) and possibly d3 (150 m) this is in the middle of the 'Gray Zone' of Wyngaard (2004). Can the authors comment on that?

Tab 1 please add the heights of the first few model levels

P6, l.11 logarithmic wind profile: does this mean that neutral stratification is assumed? (this might be essential when explaining the substantial differences in wind speed between observations and model)

P6, l. 12 this correction is quite unclear: what is 'corrected'? the measured wind speed? The modelled wind speed? In other words: is the modelled wind speed 'extrapolated' to the height of the observation (if so, using which input (friction velocity and roughness length from observations? Or from the model?) or are the measured wind speeds 'corrected' to the first model level?

P6, l. 16 included into what?

P7, l. 29 'we observe. . .' seems to be somewhat misleading (I assume it should mean that the lowest observation level is at 2800 m). Please reformulate.

P8, l.22 'the domain. . .covers 30 km': with a radius of 30 km? or a radial extent? Anyway, the domain should probably be characterized by an area information not a distance.

P15, l. 2 i.e. a high median is present/ is modelled/ occurs if the large-scale. . ..

P16, l. 22 in-depth Tab 2 caption refers to 'WRF snow precipitation' while the entries are either 'WRF 2830 m asl' or WRF total ground precip. Please clarify.

P17, l. 14 reproducing the trend: what is now the trend? Do you mean what before was called the inclination? Please clarify.

Fig. 6 If the normalized variance is shown (as indicated in the caption): why then can it be at times larger than 1? Please clarify (normalized by what). Same in Fig. 9

P20, l. 16 represent

P21, l.15 geostatistical analysis: either 'analyses show' or 'analysis shows'. However, it is not clear, whether the authors refer to their own analyses here (as presented in the foregoing sections) or to other analyses (by others), in which case a reference would be needed.

References Jimenez, P. A., and J. Dudhia, 2012: Improving the representation of resolved and unresolved topographic effects on surface wind in the WRF model. J. Appl. Meteor. & Climatol., 47, 308–325

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