Answer to reviewers - second revision

Daniel Wolfensberger and Alexis Berne

May 11, 2018

Anonymous reviewer #1

We thank reviewer 1 for taking the time to revise again our work and for suggesting some additional corrections

Technical corrections

1. *Page 33, line 1: numerical representation of the Gaussian convolution*

   Thank you for these precisions. We have adjusted the equation according to your suggestions.

   $$S^{\text{corr}}[i] = \frac{\sum_{j=0}^{N_{\text{FFT}}} S[j] G(v_{\text{rad,bins}}[i] - v_{\text{rad,bins}}[j])}{\sum_{j=0}^{N_{\text{FFT}}} G(v_{\text{rad,bins}}[i] - v_{\text{rad,bins}}[j])} \tag{1}$$

   where G is the Gaussian kernel defined by:

   $$G(x) = \frac{1}{\sigma_{t+\alpha}\sqrt{2\pi}} \exp\left[-\frac{x^2}{2\sigma_{t+\alpha}^2}\right] \tag{2}$$

   where $\sigma_{t+\alpha} = \sigma_t + \sigma_\alpha$

2. *Page 4, line 16-20: ice sedimentation in the COSMO version that is being used*

   Thank you for this information. We have corrected the text accordingly.

   In the version of COSMO that is being used (5.04), ice crystals have a bulk non-diameter dependent terminal velocity, that depends on their mass concentration.

   And in the next paragraph:

   In COSMO, with the exception of ice crystals and rain in the two-moments scheme, mass-diameter relations as well as velocity-diameter relations are assumed to be power-laws. For rain in the two-moments scheme, a slightly more refined formula by Rogers et al. (1993) is used. For ice crystals, the two-moment scheme, in contrast with the one-moment scheme uses a spectral (diameter-dependent) representation of ice crystal terminal velocities.

Anonymous reviewer #2

We thank reviewer 2 for taking the time to revise again our work and for suggesting some additional corrections

Technical corrections

1. *p4l16-19. 'In terms of terminal velocities, in the version of COSMO that is being used (5.04), neither ice crystals nor cloud droplets are sedimentating.' is repeated twice.*

   We have rephrased this sentence in accordance to the second remark of reviewer 1. So there is no repetition anymore.

2. *p10l34-p11l5. Please either remove these explanations or merge them with the newly introduced ones (in blue p10l25-32).*

   We're sorry about this regrettable formatting issue. We indeed forgot to remove the older part when we added the new one (in blue). This is now fixed.

3. *p11l6. 'Trilinear downscaling' should be mentioned before. Otherwise, the previous explanations are wrong in the general case: for example, nearest-neighbour interpolation preserves the mathematical relations between radar observables. By the way, the term 'interpolation' seems more appropriate to me than 'downscaling' in this context.*

   Yes we agree. We have replaced all mentions of downscaling by interpolation and we have modified the mentioned paragraph to introduce the fact that trilinear interpolation is being used and insist that the explanations are valid for linear interpolation only.

   Once the coordinates of all radar gates have been defined, the model variables must be interpolated to the location of the radar gates. This is done with trilinear interpolation (linear interpolation in three dimensions). The advantage of interpolating model variables before estimating radar observables, instead of doing the opposite, is twofold. At first, it is much more computationally efficient, because computing radar observables requires numerical integration over a particle size distribution at every bin, which is costly. Secondly, computing radar observables after linear interpolation allows to preserve the mathematical relation between them. Indeed, radar variables are far from being independent. For example, in the liquid phase Z_{H} is closely co-fluctuating with Z_{DR}, in the form of a power-law that tends to stagnate at large reflectivities. Some tests were performed on random Gaussian fields of rain mass concentration. The results indicate that when computing the radar observables first and then interpolating them, this theoretical relation becomes more and more linear when the the interpolation resolution increases, which is quite unrealistic. In contrary, when computing the radar variables after interpolating the rain concentration field, the theoretical relationship is always preserved, regardless of the interpolation technique that is being used.

4. *p13eq8. The 3-dB beamwidth should be squared.*

   Thank you for pointing this out. Indeed we forgot the square. We have fixed it in the equation.

$$I\left[y\right](r_o,\theta_o,\phi_o)=\int\limits_{\theta_o-\pi/2}^{\theta_o+\pi/2}\int\limits_{\phi_o-\pi}^{\phi_o+\pi}y(r_0,\theta,\phi)\,\exp\left(-8\log 2\left[\frac{\theta_0-\theta}{\Delta_{3\mathrm{dB}}}\right]^2-8\log 2\left[\frac{\phi_0-\phi}{\Delta_{3\mathrm{dB}}}\right]^2\right)\cos\theta d\theta d\phi \tag{3}$$

5. *p13eq9. Inconsistency in equation*

   Yes, thank you for insisting on this point. There were indeed many inconsistencies in this equation and in the following explanation. First of all, the equation should have a double sum, and second z_j and z_k were used in the equation but z_i and z_j were used in the following explanation. We have rewritten the equation and the explanation to make it consistent and accurate.

   Note that the index j now stands for elevational integration and the index k for elevational integration, to be consistent with equation 8., where the first integral corresponds to the elevation angle, and with order of the arguments of y. Placing the elevational integration sum first also allows to factorize the cosinus term, simplifying the equation.

$$I[y](r_o, \theta_o, \phi_o) \approx \sum_{j=1}^{J} w'_j \cos(\theta_0 + z'_j) \sum_{k=1}^{K} w'_k \; y(r_0, \; \theta_0 + z'_j, \; \phi_0 + z'_k) \tag{4}$$

where $w'_j = \sigma w_j$, $w'_k = \sigma w_k$ and $z'_j = \sigma z_j$, $z'_k = \sigma z_k$ with $\sigma = \frac{\Delta_{3\mathrm{dB}}}{2\sqrt{2\log 2}}$, where $\Delta_{3\mathrm{dB}}$ is the 3 dB beamwidth of the antenna in degrees. w_j and z_j are respectively the weights and the roots of the Hermite polynomial of order K (for elevational integration) and w_k and z_k are the weights and roots of the Hermite polynomial of order K (for azimuthal integration).

6. *p24 eqs28 and 29. I thought that the fraction in eq 28 could be simplified by dropping Q^s in the numerator and denominator, and that the fraction in eq 29 could be simplified by dropping Q^g in the numerator and denominator. After doing so, isn't it obvious that $f^{ms}_{wet} = f^{mg}_{wet}$?*

   Yes, thanks, of course they are equal. Sorry for not realizing this earlier...We have fixed it in the text:

   The wet fraction within melting hydrometeors can be estimated by the fraction of mass coming from rainwater over the total mass. This results in equal wet fraction for wet snow and wet graupel:

$$f^{ms}_{\mathrm{wet}} = f^{mg}_{\mathrm{wet}} = \frac{Q^r}{Q^s + Q^g + Q^r} \tag{5}$$

7. *p45eqC2. The upper limits of the integrals are misplaced.*

   Yes thank you for pointing this out. This is a small issue that appeared when using a script (*latexdiff*) to highlight in blue the modifications added to the text during the first revision. This typo is not present in the latest version of the manuscript.

8. *p46-52. Please check the references carefully. There are still typos here and there, such as 'reectivity' and missing conference name in Furukawa et al. (2016).*

   Thanks for pointing this out, we have corrected the typos, added the conference name for this particular citation and have homogeneized the references, for example by adding all the DOIs that were missing.

9. *p58fig20. I am not sure I understand the y axes. Which density is meant here? What is the reference? In panel c, density does not seem to be normalized (ie, the area under the curve is not equal to 1) since the density of GPM observations can exceed 1. I am actually wondering whether the densities of observations and COSMO in panel c are comparable. GPM can certainly not detect low values such as 1E-3 mm/h (by the way 'hour' should be abbreviated with 'h' in the international systme of units). In panels a and b, the x axis starts at 14 dBZ, which corresponds to the sensitivity of GPM observations (by the way, in panel c, the x-axis legend is wrong). In contrast, in panel c, the sensitivity of GPM observations does not seem to be taken into account. The same remarks about the meaning of 'density' apply to p56fig17 and p59fig21.*

   The density here indicates the frequency density, i.e. the y-axis of a normalized histogram for which the area (integral) is equal to one. So the units would be the proportion of samples per unit of variable of interest x, so 1 over over the units of x, in analogy with the density of a probability density function. We have added an explanation the first time the term is employed (second paragraph of Section 4.2).

   Note that in the scope of this work, the term *density* indicates the frequency density, in analogy with a probability density function. It represents the proportion of samples within every bin divided by the width of the bin, such that the integral of the empirical distribution is equal to one. It is thus in units of x^{-1}, where x is the unit of the considered variable (in this particular case $x = \mathrm{m\ s^{-1}}$).

   We have also fixed the notation for hours by replacing hr by h on all the plots.

   Finally regarding the strange values of density in the precipitation distribution, you are indeed right, this is confusing. The reason for this is that the histogram has actually been computed in $\log_{10}$ scale, so the values are between -5 and 2, and the bin widths are thus smaller than 1, which explains why you can have values of density larger than 1. Thanks to your pertinent remark, we noticed that for this particular type of plot, it is not correct to replace the logarithmic x axis tick labels by their linear equivalents (e.g. $1 = 10$, $2 = 100$). We have thus reverted to the tick labels in logarithmic scale and have updated the x-axis label accordingly (insisting on the fact that the histogram is computed on the logarithm of precip. intensities).