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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigating the sensitivity of high-resolution mesoscale models to microphysical parameters by the use of polarimetric radar observations" *by* R. Ferretti et al.

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Anonymous Referee #1

Received and published: 14 October 2010 Review of: "Investigating the sensitivity of high-resolution mesoscale models to microphysical parameters by the use of polarimetric radar observations". Authors: Ferretti et al.

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Interactive Discussion

Discussion Paper

GENERAL COMMENTS The manuscript describes a series of sensitivity tests, pertaining to the graupel parameters in one-moment bulk microphysics schemes, that have been done with two mesoscale models for 1-km simulations of a hailstorm. Comparison are made to radar reflectivity and hydrometeor type and content derived from polarimetric radar observations from two radars.

A considerable amount of work has obviously been done for this study and several different approaches to model-to-radar comparisons have been attempted. Overall, I find the depth of the analysis to be too insufficient to provide much useful insight into the understanding of model sensitivity to microphysics parameters. I believe that the observations, interesting though they are, are not at all useful for this study given that the model simulations are so different from the observations. This is not a criticism of the models or the modellers – it is very difficult to simulate an individual hailstorm. But given this, the comparisons to observations are thus intrinsically problematic for this type of examination of model microphysics.

»> The referee states that both models are not able to reproduce the storm. In our opinion the storm signal is surely reproduced by the models, even if some flaws are found for both models, with a larger extent for COSMO-LAMI. Beside for a time shift MM5 well reproduce the ground reflectivity and the precipitation; for what concerns the vertical structure two cells are initially produced which finally merge in to one, as it is observed. Actually, the problem for MM5 is the correct reproduction of the vertical content of the hydrometeors: graupel is correctly reproduced by only one experiment, but it does not reach the ground, a fact which would suggest a not correct setting of the parameters. Differently for COSMO-LAMI, in fact in this case difficulties are found in the correct reproduction of a widespread structure of ground reflectivity instead of a localized one. For what concerns the raincell, COSMO-LAMI produces two cells which do not merge together. An attempt to explain this behaviour is to speculate on the different diffusivity capability of the two NWP models. MM5 is probably characterized by a larger diffusivity than COSMO-LAMI which would smooth down the downdraft

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strength allowing for the cell to reinforce and aggregate; on the contrary, COSMO-LAMI downdrafts are probably to strong enabling the development and the aggregation of the cell at this resolution.»>

My primary criticism, however, is the general lack of depth in any of the analysis. A series of sensitivity tests has been performed (following very closely to the study of Gilmore et al. 2004) and the results have been examined in a few different ways, but nearly all of the discussion is essentially just a description of the differences between the results. There is very little discussion, examination, or even speculation as to the reasons for the differences. One of the primary goals of such a study should be to provided recommendations to modellers regarding the best choice of parameter settings. But all this manuscript shows is that the model simulations are sensitive to graupel parameters (which is already known from previous studies). Even with major revisions, I do not believe this manuscript is sufficient for publication.

»> We thank the reviewer for the suggestions. We realized that we were not effective to explain the rationale of our work and to convey the results of our analysis. We hope that our detailed replies, listed below, will help to clarify our point of view and to improve the quality of the revised paper. We note that the aim for using two different NWP models was basically to investigate the response of different models to the same changes in the precipitating high-density hydrometeor (named “graupel”) parameters. The results show that a sensitivity analysis of microphysical parameterization may be significantly dependent on the NWP model once ensured that the “best” NWP model configuration (with respect to available measurements) is chosen as a reference configuration. »> A revision of the paper giving explanations or speculating on the reasons for the differences between the models has been carried out in the revised paper.»>

SPECIFIC COMMENTS 1. It is very unclear why two different mesoscale models have been used. The models give very different solutions for this case and different microphysics schemes have been used. So it is unclear to me how the use of two models helps answer the questions posed regarding the sensitivity to parameter settings for

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graupel. I suppose one could be looking to see if, given different models with different schemes, the same general changes in the model solutions occur when the same changes are made to the graupel parameters, but it is not presented this way in the manuscript, at least not clearly. The authors need to do a much better job at explaining why two models are used and showing that this is useful.

»> We are sorry, we did not stated clearly, but the reason for using two different models was just to investigate the response of different models to same changes in the graupel parameters. We add a sentence for clarify this point in the introduction.»>

2. Although comparison to observations is generally desirable when examining model performance and sensitivities, in this particular study I believe the inclusion of observations does not serve a useful purpose. The simulations using both models are very different from the radar observations, so one cannot make any claims that a given parameter change to the microphysics which leads to a solution that is closer to the observations is thus a change for the better. For example, we have no idea if the vertical motion in the model is accurate, so a change to the microphysics which improves the hydrometeor mass content, compared to the radar estimates, may or may not be due to genuine improvement to the microphysics – but that is what is argued. Unless the control simulations are very close to the observations, which is not the case here, use of radar data for this type of sensitivity study is intrinsically problematic.

»> The referee stated :' Unless the control simulations are very close to the observations, which is not the case here, use of radar data for this type of sensitivity study is intrinsically problematic.' Actually, we believe that if a control simulation is very close to the observation, the sensitivity study is a pure theoretical exercise; on the contrary, if the purpose is to improve the simulation, which may turn in improving weather forecast, a poor weather forecast is the right laboratory to start with.»>

3. A comparison of the model storm cell positions from the various sensitivity runs is done, where the cell location is defined as the location of the peak updraft. But what

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does this show? For one thing, the COSMO storm systems are broad and multicellular, so a change that slightly switches which updraft is stronger does not indicate a change in location of the storm system. More importantly, what does this show? If the location of the peak updraft changes when the assumed bulk graupel density is changed, what are we to take from this?

»> The raincell location analysis as function of the graupel parameters allows to assess the storm structure, as you suggested, and the robustness of the model dynamics. We would expect a small variation in the cell location in term of updraft strength and not a widespread distribution if the thermodynamics is correctly reproduced. In this study, the two NWP models clearly produce a quite different response in term of cell location. This would suggest a weakness in one of the NWP models.»>

4. The spectral analysis of the hydrometeor fields confuses me. I have never seen this done before – if it has been, references should be given. I do not understand what is to be learned from this (admittedly, this may be my own fault, but I think it needs to be either better explained or referenced). Perhaps if the model storms were actually similar to the observed storms – which they are not – then we could conclude that the spectra which better match the radar-deduced (not "observed") hydrometeor Energy spectra are better. But as is, I fail to see how this section addresses the goals of the study.

»> The purpose of the spectral analysis was to objectively compare the models results. But we agree it is somehow confusing: therefore we have decided to cut this part.»

5. One thing that strikes me as being conspicuously absent from the conclusions of this study is the following. The radar particle type retrieval indicates that there is both hail and large amounts of graupel in this storm. The authors argue that their sensitivity tests, and those of other studies, indicate that better solutions are obtained with more hail-like parameter settings for the graupel category. Surely the obvious conclusion is that a bulk microphysics scheme should include separate categories for graupel and

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hail.

»> Also in the conclusion we probably did not clearly stated the finding of the study. As you suggested an already known is:'Surely the obvious conclusion is that a bulk microphysics scheme should include separate categories for graupel and hail'. We add a sentence in the conclusion. But what we believe is important and quite new is this kind of comparison with the observations: hydrometeors vertical structure and not only the radar reflectivity or rain at the ground.»>

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 20461, 2010.