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Geoscientific Model Development Discussions

Interactive comment on "Influence of microphysical schemes on atmospheric water in the Weather Research and Forecasting model" *by* F. Cossu and K. Hocke

Anonymous Referee #2

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The manuscript entitled "Influence of microphysical schemes on atmospheric water in the Weather Research Forecasting model" studies the effects of different options to parameterize microphysics processes on topographically-induced precipitation in a regional climate model through idealized simulations. Although the effects of parameterizations on the model performance have been widely studied in the past, the authors provide an interesting approach to the problem and make a useful contribution to the field. The paper is clearly written and well structured. I would recommend its publication after some minor remarks are addressed.

Minor comments

1. P4565 L25-29 Although it has not been examined in the manuscript, I would also mention the importance of feedbacks between different schemes, which are often a dominant factor in the model performance (see also next comment)

2. P4566 L21-23 (and also PG 4567 L6) Far from being a disadvantage, the inclusion of these non-linear interactions is a must in any sensitivity test because the schemes never act independently but in combination with others. Therefore, it is impossible to isolate the effects due to a particular parameterization (e.g. microphysics) because its performance strongly depends on the other scheme options and they cannot be studied as separate entities. The results here presented are also affected by these feedbacks and could be completely different if an alternative, let say PBL, was chosen; and the differences cannot be only attributed to a particular scheme. The study of various combination of schemes should be regarded as a strength instead of a disadvantage. I understand that computational resources often limit the number of experiments that can be performed, but the authors should not present this limitation as an asset of their work. This should also be mentioned in the conclusions

3. P4566 L23 The term "ideal" should be avoided in this context because it is often the case that no configuration outperforms the others in all circumstances.

4. P4568 L1 The authors emphasized the climate component of their study and then used "long term" to describe the period studied. From a climate point of view, this is not exactly a long-term simulation.

5. P4570 L25. How is the surface temperature determined? Does it not change with time at all? Please, specify.

6. P4572 L9 . The authors should provide examples of how the water mass could be lost. More generally, examples of mechanisms (e.g. numerical) that could invalidate the water mass conservation.

7. P4572 L20 to P4573 L9. (Also Fig 3). The first hours of the simulations and the

processes taken place in that period are a direct consequence of the model spin-up. The model adapts the initial conditions to its internal dynamics and thus the results during the first hours should be interpreted carefully (this applies to all other variables). The authors should at least mention this caveat.

8. P4574 L21-27 This is a highly ideal set-up (e.g. periodic boundary conditions) and thus it is difficult to compare with observed values in reality. This comparison does not really add much to the study. In addition, it is not clear to me why the presence of the (idelized) mountain is responsible for such low values with respect to reality, where mountains also play a similar role. The fact that the simulation has many prescribed and idealized features is likely to be the dominant factor instead.

Interactive comment on Geosci. Model Dev. Discuss., 6, 4563, 2013.