The authors are thankful to Anonymous Referee #2 for the comments provided. In the following, we answer the comments point by point. The original comments from Referee #2 are reported in bold font and our answers in normal font.

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)

We will add this point in the revised manuscript.

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

We agree and we will rewrite some parts of the text in Sect. 1 according to these suggestions. We will also change the conclusions accordingly.

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.

After the changes made for the previous point, this sentence will be no longer present.

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.

We will rewrite the sentence as follows:
"In order to enhance the differences between the microphysical parameterizations and ..."

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

The constant surface temperature profile is a consequence of the idealized simulation which doesn't allow the proper initialization of the LSM. We observed similar problems also for other variables computed by the LSM, such as the temperature of the soil at different depths.
We will add a sentence in the revised manuscript to mention this drawback caused by the idealized simulation.

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.

The cause of this water loss could be a numerical truncation error, due to the approximation of the derivatives in the microphysics equations with finite differences, or simply a code bug (new bugs are discovered at every WRF release).
We will add this clarification in the revised manuscript.

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.

We agree and we will modify the text in Sect. 3, mentioning the model spin-up and removing the description of the first hours of the simulation. Furthermore, we will mention the model spin-up also in Sect. 4.1 and in Sect. 4.3. Lastly, we will change the paragraph corresponding to P4576 L14-16 of the discussion paper.

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.

We agree that in the real world mountains play a role similar to our idealized mountain, however in the real world water vapour is replenished by new moisture transported by the wind. In our simulation the periodic boundary conditions inhibit the entry of new moist air in the domain. The only external source of water vapour is the surface evaporation, which allows to balance the loss of water vapour only in the last days of the simulation.
In the revised manuscript we will remove the comparison with real observed values and we will try to better explain why the idealized mountain and the periodic boundary conditions are responsible for the drastic reduction of water vapour.