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Comment

## ***Interactive comment on “Downslope windstorm in Iceland – WRF/MM5 model comparison” by Ó. Rögnvaldsson et al.***

**Anonymous Referee #1**

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### General Comments

The main focus in the work entitled "Downslope windstorm in Iceland - WRF/MM5 model comparison" is on the dynamics of a specific windstorm (also discussed in OA-07), as represented by two numerical weather prediction models. More specifically the authors discuss the comparison of two numerical weather prediction models which are forced with the same boundary conditions, and where the models are configured in as similar way as possible. Further, the comparison of the two models, with two different PBL schemes are discussed before the sensitivity to six different microphysical parameterizations available in one of the models (WRF) is investigated. The model comparisons are supported by measurements from a few surface stations which also are used for the model validation.

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The paper addresses the interesting issue of downslope windstorms, a phenomenon which is quite common in Iceland. Since these episodes often are connected to heavy precipitation and very strong winds, it is of crucial importance to test the models ability to reproduce them and to find the sensitivity to different physical parameterizations. The present work has contributed to increase our knowledge of modeling downslope windstorms, to identify the sensitivity to the model configuration for the present case as well as revealed important parameters to include in field campaigns of such phenomena.

The manuscript is general good, and it is well written and relatively easy to read, and it is an appropriate contribution to Atmospheric Chemistry and Physics. The sensitivity to micro-physics scheme, in Section 3.1.2., gives a good illustration of the gain which can be obtained from using more sophisticated parameterizations in numerical modeling. The authors also conclude that downslope windstorms seem to be sensitive to the microphysics.

As the authors say the research on downslope windstorms in Iceland has been limited until recently. Anyway, the manuscript would benefit from including a somewhat more comprehensive discussion about the theory on downslope windstorms, including some more references. It would also have been preferable if it was clarified to which extent the present work is an extension of the OA-07 paper.

The model set-up of the experiments is included in the introduction and Section 3. The models are neither described in sufficient details, and a supplemental section with model description and model set-up, where the differences between the two models and especially between the PBL schemes are needed. Also a more extensive description of the new two equation scheme should be included. The "model, set-up" section should include the following points:

- a)WRF and MM5 differences?
- b)ETA/MYJ identical in WRF and MM5?

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-give a description of the ETA/MYJ scheme in the same way as done for the microphysics in Section 3.1.1

c)New 2 equation PBL scheme

- describe in more details

-has it been used/described before?

-will it become one of the options in WRF?

d)Microphysics (as in Section 3.1.1)

Further the text lack a discussion on the resolution of the flow in this very complex topography. Can for instance the relatively large differences in wind speed seen in Fig. 4 be due to large spatial variability and poorly resolve dynamics?

The text should also include a discussion on the feedbacks between the physics and the dynamics, as well as some more speculations/explanations about the findings.

Despite all the interesting readings I found in this work, the authors should spend some more time on the motivation part, and ensure that they answer questions like: What is the main idea behind this work? Why has it been done? Why has this data set/model been selected? Why has this method and specific parameterizations been selected? What is the main contribution from this work?

If the above recommendations are met by the authors, I can recommend this paper for publication in Atmospheric Chemistry and Physics.

SPECIFIC COMMENTS:

1)Abstract and text. The use of name for the PBL scheme (ETA and MYJ) should be clarified and consistently used (ETA for MM5, MYJ for WRF or ETA/MYJ for both).

2)Define "near surface wind" (first model level (xx m agl) or 10m wind?)

3)Page 3. Is there any significant difference between the MM5 version used in OA-07

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and the V3-7-3 version used in this paper?

4)Page 3. The footnote should be included in a section describing the model.

5)Page 3. The name Freysnes should be included in Figure 2.

6)Page 3 and 4: could it be reasonable that the boundary conditiions from MM5 (3km) could be responsible for some of the observed differences. (For some model runs the MM5(3km) is configured in the same way as in the present work, but for most runs there will be differences)

7)Page 4. Synoptic overview. Include ref. to OA-07

8)Page 4. The sentence explaining the objective should be clarified, especially... caused by the differences in the numerics of the two models, and what about the PBL schemes?

9)Page 4. The highest peak exceeds 2100 m.... What about the model topography which must be much smoother?

10)Page 5. Both models correctly simulate the dry area downstream of M.O - include station name.- but tend to overestimate the precipitation on the windward side - include station name

11)Page 5. Greater should be substituted by higher?

12)Page 11. Spell, autovonverting should be autoconverting

13)Page 11. ...convert ice cloud to snow and THEN.....

14)Page 21. Define near surface wind, Figure 3

TECHNICAL CORRECTIONS, Figures and Tables:

1.Include definition of Brunt-Vaisala frequency, Table 3

2.Fig.2, 3 and 7. If possible, improve the quality of station names (bold, black, only two

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letters?) and increase the line thickness (A-B)

3.Fig. 4. Line colour should be blue (not black) for MM5 ETA, according to the figure text.

4.Fig 8. Could the location of station SKAFT be plotted?

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 6437, 2008.

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