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Atmospheric Chemistry and Physics Discussions



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

**Anonymous Referee #2**

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## General evaluation

The authors present a model sensitivity study on a downslope windstorm case observed in Iceland, comparing two models (MM5 and WRF), two different PBL schemes for both models, and six microphysics schemes for WRF. There are some results that have the potential to become interesting once they are properly discussed, but the paper in its present state is not suitable for publication because of severe technical and scientific deficiencies. Most importantly, the whole manuscript is very confusing because many different simulations are discussed at the same time without proper structuring, and there are numerous discussion items that are either questionable or speculative. Specific comments follow.

## Specific comments

1. The references cited in the introduction appear to be selected quite randomly and do not provide an appropriate background for the work presented in this paper. Though there might be few published papers on downslope windstorms in Iceland, there is a huge number of high-resolution modelling studies on similar phenomena in other mountainous regions, at least some of which need to be discussed to put the work presented here into a proper scientific context.

2. Introduction, 2nd para: The theory by Smith (1985) does not predict flow over vs. flow around because it is 2D. However, the restriction to uniform wind speed and stability has been relaxed by a number of follow-up studies.

3. Introduction, footnote 1, and related text: Given the fact that the authors use a completely new PBL scheme, a more complete description than just citing two equations is needed (or, alternatively, a reference in which the new two-equation turbulence scheme is described and validated).

4. A section describing the setup of the MM5 and WRF simulations is missing. Although some information about variation in physics parameterizations is scattered through the manuscript, it remains completely unclear which physics options have been used as reference in MM5 and WRF, respectively, and if there are essential differences between the reference setups of MM5 and WRF. For example, which microphysics options have been used in MM5 and WRF (as reference), are they reasonably similar to each other, is the domain configuration the same, what about radiation and cumulus schemes, etc. etc. ?

5. The whole discussion in section 3 is very confusing because too many simulations are discussed promiscuously, and it is often unclear which discussion items refer to one of the figures and which provide additional information. Some specific issues:
Why is the impact of the new 2-equation PBL scheme opposite in MM5 and WRF,



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compared to the respective reference PBL scheme? As far as I know, the WRF MYJ scheme is an advancement of the MM5-ETA scheme, so it is not clear why they should behave so differently.
Why are the 2-m temperatures essentially different between WRF and MM5?
It is not clear which corner wind the authors are talking about.
It is not evident from Fig. 3 that the WRF simulations have a stronger wind at FAGHO than the MM5 simulations, which would be required for them to be considerably better (as stated in the text).
In Fig. 5, significant TKE extends up to the 304 K isentrope in both MM5 simulations, whereas the text states that TKE is confined below 286 K in MM5/2EQ and below 289 K in MM5/ETA.
Generally, the relationship between the differences in the TKE field and the differences in the wind field remains largely unclear.

6. Section 3.1: While a general description of the model setup is missing, far too much detail is provided on the microphysics schemes, given the fact that this paper focuses on downslope windstorms and not on orographic precipitation. On the other hand, the reader is still not informed which microphysics scheme is used as reference option in WRF. The description of the microphysics schemes should be restricted to the relevant points, and it should be structured according to the complexity of the schemes rather than their number in the WRF namelist. Apart from that, it is unclear why the cloud microphysics schemes have been varied only for WRF but not for MM5. MM5 has a similar number of microphysics options.

7. Section, 3.1.2, 2nd para: The authors state that the effects of increased complexity in the microphysics schemes are clear. I don't agree with this statement. For example, it is not clear to me why the simplest scheme (Kessler) produces a precipitation field very similar to Lin and WSM6, whereas WSM3, having the same number of microphysics variables as Kessler, produces a completely different field with a dominant maximum over the lee slope and a weaker maximum over the windward slope.



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8. Section 3.1.2, 3rd para: The authors state that no cloud ice was simulated with the five and six class microphysics. This is hard to believe, because something must have initiated the snow appearing in Fig. 9.

9. Section 3.1.2, last para: Here, the essential difference between the Thompson simulation and the other experiments is discussed. Unlike the other schemes, the leeside downslope flow does not separate from the ground in the Thompson simulation, leading to lower humidity in the lee region. The important question is, however, why flow separation does not occur with this scheme. Is it because there is less spillover of precipitation and therefore less evaporative cooling, which is known to damp gravity wave activity in the lee of mountains? This could be tested with sensitivity experiments in which evaporative cooling is turned off in the innermost model domain (or, alternatively, in the lee of the mountain only).
In addition, one wonders how MM5 behaves with Thompson microphysics, in case this was not the reference option in MM5.

10. Section 4, 1st para: Once again, as MM5/ETA and WRF/MYJ are similar parameterizations, it is not clear why they produce so different results.

11. Section 4, 3rd para: Why should the upstream flow *direction* depend on the PBL scheme?
Later in this paragraph, the authors report that the precipitation field obtained with the Kessler scheme is similar to that of WSM6, Lin and Thompson, which is then stated to be in agreement with the results of Miglietta and Rotunno (2006). Just a few lines later, the authors say that Miglietta and Rotunno obtained very different rain rates with Kessler and Lin. This is a clear contradiction! What is correct now, and what is the relevance for the present study? Also, the relevance of the autoconversion thresholds for the differences in downslope windstorm behaviour does not become clear.

12. Section 4, last para: The argument that different precipitation distributions cause variations in upslope static stability is unconvincing. Effective static stability mainly



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depends on whether condensation takes place or not, and the condensation process is treated in a very similar way in all microphysics schemes (instantaneous removal of supersaturation). Apart from that, microphysics may have a significant impact on stability in case of multiple condensation and evaporation, but this is not very likely to be the case because the mountain under consideration is an isolated peak protruding into the ocean.

13. Section 5, 3rd para: Here in the summary, the authors state that in the Thompson simulation, "‘the lifting of an upstream isotherm layer from mountain height to about 1.3 times the mountain height leads to a significant increase of the downslope windstorm"’. Nowhere in the paper this has been discussed, let alone demonstrated. In fact, Fig. 8 shows a very similar nearly-isothermal layer for the Kessler scheme simulation, which is also lifted over the mountain but obviously does not lead to a downslope windstorm. As already pointed out in comment 7, the authors should check if leeside evaporation is a relevant factor for the difference in flow dynamics.

14. Section 5, last para: The statement that the advanced numerics makes WRF more suitable for simulating downslope windstorms than MM5 is a pure speculation that has nowhere been discussed. Throughout the paper, the authors argue that the PBL and microphysics parameterizations are the most important components.

15. Table 1: Why is the impact of the PBL scheme on precipitation an order of magnitude larger in WRF than in MM5?

16. Figure 10(a): Why does the Kessler scheme produce so tremendous precipitation amounts in the lee of the mountain whereas all other schemes look plausible?

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