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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.

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Reviewer 1

Question/comment #1

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.

Answer to QC#1

The introductory part of the paper has been greatly enhanced and now addresses

these issues in a comprehensive manner.

Question/comment #2

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?

-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)

Answer to QC#2

The paper has been restructure according to these suggestions and an official reference for the two equation (2EQ) scheme has been added.

It is the wish of the authors that the new 2EQ scheme will be made part of the official WRF package. Work is being done to prepare the code so it fulfills the necessary requirements and hopefully the 2EQ scheme will be available in V3.3 of the AR-WRF model to be released in the spring of 2011.

Question/comment #3

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?

Answer to QC#3

A bug was found in the original WRF/2EQ code that resulted in considerably different surface winds (less wind speed) from the WRF/MYJ configuration. This bug has now been fixed and the simulated wind field from the two schemes is very similar.

Question/comment #4

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.

Answer to QC#4

The discussions part of the paper has been extended considerably and now includes a more comprehensive discussion about the results.

Question/comment #5

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?

Answer to QC#5

The introductory part of the paper has been rewritten and now includes sentences where these issues are addressed.

Question/comment #6

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).

This has been done.

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

The text now includes explenation of the term “near surface winds” → winds at lowest half sigma level, approximately 40 meters above ground.

3)Page 3. Is there any significant difference between the MM5 version used in OA-07 and the V3-7-3 version used in this paper?

The only difference in model setup is that the current version uses the RRTM radiation scheme instead of the CCM2 scheme used in OA-07. Other differences between the two model versions are bug fixes.

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

A technical document has been published by NOAA that describes the 2EQ scheme in full detail. That document is now the official reference for the 2EQ scheme and is listed as such in the Reference section of this paper.

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

The location of Freysnes coincides with that of station SKAFT. This is now stated in the figure caption.

6)Page 3 and 4: could it be reasonable that the boundary conditions 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)

The input for the initial and boundary conditions is the same for all simulations, MM5 and WRF alike. Hence, we do not believe that the differences between the simulations stems from the input data.

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

The reference to ÓÁ-07 has been added.

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?

The objectives of this study has been clarified in the introductory part of the paper. As stated in the Introduction, the objective of this study is twofold. Firstly, to investigate the differences in the simulated dynamics of the downslope windstorm that are caused by the differences in the dynamical cores (including numerics) of the two models. The second objective is to investigate the sensitivity of the simulated downslope windstorm to different micro–physics schemes available in the WRF model.

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

The Mnt. Öræfajökull peak reaches 1920 m.a.s.l. at 1km resolution, this information has been added to the "Experimental setup" section.

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

The text has been modified and now contains a more detailed description of the results.

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

Modification done.

12)Page 11. Spell, autovonverting should be autoconverting

Correction done.

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

Correction done.

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

Definition of surface winds included in the figure caption (now Fig. 6 in current version).

Answer to QC#6

Question/comment #7

TECHNICAL CORRECTIONS, Figures and Tables:

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

The table caption now includes the definition of the B-V frequency.

2.Fig.2, 3 and 7. If possible, improve the quality of station names (bold, black, only two letters?) and increase the line thickness (A-B)

We now believe the figure quality is adequate.

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

Line color is now in accordance with figure caption.

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

This has been added to the figure.

Reviewer 2

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.

Answers to general evaluation

The paper has been restructured, taking into account comments and suggestions made by two anonymous reviewers. We believe the revised paper is free of the structural deficiencies hampering the earlier version.

Question/comment #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.

Answer to QC#1

The introductory part of the paper has been rewritten and now provides the necessary

background for present findings.

Question/comment #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.

Answer to QC#2

This sentence has been removed from the revised version of the Introduction.

Question/comment #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).

Answer to QC#3

There is now a technical paper, published by NOAA, describing the two equation PBL scheme. In the revised version of our paper, this is the reference for the two equation scheme.

Question/comment #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. ?

Answer to QC#4

The paper now contains a section focusing on the experimental setup. In this section there are subsections describing the various models and schemes in considerable detail.

Question/comment #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, 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 289K in MM5/ETA.

Generally, the relationship between the differences in the TKE field and the differences in the wind field remains largely unclear.

Answer to QC#5

1. A bug was found in the WRF/2EQ scheme that lead the scheme to produce

opposite results compared to MM5/2EQ. This bug has now been fixed and the schemes behave in a consistent manner.

2. The lee side winds in WRF are stronger than in MM5 resulting in greater horizontal extent of warm winds from aloft. This explains the warmer 2meter temperatures at location SKAFT in the lee of Mnt. Öræfajökull.
3. The corner wind being referred to is the flow speed-up at the southern edge of Mnt. Öræfajökull. This has been clarified in the new version of the paper.
4. An additional figure has been added and the text has been modified accordingly.
5. The time at which the TKE was confined below 286 K (MM5/2EQ) and 289 K (MM5/ETA) was 03UTC. Figure 5 (now Fig. 8) is however valid at 06UTC. This discrepancy has been clarified in the current version.

Question/comment #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.

Answer to QC#6

The objectives of this study has been clarified in the introductory part of the paper. As stated in the Introduction, the objective of this study is twofold. Firstly, to investigate the differences in the simulated dynamics of the downslope windstorm that are caused by the differences in the dynamical cores (including numerics) of the two models. The second objective is to investigate the sensitivity of the simulated downslope windstorm to different micro–physics schemes available in the WRF model.

Question/comment #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.

Answer to QC#7

It is stated that the effect of increased complexity within the three WSM (i.e. WSM3, WSM5, and WSM6) schemes is evident, not all six micro-physics schemes. This has been made clearer in the current version.

Question/comment #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.

Answer to QC#8

This sentence has been removed from the revised version.

Question/comment #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, lead-

ing 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.

Answer to QC#9

A "dry" simulation was done, and the results constitute an additional part of the paper.

The Thompson scheme, as implemented in version 2.2 of the WRF model, is not available for MM5. Rather, we use it's predecessor, the Reisner2 scheme.

Question/comment #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.

Answer to QC#10

Having fixed the bug found in the earlier WRF/2EQ code, this discrepancy is no more.

Question/comment #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.

Answer to QC#11

This sentence has been removed from the paper.

The difference in rain rate between Kessler and Lin et al., found by Miglietta and Rotunno, is a function of mountain height. For low mountains the rain rate differs, but for 2000 metres high mountains (as is the case in our study) the rain rates become similar. This is now stated clearer in the current version.

Question/comment #12

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

Answer to QC#12

The discussion part of the paper has been greatly enhanced. The authors believe that the shortcomings pointed out by reviver #2 have been amended.

Question/comment #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.

Answer to QC#13

This sentence has been removed from the revised version.

Question/comment #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.

Answer to QC#14

This statement has been removed from the current version of the paper.

Question/comment #15

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

Answer to QC#15

Again, this was due to the bug found in the earlier version of the WRF/2EQ scheme.

Question/comment #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?

Answer to QC#16

The Kessler scheme does not produce much precipitation in the lee of the mountain, rather it has characteristics similar to that of Lin et al. and Thompson (most of the precipitation falling on the windward side). It is the WSM3 scheme that produces the most leeward side precipitation, the reason for that is discussed in section 4.2 in the revised vesion of the paper.

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