REPORT C147-2013

1. Looking at the simulation grid domain (Fig. 11), I notice that the East boundary drops in an area partly characterized by highly complex orography, that is the Italian Alps. This may lead to problems in the simulation and numerical instabilities. Did the authors check this aspect and could they comment on it?

The domain presented in the paper has been chosen to be the largest possible to be initialized with the AROME analyses. It corresponds to the domain chosen for the operational Météo-France model AROME from 2008 to 2010 and that has been extensively validated as described in Seity et al. (2011). Moreover, the Meso-NH model has been successfully used over domains with complex terrain near the boundaries (Drobinski et al. 2007; Richard et al. 2007).
We made a number of tests on another situations for which we used different initial conditions (ARPEGE or ECMWF analyses) and a larger domain covering the whole Alps ridge (the East boundary was located at Budapest). The results on wind shear prediction over the Nice airport were found similar to the 27 March 2009 situation presented in the paper.

2. The grid resolution is 2.5 km, this is considered a relatively coarse resolution when dealing with complex orography, like also the Nice airport site experiences (complex terrain and coastal location). Several studies […] proved that in highly complex orography certain terrain-induced meteorological processes cannot be captured at resolutions coarser than 1 km. Given the local characteristics of the windshear effects, this issue should be addressed. Only in the conclusions the authors comment about runs performed at 500 m resolution. I think this should be anticipated and better discussed already in this section.

A part has been added in the subsection 4.1 to discuss the effect of high resolution over wind shears simulation.

3. It is not fully clear to me what the authors mean on page 5 lines 469-473: given that before they specify that initial conditions are taken by AROME operational analysis at the same resolution of 2.5 km, how do they 'couple' Meso-NH with the hourly forecasts from ALADIN? What do they specifically mean with 'coupling'?

The text has been modified as follows:
"The boundary conditions are obtained from hourly forecasts from the coarser scale (9.5~km) limited area model ALADIN (Bubnova et al.,1995) run over a wider domain."

Initial conditions are taken from AROME operational analyses and then boundary conditions during the simulation are taken from ALADIN forecasts. This configuration is compatible with near-real time wind shear prediction at Météo-France since it corresponds to the design of the first operational AROME model.

4. To a non-expert, it is also not fully clear how the 'lidar simulator' operates and how the 'simulated lidar scans' are obtained from the 2.5-km resolution simulation outputs (pages 5 and 6, lines 479-489). Since this is largely used to present comparisons between observations and prediction, a more detailed explanation has to be provided.

At the end of section 4.1 a description of the lidar simulator is given with appropriate references

5. The authors present their interpretation of the shift in space and time. There could be some other causes contributing to this shift and related to the configuration and setup of the numerical simulations, for example (1) a too coarse resolution (see also my previous point 2); (2) a too short

spin-up time; (3) initial or boundary conditions. Did the author investigate these and other aspects? Could they comment on them?

A number of sensitivity tests has been undertaken to interpret the situations. Our first idea was to increase the resolution over the airport. A 500 m resolution simulation domain has been run as a nested domain for both simulations. It was rather small (360 x 200 points) and centered over the Nice airport. It did not lead to a better representation of the wind shear time occurrence over the Nice airport even if the wind shear front was better captured due to a higher grid point density (added in section 4.1).
Our second idea was to modify the initial and boundary conditions and the initialization time. A longer simulation starting from initial conditions 6 hours earlier lead to a reduction of the vortex extension in the 'vortex case' for example. The wind shear prediction is really sensitive to initial conditions and the loss of analysis improvements due to data assimilation conducts to a misrepresentation of the wind shear event. The resolution of the initial condition is also important. The wind shear prediction obtained with a simulation initialized with AROME was better than with a simulation initialized with the global Météo-France model ARPEGE which were also better than a simulation initialized with the ECMWF analysis at coarser resolution.
We also tested the influence of the vertical resolution. A 41 vertical level simulation was compared to a 61 vertical levels simulation. The resolution increase in the boundary layer generates an improvement of the wind structure and a better representation of the wind shear front.

6. Since only one case was considered here, I suggest the authors to be cautious declaring that "However the time and duration of the hazard cannot be provided" (page 6, lines 569-575). This is a single result and does not make a rule.

The comment has been moderated.
"However in the configuration presented in this paper the time and duration of the hazard cannot be anticipated."
However, even if there is a rare event upon two studied, one has not been predicted at the right time and the right place and its duration was not as long as observed. It means that in the configuration presented in the paper, at least one case could not be anticipated.

7. Also in this case (see previous point n. 5) one wonders whether, even when using analysis as initial condition, the relatively short spin-up time (the event occurring only 1.5 h after the start of the simulation) may have affected a correct simulation of the wind velocity.

As stated at point #5 if the simulation starts earlier, the wind shear event over the airport is not predicted.

8. Page 7, lines 613-617: given that the resolution is 2.5 km, it is a bit peculiar that differences in the wind speed could be attributed to the choice of a grid point just a few metres far away (on the sea or on the land) from the observational point. Just a curiosity: was it not feasible to build the simulation grid so to get a point at the observational site?

The sentence has been modified to:
"Both the overestimation of the wind speed and the difference in wind direction can be explained by the fact that the chosen grid point (the nearest) in the numerical simulation is south of the airport in a grid cell covered by ocean having small roughness values."
Given the small scale surface heterogeneities in the surroundings of the airport (mountains, Var valley, coastline) the representativeness of a local measurement with a 2.5 km resolution is particularly challenging.

9. Some quantification, through a statistical analysis, of the agreement between predictions and observations might be worth: this would help avoiding general statements like "The simulation reproduces the wind structure in a satisfactory way" (page 7 lines 642-644).

A quantitative comparison between model forecasts ans observations is indeed relevant. However, the skill of the model would probably not show up with standard statistical measures (correlations, RMS, …). The use of « fuzzy » statistical methods (Ebert, 2008) would be more appropriate but they have not been considered in this study.

10. .... and all of a sudden here the reader discovers that the study presented is 'preliminary', that further studies at a higher resolution (500 m) were performed and by chance the simulation of the orographical effects (fundamental in such complex area and for wind-shear effects!) greatly improved. This is a sort of candid statement, since pushes a reader to ask for the 'final' results here and now.... If the authors want to avoid this because maybe they want to present these 'final' results in another publication, I suggest them to clearly state already in the introduction what are their intentions here and not to surprise the reader at the end.

At first the word 'preliminary' has been removed from the conclusion since it induced misunderstanding. This word was put here to indicate that the study was restricted to only 2 wind shear events and further cases should be investigated in the future to reach more robust conclusions.

Minor Comments
Page 3 lines 205-209: the authors should cite the reference of the Meteo-France internal report, even when possibly not publicly available (specify it in the case).

We have replaced the reference to the report by a personnal communication, since it cannot be made available.

There are some typing and English errors here and there in the text, to be checked.

English errors have been corrected. Some sentences have been rephrased for the sake of clarity.

The figures are a lot, often they are a bit too small to be clear when printed, and one has to zoom on them also in the electronic version.

We made the figures more readable by changing alignment from horizontal to vertical. As a consequence, the size has been enhanced.

REPORT C207-2013

In the introduction I would be happy to have a better definition of the various types of wind shears, some which can be measured by ground-based remote sensing systems like wind lidar and some which cannot (like micro-bursts). If I well understood, the paper focuses on long lasting wind shears (several hours).

The introduction (lines 70-76) and the subsection 2.1 (lines 164-168) have been completed to better describe the type of wind shears study in this paper and the way to observe the type of wind shears occurring over Nice airport.

The 2.5km horizontal resolution of the model does not seem to be adapted to the scale of the phenomena analyzed (low-level wind shears), both for the temporal and the spatial scales.
The conclusion related to 500m resolution tests without improvement did not convince me (lines 685-690).

Our study considers wind shear events that are mainly driven by synoptic scale flows. A part has been added in subsection 4.1 to explain that the 500 m resolution domain improves the simulation of local effects. It also explains that the relative small size of the domain did not allow to study the synoptic phenomena generating wind shears.

To echo back the next comment, a sentence has been added in the conclusion to explain that the 500 m resolution domain is mandatory for data assimilation at high frequency and high resolution.

An essential topic to consider in such analyses is the effect of assimilation of such data into the model in order to improve its ability to describe (and forecast) the phenomena (see reference below), especially in complex topography. The authors just mention it.

This statement is completely true. Data assimilation improves the numerical prediction results. In this paper this result is hidden behind the AROME analysis. When the initial conditions are obtained from a coarser resolution analysis than the AROME analysis the wind shear prediction is worse. (see comment on report C-147). We could improve the wind shear prediction through data assimilation if we were able to assimilate high resolution data at high frequency; for example each lidar scan or the 3 min wind profiles from the wind profiler.
To set up the data assimilation system mentioned in the text (the back and forth nudging algorithm) we wanted to study the ability of the model to reproduce wind shear events.
The conclusion of this paper is that the model is able to reproduce rapid wind direction change even if it is not well located. Data assimilation can then be considered as a solution to improve the prediction of wind shears. We added the reference mentioned by the reviewer to reinforce this statement.

Minor comments:
- Line 100: it is quite presumptuous to determine the capability of a model to predict wind shear on the basis of two events only (one being rare)

The sentence has modified to be attenuated:
"The comparison presented in the following study focuses on the strengths and weaknesses of the model to predict these wind shear events"

- Line 112: in "the following section" instead of "the following of the section"

Done

- Line 202-208: the sentence is not clear to me (10m ?) and the comparison is not very impressive (references to comparisons between wind lidars and other observations should exist…)

A Météo-France study has shown the good agreement between lidar and standard anemometer measurements with a correlation coefficient of 0.949 even with an altitude difference of 10 m.

- Line 254: "value is" instead of "value eis" - Line 260: add "(not shown)"

Done

- Line 321: did the wind profiler show the two layers for this event ?

The sentence has been modified:
"Vertically, the wind flow over the Nice airport can be separated in two parts clearly visible on the wind profiler time series as well as on the vertical wind profiles given by the AMDAR message at 8:37 presented in Figure 8."

- Line 387-389: why ? do you have a reference to explain this fact?

No we did not find any good explanation for this fact and no references either.

- Line572-575: weak statement (see my comment on assimilation and model resolution above)

"It could be profitable in that case to run a model at higher resolution and to assimilate local high resolution data (wind lidar for example) and/or high temporal frequency data (wind profiler) to study the impact on the LLW position."

- Line 612-615: does a 10m difference make a difference on a 2.5km grid model ?

The statement was not precise enough. The sentence has been modified to:
"Both the overestimation of the wind speed and the difference in wind direction can be explained by the fact that the chosen grid point (nearest) in the numerical simulation is south of the airport in a grid cell covered by ocean having small roughness values."

- Figure 8: the simulated and observed wind barbs are on the right!

Done

Références:

E. Ebert, 2008: Fuzzy verification of high resolution gridded forecasts : A review and proposed framework. Meteorological Applications, 15, 51-64

Drobinski et al. 2007: Föhn in the Rhine Valley during MAP: A review of its multiscale dynamics in complex valley geometry. Q. J. R. Meteorol. Soc. 133: 897–916

Richard et al., 2007: Quantitative precipitation forecasting in the Alps: The advances achieved by the Mesoscale Alpine Programme. Q. J. R. Meteorol. Soc. 133: 831–846