

Interactive comment on “Wind shear over the Nice Côte d’Azur airport: case studies” by A. Boilley and J.-F. Mahfouf

Anonymous Referee #1

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The study is indeed of interest, it is carried on with a fairly rigorous scientific approach, the level of details provided in the manuscript is appreciable. However, there are some weaknesses that should be addressed before considering it for its publication. In the following I report some comments and questions.

SECTION 4.1

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?
2. The grid resolution is 2.5 km, this is considered a relatively coarse resolution when

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dealing with complex orography, like also the Nice airport site experiences (complex terrain and coastal location). Several studies [see references in the end] 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 wind-shear effects, this issue should be addressed. Only in the conclusions the authors comment about runs performed at 500m resolution. I think this should be anticipated and better discussed already in this section.

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’?

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.

SECTION 4.2

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?

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.

SECTION 4.3

7. Also in this case (see previous point n. 5) one wonders whether, even when using

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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.

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?

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).

CONCLUSIONS

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.

MINOR POINTS

- 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).
- There are some typing and English errors here and there in the text, to be checked.
- The figures are a lot, often they are a bit too small to be clear when printed, and one

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has to zoom on them also in the electronic version.

CITED REFERENCES

Gohm, A, Zängl, G, Mayr, GJ, 2004. South Foehn in the Wipp Valley on 24 October 1999 (MAP IOP 10): verification of high-resolution numerical simulations with observations. *Mon Wea Rev* 132: pp. 78-102.

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