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Discussions

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Interactive comment on "Numerical simulation of relatively heavy nocturnal rain bands associated with nocturnal coastal fronts in the Mediterranean basin" *by* J. Mazon and D. Pino

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REFEREE 2

We would like to thank this referee for their comments and suggestions to improve the quality of the manuscript.

We answer below his/her suggestions bellow.

1) No evidence is provided about the realism of the simulation: there is no attempt to perform at least a minimal verification against observations;

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The referee is right. In the new version we have included observations to validate the simulations.

2) the onset of convection is attributed to the formation of cold drainage currents, but no direct evidence is provided on this aspect – all currents are tagged as nocturnal, but I have some doubt that in all three cases the nocturnal cooling plays a crucial role – some more investigation, even only using model diagnostics or sensitivity experiments, should be provided;

Please see the related comment below.

3) the conclusions seem a bit too simplistic, also in consideration of the two items above.

We have included theoretical parameters proposed by Miglietta and Rotunno (2010) and Durran et al. (1987) to further analyze the events. The conclusions have been rewritten accordingly.

Specific comments:

- Title: the word "nocturnal" occurs twice – I think it can be used only once (either for rain bands or for fronts) without detriment of clarity.

We have changed it to Mesoscale numerical simulations of nocturnal heavy rainbands associated with coastal fronts in the Mediterranean basin.

- Page 7599, lines 4-7: model validation should not be confused with a verification of the realism of simulations of individual case studies. The first has to do with model quality statistics, the second with the appreciation of the quality of specific model results that may depend on initial analysis as well as on model characteristics. In the case-study framework, I do not think that firm conclusions can be drawn based on pure model output, completely neglecting the observations. In the present case, I understand that it is very difficult to find proper observations over the sea. However, a minimum verification is necessary, at least by presenting a minimum of satellite imagery/data (for example Meteosat IR data and/or TRIMM data that have been taken into account, as mentioned at lines 15 -16).

We agree with the referee. To validate the simulations, we have added the available observations (reflectivity radar and Meteosat images) and estimations from TRMM in the new version of the manuscript. Unfortunately, radar observations for one of the days under study (GEN event) from the Italian national weather service were not available. (Francesco Silvestro (CIMA Research Foundation, Italy) personal communication: “[. . .] the event you requested was during a test period of the system and we have no raw data, so we have no way to produce again that event [. . .]”).

- Page 7600, lines 9-10: please specify which type of ECMWF reanalysis (ERA-Interim? operational?) has been used and at which resolution.

We have specified the type of data used to provide initial and boundary conditions to the model.

-Page 7600, line 14: why an interval of 10 h accumulation has been chosen for all events?

We have chosen this period because it covers the whole night. It is a bit more than the lifetime of the three rainbands, between 6 and 9 hours.

- End of page 7600 and beginning of 7601: the presence of non-weak synoptic winds seems to point to the presence of larger scale disturbance that may have modified or perhaps even caused the presence of the cold currents out of the coast – this aspect should be better discussed in relation to the specific meteorological situation of each event.

We have added a short discussion explaining the meteorological synoptic situation for each event.

-Section 3.2: the formation of convective cells in the model is discussed – is it possible to compare this with satellite images or with TRIMM rainfall - derived values?

We have added satellite images in order to qualitatively validate the simulations for the three studied events

TRMM estimations are also shown in the manuscript. Despite TRRM only provides estimations of the precipitation, WRF results are compared with these estimations.

- Page 7602, line 24: I think that the use of the word "fronts" can be questionable - normally in dynamic meteorology fronts are related to quasi - balanced rotating dynamics (e.g. semi-geostrophic balance), while here the discontinuities are likely to be related to density current/cold pool boundaries (perhaps one could say "density current fronts" – however, "coastal fronts" already denotes this specific phenomenon in the literature, so I am non strict about this point).

To our opinion “coastal fronts” is appropriate in this context because we are referring to “fronts” formed near the coast, which also disappear near the coast. It is true that density currents are the precursor of the studied fronts, but the proposed term “density current fronts” can be also used for phenomena occurring inland as a consequence of large nocturnal cooling. In order to clarify this point, we have added a sentence in the introduction explaining that the studied coastal fronts are formed from density currents.

-Page 7603, lines 12-14: this sentence is dynamically incorrect –density currents have their own propagation speed different form the average ambient speed (and different from both the speed of the warm and of the cold sectors).

We have modified this sentence.

-Page 7603, lines 19-22: this explanation is a bit simplistic – a mountain is fixed and solid, a cold pool is not, and therefore the generated vertical motions are quite different –it can be taken only in a very loose sense. So, criteria established for orographic flows should be applied carefully and qualitatively to these cases. The authors should at least point out the cautions and differences.

We have explained in the introduction the limitations of this model applied to den-

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sity currents. In addition, we have commented the differences between the two approaches.

-Page 7607, Table 1: I do not understand why simulations have been performed for a so long time (90 and 72 hours) compared with the short duration of the convection and of the precipitation (less than 10 hours). Still, related to this point the time of the initial condition for each simulation is not specified in the paper (or did I miss it?).

Simulations include a spin up period of at least 24 hours. Consequently, we start the simulations 24 h before the time in which rainbands are detected in TRMM and/or reflectivity and satellite images. Moreover, by so doing, we are able to analyze when the convection starts and whether it is associated or not to the nocturnal drainage flow.

Due to a similar reason, simulations finish at least 24 hours after the rainbands events, in order to analyze how rainbands extends during the morning, and if they are present the following night. Then, we can certify that onset convection is associated to the rainband formation, starting when drainage winds start, and finishing when drainage winds disappear.

Date start for each simulation is located at the beginning on the second column in Table 1.

-page 7602, Fig. 2b: I am not convinced that in this case the N-NW flow to the north of the precipitation line is of purely "nocturnal" nature, considering that there is a considerable mountain barrier (about 700-1000 m high)near the coast of the west part of the Gulf of Genoa. In my opinion, the orography induces a katabatic flow mainly in response to the presence of a relatively deep layer (probably hundreds of m) of cold air over the Po valley to the north –this is at least a common occurrence. But I cannot exclude that the nocturnal cooling adds a contribution to the orographic katabatic flow.

We agree with the referee that the orographic barrier surrounding the coast in the Gulf of Genoa may induce a katabatic flow, This flow contributes with the drainage air to move the inland cold air offshore though the Po valley and other streams. However, drainage flow is a generic concept, which includes katabatic flows (see AMS glossary, 2011). Both katabatic and "drainage" flows contribute to form a line of convergence offshore, where a rainband forms. Figure 1 below shows the convergence pattern at 23 UTC on 29 January 2008 and Fig. 2 at 03 UTC on 30 January 2008. According to the simulations a convergence line is formed in a few kilometers offshore.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 7595, 2013.

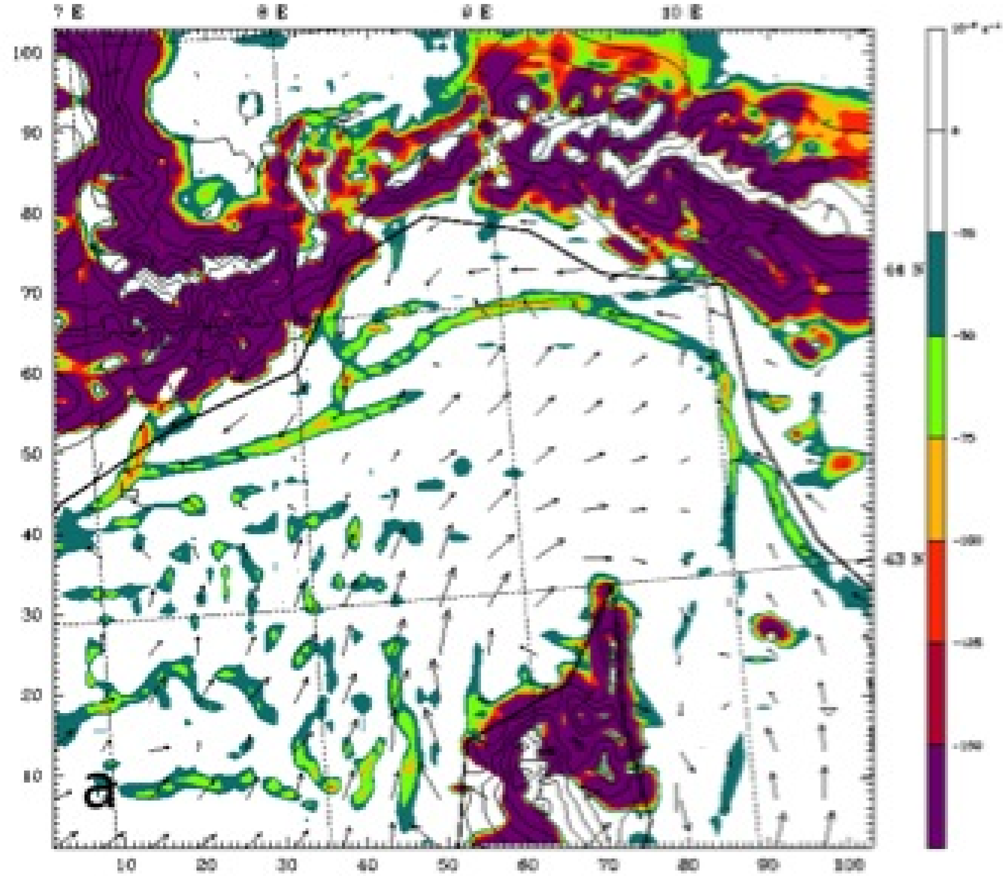

Fig. 1. Simulated negative divergence (convergence) at (a) 23 UTC on 29 January 2008

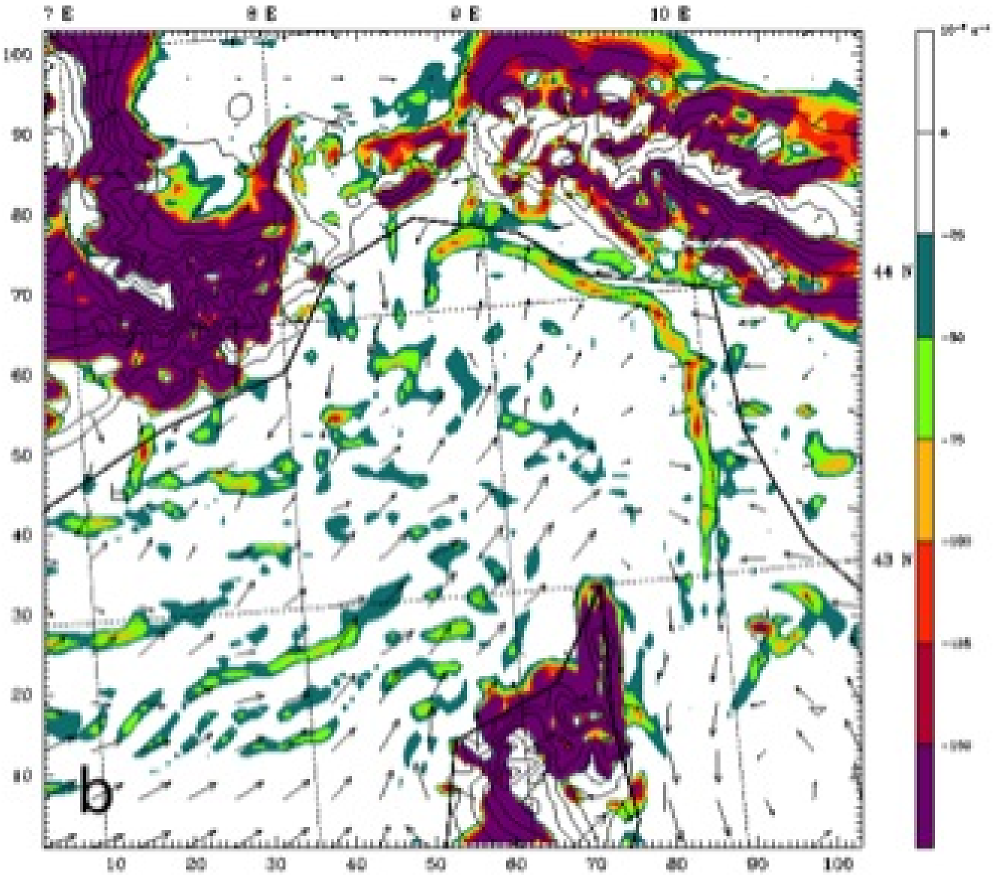

Fig. 2. Simulated negative divergence (convergence) at 03 UTC on 30 January 2008 for the GEN event.

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