Review of the paper "Numerical simulations of relatively heavy nocturnal rain bands associated with nocturnal coastal fronts in the Mediterranean basin", by J. Mazon and D. Pino.

General comments:

This paper investigates, using numerical tools, the formation of rainbands associated with cold ("drainage") currents over different coastal areas of the Mediterranean. This phenomenon is not well know, partly due to the fact that monitoring precipitation over sea areas is problematic. Therefore most of the studies have been devoted to precipitation over land, while the occurrence of sea convection has received comparatively less attention and therefore is worth of further investigations, also because, as in the events discussed in this paper, marine convection is often associate with non obvious dynamical features and also because it can contribute to modify the atmospheric environment over the sea. This paper is generally well written and with a clear presentation of the material, although in a quite concise form. However, I think it is not worth of publication on NHESS in the present form, for basically the following three concerns:

1) No evidence is provided about the realism of the simulation: there is no attempt to perform at least a minimal verification against observations;
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;
3) the conclusions seem a bit too simplistic, also in consideration of the two items above.

In conclusion, in my opinion this paper requires a major revision, needing the insertion of some additional observational evidence and of a somehow deeper analysis on the formation and dynamics of the cold currents triggering the convection. More specific comments are added below.

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.
- 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).
- Page 7600, lines 9-10: please specify which type of ECMWF reanalysis (ERA-Interim? operational?) has been used and at which resolution.
- Page 7600, line 14: why an interval of 10 h accumulation has been chosen for all events?
- 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.
- 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?
- Page 7602, lines 10-12: "a horizontal gradient of potential temperature...".

- 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).
- 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).
- 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.
- 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?).
- 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.