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Natural Hazards and Earth System Sciences
Discussions

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 1

We would like to thank this referee by the exhaustive revision of this manuscript, which improves the quality of this investigation. We thank the suggestions done, as well as the misleading English statements.

We answer below his/her suggestions.

âĂć SPECIFIC COMMENTS

1) The sentence that appears in the first paragraph of Page 5 is a critical -and funny one. The authors declare "Taking into account that we are not interested in validating the mesoscale model, no comparison between observations and numerical results is made throughout the paper". This lack of interest might be the case for the authors, but this reviewer and any potential reader of the paper will certainly like to see some sort of comparison between the simulations and observations. An initial validation of the simulations is an essential step in this kind of studies before exploiting the great potential offered by the numerical tools for an improved physical understanding of the phenomenon under analysis. At these fine-grid resolutions, perfect simulations in terms of the spatial and temporal evolution of the fields can not be expected, but at least we should demand that the general pattern of the episodes is successfully captured by the model. The authors have, at a minimum, the opportunity to show the numerical results of Figs. 2-4 along with the TRMM satellite data they used to identify the events and possibly some additional material like radar data or observed rainfall at coastal stations. Addition of observations would not only justify the use of the simulations for the kinematic and thermodynamic characterization of the rainbands, but would also serve to better document these interesting case studies.

We agree with the referee that the sentence was not very fortunate and we have removed it. Additionally, we compare simulated rainbands with TRMM estimations. In the new version we have added the TRMM data for the three events. However, it is important to remark that TRMM multi-satellite only records the precipitation field for latitudes below 37 o, and estimates it by using a mathematical algorithm for latitudes higher than 37o. Consequently, when comparing WRF simulation with TRMM “observations” we are not comparing the model with observations, but estimations.

Besides TRRM data, in order to further validate the simulations, we have also included radar and satellite observations. For the IP event, we have added the reflectivity radar images from the Spanish Weather Agency, and the Meteosat satellite in the IR channel.

For the ISR event, we have added a Meteosat image, a reference where the reflectivity

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radar image from the Israeli weather service can be observed. We couldn't obtain any image of this event to be used in the paper.

For the GEN event reflectivity radar images and Meteosat images are not available for the studied days. (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 [. . .]"). We have also added an image from Meteosat satellite.

2) The link between the conceptual model of Miglietta and Rotunno (2010) explained at the beginning of Page 3 and the topic investigated by the authors becomes unclear till the last paragraph of Page 8. You are dealing with maritime rainbands whereas Miglietta and Rotunno (2010) dealt with precipitation over mountains. You are defining H (the coastal front depth) whereas MR2010 defined h (the height of the mountain). The physical analogy between both phenomena and the equivalent role of H/LFC and h/LFC are not established till Page 8, so the reader would interpret the discussion of MR2010 in the Introduction as out of context. Accordingly, first paragraph of Page 3 and/or last paragraph of Page 8 must be appropriately rephrased to better -and earlier- guide the readers through the physical analogy applied in the paper.

The referee is right. We have modified the explanation.

3) The second paragraph in Page 2 is atypical as it merely provides a long list of related references but no conclusions or relevant findings made in those studies. To enhance the value of this bibliographical revision, please provide for each or some of the references additional elements beyond a simple list of authors / region / phenomenon.

We have enlarged the state of the art of the subject.

4) Several physical interpretations or definitions made through the manuscript need revision or further clarification and, in some cases, additional supporting diagnostic products and references. Namely:

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- First paragraph in the Introduction: the use of the H/LFC ratio would suggest that these two parameters are defined/calculated from a same atmospheric column, but I understand H is defined as the depth of the colder inland air mass while LFC belongs to the warm, moist, unstable maritime air mass. So there seems to be a spatial dislocation between H and LFC, contrary to h and LFC in the Miglietta and Rotunno (2010) where a single air mass is considered. Is this interpretation correct?

The interpretation is right. This point has been clarified in the introduction.

- Last sentences of first and second paragraphs of 3.2: dissipation of the rainbands is explained in terms of the weakening of the convergence lines only. That may be seen as an over simplistic assumption, since other factors in addition to the triggering mechanism might also be involved in the dissipation of the rainband (e.g. convective instability depletion, surface horizontal spread of convective downdrafts, etc...). Additional diagnostic products would help to better isolate these influencing factors.

The referee is right, and some new parameters have been introduced in the diagnosis. The discussion has been focused on the triggering parameter, the blockage effect of cold front to prevailing flow, and the deceleration induced by cold front on the upstream flow. A description of these new parameters has been added in the introduction.

- Page 8 (L22-23) and Page 9 (L19-21): There are well established theories explaining the transverse circulations forced across other types of density fronts. The cross sections shown in Figure 5 recall, in fact, the dynamical structure of a density current.

The referee is right. New references about this subject have been included

- Near the end of second paragraph of the Conclusions, on the causes of the quasi-stationarity of the rainband: Again, this interpretation seems rather simple, as it ignores the effects on the line movement by convective progagation, vertical wind shear, etc...

We have removed this sentence, and argued based on the estimated parameters.

- Section 3.3: The criteria used to calculate the H and LFC parameters that define the

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triggering parameter are poorly defined and it would seem these are largely subjective (are they?) in the interest of the authors to get the nice temporal evolutions of Fig. 6. In addition to provide futher details, I suggest that the authors use Fig. 5 to illustrate graphically several issues of the method, like: (i) where, horizontally, is the front located? (ii) where is H defined and what is its vertical depth? (iii) for which surface parcel (i.e. horizontal position) is the LFC calculated?

Some subjectivity exits to the define H from the vertical cross sections. However, if the proposed method is used, small variations of H are found depending on the point where it is estimated.

Concerning to the evaluation of the LFC, it is calculated for the WRF output by using the RIP package (http://www.mmm.ucar.edu/mm5/documents/ripug_V4.html).

In the new figure 7 (old Fig. 5), H estimation is marked as well the point where LFC is calculated.

âĂć TECHNICAL CORRECTIONS

All the technical corrections and suggestions below have been modified. Below we specifically answer to some of them.

PAGE 1

- The title of the paper is too long and complex and the term "nocturnal" is repeated. My suggestion is: Numerical simulation of nocturnal heavy rainbands associated with coastal fronts in the Mediterranean basin.

We have modified the title.

PAGE 4

- L15-25: As described, the method used by the authors is not fully clear. First, it would seem TRMM was used to watch the whole history of images, then screening hundreds of potential coastal rainbands and ending with only three cases? Was this the case?

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The referee is right. Some new sentences have been added in the methodology in order to clearly explain the methodology used.

- Also, it is not clear if/why two different meteorological data analyses (NOAA-CR20 and NCEP reanalysis) were used. From the last paragraph of section 2 it would seem that more than three cases were simulated although only three are analysed in the paper. Please, clarify all these obscure methodological aspects.

We have clarified it.

- L25 and ALL the Section 3: Inappropriate use of verb tenses when describing the results, like in "this line of precipitation WAS quasistationary". The use of the past tense would be pertinent if true fields (i.e. observations) are being analysed. If these fields come from unverified simulations then we can only refer to what the numerical outputs show, not to the factual reality, and the present tense should be used. Of course, after reconciliation of the simulations with the observations (see my first major comment) general use of the past tense becomes suitable. Finally, do not mix verb tenses in a same sentence.

We have modified the text accordingly.

PAGE 10

TABLES AND FIGURES - Table 1: it is not clear in the text why are these fine-grid mesoscale simulations so long, as much as 90h. And also the fact that for ISR and GEN the results shown are for the second and third simulated day !!! whereas for IP they are from the first day, a more traditional approach. Please, clarifiy all these issues.

Simulations start at least 24 hours before the time in which rainbands are detected in TRMM and/or reflectivity and satellite images in order to analyze when the convection starts and whether it is associated or not to the nocturnal drainage flow.

By the similar reason, simulations finish at least 24 hours after the rainbands events, in order to analyze how rainbands extends during the morning, and the next night.

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The referee is right. It is not necessary so large simulation time as in the GEN and ISR cases and consequently for the IP simulation we decided to reduce the duration of this simulation. The difference between the duration of IP and the other simulations is only related to the time when the simulation was performed. GEN and ISR were among the earliest simulations performed. IP simulation was one of the last ones.

- Fig. 5: According to my last major comment, complete this figure with an indication for each case of the front position, vertical extent of H, and surface parcel used to calculate LFC (give the resulting values of H and LFC in caption). Also, indicate the areas with convective instability (i.e. decrease of THETAe with altitude) mentioned in the text, and add subdivisions to the vertical axis to help reading height values.

We have completed the figure accordingly.

- Fig. 6: Did you find any correlation between the triggering parameter and the 1 h rain rate in the simulations. For example, is this rain rate lower for IP given the lower values of the parameter for this case? Please, comment on this in the text.

It has been commented in the manuscript.

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