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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

Anonymous Referee #1

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GENERAL COMMENTS

In this short study the authors provide WRF-ARW numerical simulations of three coastal nocturnal rainbands that affected Israel, the Gulf of Genoa and the eastern Iberian Peninsula. The results emphasize the physical mechanism responsible for this type of precipitation system: the convergence between the onshore background synoptic flow and the nocturnal drainage winds from the continental lands. A conceptual model built on the ratio between the coastal front depth and the level of free convection of the maritime air is used to explain the convective character and high precipitation rates of the analysed episodes.

The subject of this numerical study is an interesting one. This kind of coastal phenomena appears to be quite common in many tropical and mid-latitude regions under weak synoptic forcings and abundant bibliography on the topic can be found, as confirmed by the second paragraph of the Introduction. New insights can be derived, however, from the experiments of this paper, even for a clear benefit to regional forecaters. The methodology is properly designed and the application of a simple conceptual model originally formulated for the description of topographically-induced rainfalls is useful and interesting.

In my view, however, a major deficit of the manuscript that impedes its publication under its current form is the lack of any observational data. These data become essential not only to better document the cases, but also to provide a general validation of the numerical simulations. In addition, although I am not a native English speaker it is evident the paper contains a large number of unclear or misleading statements, grammatical errors and technical problems that must be corrected. All these major and minor problems are listed below:

* 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

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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.

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.

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.

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:

- 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

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a single air mass is considered. Is this interpretation correct?

- 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 oversimplistic 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.

- Page 8 (L22-23) and Page 9 (L19-21): There are well established theories expaining 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.

- 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...

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

* TECHNICAL CORRECTIONS

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

- In ALL the paper: rain band/rain bands —> rainband/rainbands

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PAGE 2

- L4: by using —> with / to study —> with the purpose of studying

- L6: Large 1h and 10h accumulated precipitations are simulated in comparison with other similar ...

- L9: northeastern COAST of

- L10: moves offshore —> moves away from the coast

- L12: is INTERcompared

- L15: drainage windS and the prevailing synoptic flow are ...

- L16: IN the tropics

- L18: ...and moves towards the coastline along the riverbeds and slopes of the coastal mountains. This cold air mass ...

- L20: arrives over the sea —> reaches the sea

- L21: and moister maritime air

- L21 and ALL the paper: wetter —> moister / wet —> moist

- L22: if the maritime air reaches —> if it reaches

- L26: These —> The

PAGE 3

- L4: ... clouds without any or with weak precipitation...

- L5: than THE LCL but lower than THE LFC; if h is lower than THE LCL clear ...

- L7: ... idealized mountains, SUCH as the ...

- L9: small —> smaller

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- L10: at —> of

- L28: there is —> there are

PAGE 4

- L6: in south Israel

- L6-10: Some of the description of the Goldreich et al. (2004) study is redundant after the previous sentence on Heiblum et al. (2011). I recommend a better integration of both sentences.

- L11 and L19: by using —> with the

- L21: This paper aims to show and describe —> This paper presents and numerically investigates

- L21: Remove "high"

- L23: of the drainage cold inland air —> of the drained cold inland air

- L24: wetter air mass —> moister maritime air mass. Finish the sentence here and add a new one: "We seek to contribute to a better knowledge of this regional phenomenon".

- L27: to some others studied events in the Mediterranean basin have been... —> to other events of the Mediterranean basin studied by the same authors (REFS..., Mazon and Pino 2013b ?) have been...

[NOTE the importance of providing these comparative studies at this point, since repeatedly along the text (e.g. Abstract L6, Page 4 L10-12) the authors emphasize the high precipitation intensity of the selected cases compared to other events, but there is no evidence till the beginning of Section 3.1 on where/which/how are these other events and thus the statement could be interpreted as highly subjective or speculative]

- L28: by using —> using the

PAGE 5

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- L1: Remove both commas, i.e.: The second event (GEN) occurred in ... January 2008 and the third ...

- L11: Sect. 4 —> Section 4

- L18: Is "record" the proper term to describe the capability of TRMM? ... I would say a remote sensing platform like this can "estimate" or "derive" precipitation...

- 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? 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.

- L24 and L26: "polar front" does not appear to be the best term in this context as this is a too general dynamical feature of the atmospheric circulation. Terms like "baroclinic fronts" or "large-scale fronts" are better suited.

- L25: Change sentence to "Finally, at the third step, the events whose precipitation is not associated with this kind of meteorological disburbances, but it is more likely due to a coastal front, are simulated ...."

PAGE 6

- L1: has —> includes

- L8: lower —> higher (remember that the resolution increases with decreasing grid-length ...)

- L9: Change to "The initial and boundary conditions (the latter updated every 6 h) were obtained from the ECMWF reanalysis model."

- L11: Remove "in this paper" / "each event" —> "the events"

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- L24-25: "...is shown in this paper for the first time". Please smooth this sentence. It would seem that plotting and showing a simulated 10h precipitation field is considered a great scientific task by the authors...

- L25: As described IN Mazon and Pino...

- 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.

PAGE 7

- L4 and ALL the paper: line of convergence —> convergence line

- L5: by interacting —> by the interaction of / a similar synoptic —> the prevailing synoptic

- L7: Change to "...reaching a 10h accumulated maximum value of about 14 mm."

- L9: Incoherent verb tenses: use moved-progressed, or much better (see comment on Page6/L25), use moves-progresses.

- L10: precipitating cells —> precipitation cells

- L13: Remove "speed" since Fig. 3 shows the full vector field.

- L16: is —> as

- L18-19: moved-were or move-are (do not mix tenses)

- L20: as IT is shown

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- L26: with a —> and the

- L27: Remove "wind speed"

PAGE 8

- L1: Remove "with"

- L3: moves FURTHER offshore

- L5: turns —> veers / northwesterly —> northerly (note that the NW winds are found too far from the arc-shaped line)

- L6: a prevailing easterly flow exists —> an easterly flow prevails

- L7: at —> by / no MORE precipitation

- First sentence in Section 3.3: This seems to be a general statement, and then a reference is needed in support of it.

- L11: where a gradient of horizontal potential temperature exists —> where a large enough horizontal gradient of potential temperature is present.

- L13: (color contour) —> (color-filled contours) / (closed lines) —> (solid lines)

- L14: (arrows) —> (vectors)

- L16: According to Fig. 5 the depth of the front seems to be much less than 600-700 m (see also my last major comment)

- L19: suggesting that weak instability exists —> suggesting the presence of weak convective instability

- L20: can reach THE LCL and LFC

- L21: as IT is shown BY the liquid-water

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- L1: Change the end of the sentence to: ...2013a,b)attains values smaller than 1 during the whole night and these events are associated with weak precipitation.

- L4: Remove "near the coast"

- L5: inland air mass that reaches the coast with a prevailing warmer and wetter air mass —> inland air mass drained towards the coast and a warmer, moister maritime flow

- L6: Really ??? ... Where are the satellite observations ??? (see my first major comment)

- L6: observations and (REMOVE comma)

- L9: According TO the simulations (NOTE: "and observations", once they are added, of course).

- L9: formed —> developed

- L11: comparing —> compared / some —> a few

- L12: Due to similar —> Owing to the similar

- L14: occurred —> developed

- L15: Regardless OF the stationarity / amount —> amountS / Remove "accumulated"

- L16: factor —> factorS / associated with THE nocturnal

- L17: has a larger contribution in producing —> contribute to produce

- L20: movements —> circulations

PAGE 10

- L2: when THE rainbandS are

TABLES AND FIGURES

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- 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.

- Fig. 1: occurred on —> that affected the..., the..., and the... / squares —> rectangles / close square —> closeD rectangle

- Fig. 2: (color contour) —> (color-filled contours) / (arrows) —> (vectors) / estimated —> estimate

- Fig. 3 and Fig. 4: (color contour) —> (color-filled contours) / (arrows) —> (vectors)

- Fig. 5: (color contour) —> (color-filled contours) / (closed lines) —> (solid lines) / (arrows) —> (vectors)

- 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.

- 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.

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