Natural Hazards and Earth System Sciences
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Discussions

Interactive comment on “Sea surface temperature and torrential rains in the Valencia region: modelling the role of recharge areas” by F. Pastor et al.

Anonymous Referee #2

I agree, in general lines, in the description and valuation made by Referee #1, and also in many details. In particular, I think the paper contains potentially significant contribution, but it needs MAJOR changes before publishing. I do not know what the editor will decide, but I have supposed that the authors will be as responsible to the Referee #1 requirements as they announce they will be in a new version of the paper. In my understanding, even so some changes could not be enough and I even suggest a few new details.

As “Referee #1” says, “The present paper describes three events of heavy rain in Valencia region, considering simulations performed with RAMS. Also, some sensitivity experiments are performed by changing the SST in some specific regions along the parcel trajectory ending in the precipitation area. In this way, the Mediterranean sub-regions that could have affected more deeply the precipitation amount and distribution are identified.

This new strategy to perturb the SST field is able to determine the regions that may have played a key role in the development of the torrential rain and then to investigate just the effect of that specific area in the model results. This approach is very interesting and could be applied also to other region in the Mediterranean basin”.

However, major and minor aspects have to be reviewed before publishing. I the next, I will insist on some of these points and I also introduce some additional aspects, but I will not repeat all what Referee #1 said and the authors answered in a clearly satisfactory line.

Regarding MAJOR POINTS, I have to insist in questions regarding SST climatologies and SST initial values. About climatology, perhaps after carefully reading the first author’s thesis (Pastor, 2012) it becomes clearer, but I think the text of the present paper has to be clear enough by itself, in this sense. Which is the base for doing the clustering process? Which are the n-dimensional elements that are used, are they SST values in every grid point? Are they grid-point daily values, along a unique period, 1982-2009? Are they grid-point monthly average values along the unique period? Are the data seasonally stratified (by seasons, by months), before doing the clustering process (that is, is there a clustering process for every season or for every month or an only one process? In case of a unique clustering process with grid-point monthly average data, how to define winter/summer, seasonal or monthly cluster assignations? In Fig. 1, it is quite surprising the almost exact similarity (with regard to shape) between the seasonal SST isotherms and the “winter”, “summer”, “transition1” and “transition2” cluster limits.

About the SST initial field that is used in the control run, for every case, in the initial text it is no clear but it continuous not being very clear for me after the response of the authors to Referee #1. From the last figure that is included in this response, it seems that the initial SST values that have been used are the monthly average values of the corresponding actual month or of the month before (Oct-2007 for the Oct-2007 case, Oct-2000 for the Oct-2000 case and Aug-1989 for the Sep-1989 case). Why the precedent month in the 1989 case? Why do not use actual initial daily SST? Monthly averages can sensible differ from daily values. It seems that an ideal way to treat with air-sea exchange question is the use of actual sea data, even with changes along the integration, through variable boundary conditions or by using and air-sea exchange complementary model. Average monthly values seem to be poor data. The other point is how and why to assign “summer”, “winter” and “summer to winter transition” models of a clustering distribution in order to introduce SST changes in the sensitivity experiments.

A question also mentioned by Referee #1, about the back trajectories, is also a major point and I wish to insist on it. It is no clear if the back trajectories, both from NCEP analyses or from RAMS runs, are 2D or 3D trajectories. In the first case, these are not realistic trajectories: they are a conceptual simplification. Real trajectories are usually 3D, with significant changes in level along the trajectory in many cases. This means that 3D trajectories have to be considered, but even when considering low level final (arriving) level the initial (departing) level can be quite high and then a direct heat and water exchange with the sea is not possible. There is no problem in modifying SST in some areas (defined by a previous clustering or by another way), but to consider that the areas that have to be considered for it are the areas under a black trajectory can not have a robust foundation, at least for distant segments of back trajectories, running at a relatively high level. Of course, at short distances the 2D and 3D back trajectories can be vertically close each other and the problem vanishes. The effect of SST changes on heavy precipitation is then logically more important for marine areas closer to the heavy precipitation zone.

Measuring the effect of SST modification needed a comparison between the observation and the simulations, not only the control simulations. This point was also mentioned by Referee #1 and it has been positively responded by the authors trough the introduction of observed precipitation, for each case. I would suggest to also adding some numerical indicators. To be strict, the most convenient way to measure effect of changing in factor on the precipitation fields is though a complex way based on shape recognition (SAL or some others methods), but it could be enough to do and indication based on maximum and total precipitation in a delimited area.

With regard to possible forecasting interpretation of the results, a reduction of SST from values of around 20ºC to 10ºC can give a qualitative idea, but it is far from realistic changes. Perhaps some intermediate values could have to be used to analyse the impact of SST on close marine areas to heavy precipitation on land. But I understand this would be a too demanding change and that is very difficult for the authors to assume this utopian suggestion.

Going to some additional MINOR points, first (page 1359, lines 1-3), the idea of a particular Mediterranean meteorology as a consequence of a singular geography, characterised by a close and relatively isolated sea, surrounded by elevated terrain, is clearly prior to Millán et al. (2005).

In page 1359, lines 5 to 12, some kind of conceptual mixing seems to appear. Perhaps it is convenient to clarify that heavy/torrential rain and cyclone/cyclogenesis are independent concepts, although in many cases cyclones are acting factors in the organisation and onset of heavy rain (see, for instance, Jansa et al., 2001, Meteorol. Appl., 8, 43-56, Jansa et al., 2014, Nat. Hazards Earth Syst. Sci., 14, 1965–1984, and references in both). Possibly analogous confusion (or confused expression) appears before, in page 1359, by line 15 and surroundings. Note that the title of the first author's thesis (Pastor, 2012), also could indicate some kind of equivalence between two different concepts, heavy rain and intense cyclogenesis. A relationship (or simultaneity) can exist, but between different phenomena.

Page 1362, line 15, the SST monthly climatologies, were they used or where they developed by Pastor (2012)?

Page 1364, lines 15 and following. It seems not, but perhaps it is convenient to indicate that SST is not included within the initial and boundary conditions package.

In Fig. 3, RAMS simulated back trajectories are much longer than the analysed ones, why?

In Fig. 7, RAMS simulated back trajectories are not only longer, but also different to the analysed ones.