

Interactive comment on “Mediterranean depression characteristics related to precipitation occurrence in Crete, Greece” by V. Iordanidou et al.

Anonymous Referee #1

Received and published: 22 October 2014

General comments:

This manuscript analyses some characteristics of the cyclonic systems in the Eastern Mediterranean over a 30-year period (1979–2011), but focusing on those triggering precipitation in Crete (Greece). For this purpose, the MS cyclone tracking scheme is applied to the 6-hourly mean sea level pressure fields from ERA-Interim reanalysis. Precipitation amounts are keyed to three severity categories of precipitation (mild, strong and heavy). The topic of the manuscript is pertinent and suitable for NHESS. Although some conclusions can be drawn regarding the cyclone characteristics as a function of both seasonality and severity, which are generally supported by previous

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studies, the paper still presents some important weaknesses (outlined below). As such, from my viewpoint, this manuscript may be accepted for publication, but only after carrying out major revisions.

1. My major concern about this study is that it merely investigates statistical associations between cyclone parameters and precipitation categories/seasonality, not providing any dynamical analysis. The results are too descriptive, without an analysis or discussion of the underlying processes and mechanisms. For example, a spatial analysis of the cyclone tracks/densities would be particularly helpful, e.g. in explaining preferred directions/origins of the cyclone tracks. A synoptic overview and discussion of the different dynamical regimes/flow types could also significantly improve the manuscript, including its applicability to weather forecasting.
2. The potential applicability of the study in regional hydrological modelling or weather forecasting is barely discussed by the authors. In fact, the ‘Conclusions’ section is essentially a mere summary of the main results. This section needs a significant improvement to comprise a comprehensive discussion of the outcomes and of their implications/potential applicability. The novelty of the results should also be clearly demonstrated against previous studies.
3. Overall, the methodology and datasets are adequate. The MS (Melbourne University) tracking algorithm has been widely applied and successfully tested under different dynamical regimes and for different geographical sectors, including the Eastern Mediterranean. Nonetheless, a more in depth comparison to other existing tracking schemes should be undertaken in section 2. Furthermore, the traceability of the results might be difficult, as some parts of the methodology are not clearly presented (see specific comments below).
4. The readability of the paper is low. A careful editing is needed. For instance, many sentences are too long, without commas. Many others are also unclear (see specific comments below).

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5. The number of figures and panels is a bit large and their captions need a significant improvement.

Specific comments:

Title: Please replace 'depression' by 'cyclone'

Abstract: The abstract should be without paragraphs. Please use '0.5°' in the grid resolution here and throughout the other instances text. The entire abstract need editing and rephrasing so as to become clear and concise.

Section 1 (Introduction): 1st sentence: the outlined studies are only for the Mediterranean region. Please either include other references covering other regions worldwide or refer the 'Mediterranean region' in the statement.

Page 6109, Line 9: please replace 'effecting' by 'controlling' or 'governing'

Page 6109, Line 12: please replace 'researchers' by 'researches'

Page 6109, Line 17: the first time you mention the MS scheme, a reference to the paper of Murray and Simmonds should be included.

Page 6110, Line 27: please be more precise about the results of Catto et al. (2012)

Page 6111, 1st sentence: confusing sentence. Please rephrase.

Page 6111, Line 6: "sense of safety"

Page 6111, Line 16: "location" instead of 'local'

Page 6111, Line 19: you have not investigated "atmospheric circulation patterns". Only statistical associations between cyclonic characteristics and precipitation events are assessed.

Please rephrase for accuracy. Please also replace "rainfall" by "precipitation".

Page 6112, Line 4-12: the method for identifying the precipitation categories should be

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better explained. Is it applied for each weather station separately? Can we then have different severity categories in the same day? How you estimate the percentiles? Empirically or by adjusting a theoretical distribution? Please also discuss the robustness and sensitivity of your results for precipitation values above the 99.5 percentile.

Page 6112, equation 1: please remove the comma within the mathematical expression.

Page 6113, Line 17: "...station can convey..."

Page 6114, Lines 8-10: I am a bit puzzled about this sentence, since you consider a minimum cyclone lifetime of 24 h. Can you have more than one cyclone a day?

Page 6114, Line 26: please refer to Fig. 2 for the geographical location of the weather stations.

Page 6115, Lines 28-29: please remove this sentence.

Page 6116, 2nd paragraph: The two referred flood events are not integrated in the analysis. Although this kind of synoptic analysis might be useful, also in agreement with my major comment 1 above, as it is currently, this section is very vague and seems to be an add-on, without a clear connection to the following analyses.

Page 6116, Line 23: please revise the citation format here and in other instances throughout the manuscript (e.g. page 6117, Line 16).

Page 6117, Line 7: Please move this second part of the paragraph "Most of the..." to its beginning.

Page 6117, Line 6: Please explain the meaning of "...in sufficient close distance from...". The method of the Euclidean distances is not clearly explained neither here nor in the methods section.

Page 6117, Line 17: "30°N"

Page 6117, Line 19: up to 80% for strong or heavy rain?

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Page 6117, Lines 20-23: the occurrence of these precipitation events need to be better discussed. Maybe you can add here some references to studies dealing with orographic effects and meso-to-micro climatic processes in Crete.

Page 6118, Lines 14-16 and Lines 20-22: there is a repetition.

Page 6119, section 4.3: an adjustment of theoretical probability distributions or a kernel smoothing would be useful to assess the normality or non-normality of the distributions.

Page 6120, Lines 4-5: the propagation velocities from your study and from Flocas et al (2010) are indeed different. Can you explain that?

Page 6120, section 4.4: this section is very descriptive and boring for the reader. Further, in many statements there are no references to figures and to their corresponding panels. Please also consider a different arrangement of the figures/panels, as their readability is not straightforward.

Page 6122, Line 3: "lowest"

Page 6122, Line 10: replace "relative" by "similar"

Tables 3 & 4: maybe you can use p-levels instead of "0" and "1".

Fig. 3: caption very incomplete.

Figs. 4 and 5: are you sure that you have relative frequencies in all charts? The horizontal bars are not in %... This leads me to another question about the interpretation of the results taking into account that the areas of each sector are very different. To what extent are the differences found between sectors area-weighted?

Please improve all figure captions.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 6107, 2014.